The Making of Dendroclimatological Knowledge

A Symmetrical Account of Trust and Scepticism in Science

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Declaration

Date: 11 September 2015.

I certify that the work contained within has been composed by me and is entirely my own work. No part of this thesis has been submitted for any other degree or professional qualification.

Small parts of Chapters 1 and 3 have been published in Ramírez-i-Ollé, Meritxell "The Social Life of Climate Science", *Method Quarterly*, Issue 2, accessed 20 July 2015. <http://www.methodquarterly.com/2015/02/the-social-life-of-climate-science/

This thesis has been proofread by a professional editor who has made no contribution to the intellectual content of the thesis or been involved in rewriting text.

For Ivan and my family.

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Abstract

This thesis presents an empirical study of dendroclimatology, with the purpose of contributing to a wider understanding of the way scientists generate knowledge about climate change. Dendroclimatology is a science that produces knowledge about past climates from the analysis of tree growth.

For two years, I have studied the work of a group of dendroclimatologists, joining them on fieldwork and sampling expeditions in the Scottish Highlands, observing how they generate data from tree samples to reconstruct past temperatures in Scotland and examining how they have mobilised a Scottish temperature reconstruction in a scientific debate over historical changes in climate. This thesis develops two parallel narratives about the practice of making dendroclimatological knowledge and the roles of trust and scepticism in this process. In describing how dendroclimatologists work to extract information about past climates from trees, I identify the importance of trust relationships and scepticism at each stage of their work.

I conduct a symmetrical analysis of both trust and scepticism in science. In the past, scholars studying science have emphasised the critical role of either trust or scepticism in the construction of scientific knowledge, and have paid relatively little attention to examining the relationship between the two.

In my study, I demonstrate that scepticism is part of the ordinary practice of dendroclimatology, and that scepticism in normal science (which I call “civil scepticism”) is fundamentally dependent (or “parasitic”) on existing trust relationships established through a variety of means. Dendroclimatologists engage in intimate interactions and mutual scrutiny of each other’s competence throughout the work they do in the field and in the laboratory, and they build upon and expand these trust relationships to create and defend climate reconstructions. I show that dendroclimatologists sustain trust relationships in part by demonstrating that they are competent sceptics (which I call “sceptical display”) and, in part by provisionally suspending their scepticism to permit agreement on what constitutes valid dendroclimatological knowledge.

I also analyse how these internal practices of scepticism and agreement are influenced by sceptical challenges from actors external to the dendroclimatology community, including challenges grounded in similar trust relationships (a further instance of civil scepticism) and challenges that are not (which I call “uncivil scepticism”).

I conclude that dendroclimatological knowledge is only possible as a result of contingent social negotiations over the distribution of trust and the boundaries of a trusting community.

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All individual scientific work is a collective achievement. I have been able to carry out this research with the support and tolerance of many people.

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1 Introduction

1.1. My Motivation

I start drafting this introduction in late September 2014 as hundreds of thousands of people are demonstrating simultaneously in different cities around the world - including Edinburgh where I live – as part of the first global climate march in history. In their manifesto, demonstrators address the heads of state gathering in New York to negotiate policies that would reduce the global emission of greenhouse gases: "Our collective demand is for ACTION, NOT WORDS. We want a world safe from the ravages of climate change".[1] These activists share the conviction that climate change is happening and is destructive. They are not alone in believing this. Different religious leaders also hold the belief that global warming is jeopardising the Earth.[2] In June 2015, Pope Francis published a 192-page encyclical (the Pope's most important teaching document, which he sends to all bishops of the Roman Catholic Church) that called for action to protect "God's creation" and to fight global warming.[3] Two months later, a group of mufti's (the Muslim legal expert empowered to give rulings on religious matters) circulated the "Islamic Declaration on Climate Change" where they affirmed that "God created the Earth in perfect equilibrium" and "The present climate change catastrophe is a result of the human disruption of this balance". [4] Climate change is also a concern for many secular citizens worried about the "carbon footprint" associated with their day-to-day activities such as travel and food. In turn, entrepreneurs are spotting new business opportunities in our fears of climate change and are developing "greener" products and markets. In many countries, the state uses taxpayers' money to create public infrastructures such as

[1] People's Climate Mobilisation. http://peoplesclimate.org/global/ (Accessed 15 May 2015).

[2] This situation supports the thesis of the sociologist Thomas Luckmann articulated in *The Invisible Religion*: *The Problem of Religion in Modern Society* (London: Macmillan, 1967) which, in many Western countries, traditional religions such as Catholicism and Islam coexist with "invisible religions" such as environmentalism that give moral meaning to the lives of many secular people. Luckmann's thesis triggered my interest as an undergraduate student in the sociology of science about climate change. I thank my undergraduate mentors Professors Joan Estruch and Salvador Cardús for introducing me to Luckmann's work, including his book with Peter L. Berger *The Social Construction of Reality: A Treatise in the Sociology of Knowledge* (London: Penguin Books, 1967).

[3] BBC "Pope to urge swift action on global warming." 16 June. 2015 http://www.bbc.com/news/world-europe-33144573 (Accessed 16th June 2015).

[4] 2015 International Islamic Climate Change Symposium - Islamic Relief Worldwide, "Islamic Declaration on Global Climate Change**"**, http://islamicclimatedeclaration.org/islamic-declaration-on-global-climate-change/ (Accessed 5 September 2015).

bicycle lanes, educational programs and carbon markets[5] to encourage (or "nudge"[6]) citizens and firms to be more "environmentally-friendly". These examples of collective action illustrate the widespread presence of social concerns about the risks posed by climate change.

Why do many people believe that the Earth's temperature has increased over time and that this climatic change puts our existence in risk? Most of us know about global warming and its effects (more frequent wildfires, increasing sea level rise, and longer periods of drought) because we have been told about them by scientists. Science provides us with "lenses" to explain the natural phenomena we observe around us. The sociologist Ulrich Beck suggests that environmental hazards "require the 'sensory organs' of science – theories, experiments, measuring instruments – in order to become visible or interpretable as hazards at all".[7] Since the late 1980s, an institution known as the "Intergovernmental Panel on Climate Change" (IPCC) has been explaining climate change to us on behalf of scientists.[8] Every four or five years, the IPCC produces scientific reports that contain a "Summary for Policymakers", which is intended to aid policy-makers in legislating against global warming.

Because our knowledge and actions regarding climate change are mostly mediated and justified by science, many people demand - and feel capable of[9] - to scrutinising how scientists

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[5] For a sociological account of the European Union Emissions Trading Scheme see Donald MacKenzie, "Making Things the Same: Gases, Emission Rights and the Politics of Carbon Markets." *Accounting, Organizations and Society*, 2009, Vol. 34, (3), pp. 440-455

6 "Nudge" has become an influencial policy concept created by Richard Thaler and Cass R Sunstein in *Nudge: Improving Decisions about Health, Wealth, and Happiness* (New Haven; London: Yale University Press, 2008). For a critique of nudge theory and other theories of social change related to climate change policy read Elizabeth Shove, "Beyond the ABC: climate change policy and theories of social change" , *Environment and Planning A*, 2010, Vol. 42, (6), pp.1273-1285 .

[7] Ulrich, Beck. *Risk Society: Towards a New Modernity* (London: Sage, 1992), 27.

[8] Mike Hulme and Martin Mahoney, "Climate Change: What Do We Know About the IPCC?," *Progress in Physical Geography*, 2010, Vol.34 (5), pp. 705-718. The historian Paul Edwards and the climate scientist Stephen Schneider describe the scientific and diplomatic negotiations involved in producing the 2nd IPCC report on "Self-Governance and Peer Review in Science-for Policy" in Clark Miller and Paul N. Edwards, eds., *Changing the Atmosphere: Expert Knowledge and Environmental Governance* (Cambridge: MIT Press, 2001). To understand the types of knowledge involved (and excluded) in the IPCC reports read Steven Yearley "Sociology and Climate Change after Kyoto", *Current Sociology*, 2009, Vol.57 (3), pp. 389-405.

[9] Harry Collins uses the term "default expertise" to describe the citizen's empowerment in *Are We All Scientific Experts Now?* (Cambridge, UK: Polity, 2014), p.15. Barry Barnes argues that the growing crisis of scientific expertise and the rise in the "culture of suspicion" is part of a larger secular trend caused by the extension of formal education, better informed citizenry and more accessible and content-rich mass media in "The Credibility of Scientific Expertise in a Culture of Suspicion", *Interdisciplinary Science Reviews*, 2005, Vol. 30, (1), pp. 11-18. Steven Yearley also offers an explanation of the crisis of scientific authority related

know what they know. Over the last few years, very active lay people or "scientific citizens"[10] have been using blogs and other online platforms to "audit" climate science. [11] Other individuals have created institutions and think-tanks that examine the "integrity" of climate science with the stated purpose of influencing and disputing policy debates about climate change.[12]

The recent "Climategate" episode is an example of the increased public interest in and public scepticism of the way scientists generate knowledge about climate change. Climategate occurred in November 2009, two months after I started my postgraduate studies at The University of Edinburgh, and it caught my attention immediately. In brief, the term Climategate refers to the online publication of the emails of a few climate scientists from the United Kingdom and the United States by an anonymous hacker whose stated motivation was "to give some insight into the science and the people behind it",[13] and the reaction of many commentators - including scientists - who interpreted the stolen emails as an embarrassment and evidence of scientific fraud (hence the suffix *-gate* as a reference to the "Watergate scandal").[14] The Climategate emails undermined public trust in science, the reason being that they revealed practices among climate scientists that many among the public thought were contrary to good science.[15]

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to the internal workings of science and the status of scientific institutions in contemporary society in "The Changing Authority of Science", *Science Studies*, 1997, Vol. 10, pp. 65-75.

[10] Alan Irwin uses the term "Scientific Citizen" to refer to the particular constructions of the members of the public in governance, policy and decision processes in "Constructing the Scientific Citizen", *Public Understanding of Science*, 2001, Vol.10 (1), pp.1-18.

[11] The blog "Climate Audit" run by Steven McIntyre is one of the most famous blogs in the "climate sceptical blogosphere" according to Amelia Sharman, "Mapping the Climate Sceptical Blogosphere.", *Global Environmental Change*, 2014, Vol.26, pp.159-170.

[12] In April 2015, the "Global Warming Foundation" in the UK launched an inquiry "into the integrity of the official global surface temperature records". In the US, the "American Enterprise Institute", "Cato Institute" and the "Heartland Institute" produce similar reports and organise yearly conferences on climate change. For a theoretical treatise on the reasons for the existence of social disagreement about the reality and risks posed by climate change read Mike Hulme, *Why We Disagree about Climate Change: Understanding Controversy, Inaction and Opportunity* (Cambridge : Cambridge University Press, 2009).

[13] Fred Pearce, *The Climate Files: The Battle for the Truth About Global Warming* (London: Guardian Books/Random House, 2010), p.166.

[14] For an analysis of the scientists' response to the allegations of scientific fraud read Meritxell Ramírez-i-Ollé, "Rhetorical Strategies for Scientific Authority: a Boundary-Work analysis of 'Climategate'", *Science as Culture*, 2015, pp.1-28.

[15] The meaning and interpretation of the content of the Climategate emails is not straightforward. Fred Pearce's book *The Climate Files* offers a good journalistic account of the background of this controversy. Two sociological accounts of Climategate that I find particularly succesful are: Marianne Ryghaug and Tomas Moe Skjølsvold, "The Global Warming of Climate Science: Climategate and the Construction of Scientific Facts", *International Studies in the Philosophy of Science*, 2010, Vol. 24, p.287-307; and Martin

Climategate is the background of this thesis insofar as it offered a glimpse of the mechanisms of *internal* credibility of a *publicly* disputed science. These two dimensions of scientific credibility seem to be related: for many people, what they read in the stolen emails about the way climate scientists secure credibility among themselves was a reason for granting less credibility to their claims. In this thesis I do not address the very important issue of *public* trust and why many people - including scientists - regarded the Climategate emails as evidence that the climate scientists were acting unscientifically. Instead, I study the phenomenon of *private* intra-group trust in one of the climate sciences - dendroclimatology - that was criticised during Climategate, and the way a group of climate scientists come to trust their work.

My motivation is in contributing to a wider understanding of the way scientists generate knowledge of climate change by presenting an empirical study of dendroclimatology. Dendroclimatology is the science that generates knowledge about past climates from the study of tree growth. To resolve the question of whether current climates are anomalously warm, scientists compare them with the climates of the past. In most countries, systematic and reliable records of temperature and precipitation exist only from the late 19th century onwards. For the period before the existence of meteorological records, scientists use trees as a source of information of past climates.

To clarify, this thesis is *about* dendroclimatology, as opposed to *in* dendroclimatology. I have neither the interest nor the competence to generate knowledge from trees. Instead, I aim to explain how dendroclimatologists make scientific knowledge. How do dendroclimatologists know what they know? For decades, the academic subfield of the Sociology of Scientific Knowledge (SSK)[16] and the broader interdisciplinary research area of Science and Technology Studies (STS)[17] have been asking similar questions with regards to other fields of science and technology. Scholars in these fields examine how the content and design of science and technology is related to the social dynamics within and outside scientific and technical communities. As I detail in the next section, this thesis focuses on trust and scepticism as examples of social processes involved in the

Skrydstrup, “Tricked or Troubled Natures? How to Make Sense of ‘climategate’”, 2013, *Environmental Science & Policy*, Vol. 28, pp. 92–99.

[16] For a review of the origins and development of SSK read Shapin, Steven. "Here and Everywhere: Sociology of Scientific Knowledge”, *Annual Review of Sociology*, 1995, Vol. 211(1), p.289-321.

[17] For an overview of the main topics in STS: Edward J. Hackett et al., *The Handbook of Science and Technology Studies***,** Cambridge, Mass.: MIT Press: Published in cooperation with the Society for the Social Studies of Science, 2008); Sheila Jasanoff et al; *The Handbook of Science and Technology Studies, (*Thousand Oaks, Calif.; London: Sage Publications, 1995).

making of dendroclimatology. I draw upon certain authors and traditions within SSK and STS - particularly those associated with the "Edinburgh School"[18] - to address how dendroclimatologists produce knowledge about climate change.

1.2. My Aim

In this thesis I aim to provide a symmetrical account of the roles of trust and scepticism in the making of dendroclimatological knowledge. By a "symmetrical account" I mean a description that shows the different constitutive roles of *both* scepticism and trust, as social activities, in the production of scientific knowledge.[19] My hope is that, after finishing reading this thesis, the reader will be convinced that I have provided sufficient empirical evidence to argue that we can only

18 Like all labels, the "Edinburgh School" one is problematic. One way to define who is a member of the Edinburgh School is to consider the people who were involved in setting up and running the Science Studies Unit at the University of Edinburgh in the 1960s. One difficulty with using this criterion is that these people might not use this label to identify themselves as part of a distinctive group, as can be seen in this interview with Barry Barnes (Ruey-Chyi Hwang et al., "Dropping the Brand of Edinburgh School", *East Asian Science, Technology and Society*, 2010, Vol. 4 (4), p. 601). Also, another problem with using this label is the assumption that the ideas of the "founders" have not changed over time. The best way to ascertain who is part of the Edinburgh School is to ask people like me who use this term. I use the term Edinburgh School to refer to the first researchers and teachers in the Science Studies Unit (David Edge, David Bloor, Barry Barnes and Steven Shapin) that developed the "Strong Programme" in the sociology of knowledge (David Bloor, *Knowledge and Social Imagery*, (Chicago; London: University of Chicago Press; [1976] 1991) and to identify the people who *explicitly* seek to build upon the ideas developed by the founders of the Science Studies Unit.

19 The idea of "symmetry" has a long foundational role in SSK and STS, and my choice of the word "symmetrical" partly seeks to acknowledge my thesis within this tradition. David Bloor first pronounced the precept of "methodological symmetry" that gave rise to SSK. Subsequent reinterpretations and applications of the term "symmetry" have effectively constituted very productive approaches within STS. Wiebe Bijker and Trevor Pinch applied their understanding of symmetry to technology and created "the Social Construction of Technology approach" in "The Social Construction of Facts and Artefacts: or How the Sociology of Science and the Sociology of Technology might Benefit Each Other", *Social Studies of Science*, 1984, Vol. 14 (3), pp. 399-441. Michel Callon sought to expand Bloor's symmetry principle to objects and "non-human actors", giving rise to "Actor Network Theory", in "Some Elements of a Sociology of Translation: Domestication of the Scallops and the Fishermen of St Brieuc Bay." in John Law (ed.), *Power, Action and Belief: A New Sociology of Knowledge* (London: Routledge and Kegan Paul, 1986), pp. 196–233, and Michel Callon, "The Sociology of An Actor-Network" in M. Callon, J. Law, J. and A. Rip (eds.), *Mapping the Dynamics of Science and Technology* (London: Macmillan, 1986), pp. 19–34. My aim to produce a "symmetrical account" of trust and scepticism does not seek to emulate any of the variants of symmetry I outlined above, including Bloor's. In this thesis I do not set out to *explain* trust and scepticism but rather to describe the different asymmetrical roles that they play.

understand how dendroclimatologists produce knowledge of past climates if we consider the two variables (trust and scepticism) together.

Whilst I insist on including both trust and scepticism in the analysis of the production of scientific knowledge I do not treat these two social activities identically or as mutually exclusive. I do not regard trust and scepticism as being in opposition or having an equal role in science. Instead, as I develop in more detail in the next section, I see trust as prior to scepticism, trust being the *primary* element upon which scepticism is dependent (or "parasitic") for contributing to the creation of truth and knowledge. Rather paradoxically, my original aim of studying symmetrically the roles of *both* trust and scepticism in making scientific knowledge has led me to conclude that they actually play unequal, asymmetric roles.

As I argue in the remaining part of this section, my symmetrical account is different from those of previous scholars studying science. Philosophers, sociologists and historians of science have emphasised the critical role of *either* trust *or* scepticism in the construction of scientific knowledge, and have paid relatively little attention to examining the relationship between the two. In this thesis, I set out to show the complementary roles of trust and scepticism in scientific knowledge production.

Since the 17th century, mainstream Western philosophy has argued that individual scepticism, critical reasoning and systematic doubt are fundamental to the production of knowledge about nature. In his first book, *Meditations*, published in 1641, René Descartes presented the "Method of Doubt" or the "Cartesian doubt", which consists of the adoption of scepticism as a starting point for knowing. Descartes described the origins of this epistemology by reflecting on his discovery that many of the received opinions he had held during his life turned out to be false. Descartes said "From that time I was convinced of the necessity of undertaking once in my life to rid myself of all the opinions I had adopted, and of commencing anew the work of building from the foundation, if I desired to establish a firm and abiding superstructure in the sciences". Similarly, as part of his "critical philosophy", Immanuel Kant argued that a critique of individual reason by reason itself, liberated from the opinion of traditional authorities, is the unique source of knowledge.[20] In *The Critique of Pure Reason* first published in 1781, Kant wrote "Reason must in all its undertakings subject itself to criticism; should it limit freedom of criticism by any

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[20] Michael Rohlf, "Immanuel Kant", *The Stanford Encyclopedia of Philosophy (Summer 2014 Edition)*, Edward N. Zalta (ed.), accessed 14 August 2015 <http://plato.stanford.edu/archives/sum2014/entries/kant/>..

prohibitions, it must harm itself, drawing upon itself a damaging suspicion".[21] In the 20th century, the philosopher Karl Popper put forward the notion of "falsifiability", the ability of theories to survive critical tests and to be proven false, as the criterion for true knowledge. In his book *Conjectures and Refutations: the Growth of Scientific Knowledge* published in 1963, Popper presented his philosophy of "Critical Rationalism" as, "The proper answer to my question 'How can we hope to detect and eliminate error?' is, I believe, 'By *criticizing* the theories or guesses of others and--if we can train ourselves to do so--by *criticizing* our own theories or guesses."[22]

The sociology of science, pioneered in the mid-20th century by Robert K Merton emerged as a response to individualistic visions of science, partly formulated by philosophers.[23] As a sociologist, Merton's interest was in scepticism as a practice shared or "organised" by a community of scientists rather than as a psychological attribute. Merton formulated "organised scepticism" as one of the four social norms defining the practice of science - the other norms being "universalism", "communism" and "disinterestedness". Merton argued that the existence of these four scientific norms could be inferred from the scientists' texts. [24] He defined organised scepticism as "the temporary suspension of judgement until 'the facts are at hand' and the detached scrutiny of beliefs in terms of empirical and logical criteria".[25]

Unfortunately, neither Merton nor any of his followers offered examples of the way organised scepticism as a social norm is constituted and functions *in practice*. Instead, Merton hypothesised, rather abstractly, that the individual scientist might feel ambivalent[26] about reconciling the social demands of being sceptical and being dogmatic and trustful towards one's

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[21] Kant, Immanuel, *Critique of Pure Reason* (Cambridge: Cambridge University Press, [1781], 1998), p.463.

[22] Karl Popper,*Conjectures and Refutations: the Growth of Scientific Knowledge*, (London ; New York : Routledge and Kegan Paul, [1963], 2002), p.34. Original emphasis.

[23] Merton's explicit goal was to establish sociology more generally and the field of the sociology of science in particular, as legitimate academic disciplines and careers. Robert Merton, "The Sociology of Science: An Episodic Memoir", pp. 3–14 in R.K. Merton and J. Gaston (eds) *The Sociology of Science in Europe*. (Carbondale, IL: Southern Illinois University Press, 1978).

[24] Merton writes, "Although the ethos of science has not been codified, it can be inferred from the moral consensus of scientists as expressed in use and wont, in countless writings on the scientific spirit and in moral indignation directed toward contraventions of the ethos". *Idem*, pp. 268-269. The sociologist Michael Mulkay argued that what Merton identified as "scientific norms" should be considered as "professed" norms or ideological self-descriptions employed by scientists rather than observable behaviour in "Norms and Ideology in Science", *Social Science Information*, 1976, 15, pp. 637–656.

[25] Robert Merton, "The Normative Structure of Science." *The Sociology of Science: Theoretical and Empirical Investigations*,(Chicago ; London : University of Chicago Press, 1973), p.277.

[26] Robert Merton, "The Ambivalence of Scientists." *Bulletin of the Johns Hopkins Hospital*, 1963, 112, pp. 77.

results and tradition of knowledge. [27] Merton expected that the individual scientist would resolve this "functional tension" by distinguishing between scepticism as a mental state and as an observable behaviour. He wrote that "The institution of science does not require scientists to *feel* detached and sceptical of their own ideas; it only requires them to *act* with detachment, at least to a degree sufficient to anticipate so far as they can the criticisms that will be levelled against their work by competent peers".[28] Merton identified the existence of a reward and monitoring system in science as the main reason why scientists feel compelled to behave sceptically and pointed out that "the institution of science has managed to *institutionalise self-criticism* (beyond the level found in other institutional domains)".[29]

The origins of SSK and social constructivist accounts of science lie in the critique of empiricism, rationalism and Mertonian functionalism. Social constructivists from different generations and schools are united in the view that scientific knowledge is a common good that results from the collective action of individuals trusting each other. Michael Polanyi, who is considered an important precursor to social constructivist ideas of science, placed trust at the core of his "Fiduciary Programme".[30] In his book *Personal Knowledge: Towards a Post-Critical Philosophy* published in 1958, Polanyi presented the Fiduciary Programme as an alternative to the philosophers' account that placed (individual) scepticism at the centre of their philosophies of science.

Michael Polanyi used the concept of the "fiduciary framework" to argue that scepticism occurs against an overwhelming background of tacit and conventional beliefs. Polanyi explained, "Tacit assent and intellectual passions, the sharing of an idiom and of a cultural heritage, affiliation to a like-minded community: such are the impulses which shape our vision of the nature of things on which we rely for our mastery of things. No intelligence, however critical or original, can operate outside such a *fiduciary framework*".[31] Polanyi's understanding of a fiduciary framework is multiple and heterogeneous; it allows for there being an affective dimension as well as an

[27] Ian Mitroff presented "organised dogmatism" as a counter-norm to "organised scepticism" in "Norms and Counter-Norms in a Select Group of the Apollo Moon Scientists: A Case Study of the Ambivalence of Scientists." *American Sociological Review*, 1974, Vol.39 (4), pp.579-595.
[28] Robert Merton, "Postscript: The Ambivalence of Scientists." in Merton, Robert K. *Sociological Ambivalence and Other Essays* (London: Simon and Schuster, 1976), p. 62.
[29] Merton, *Idem*, p.62.
[30] Michael Polanyi, *Personal Knowledge: Towards a Post-Critical Philosophy* (London: Routledge and K. Paul, 1962 [1958]), p. 278.
[31] Polanyi, *Idem,* 280-281. My emphasis.

intellectual one in the production of knowledge as his use of the phrase "intellectual passions" and the idea of "affiliation" seems to suggest.

Polanyi's most famous concept of "tacit knowledge" is at the core of his concept of fiduciary framework.[32] Polanyi argued that the "tacit assent" and the knowledge that individuals produce through practice and socialisation in a "like-minded community" and a fiduciary community of trusted peers is the basis of critical thinking and scepticism. As Polanyi also made clear in another text, his concept of "tacit knowledge" is not opposed to "explicit knowledge". Instead, Polanyi's claim is that "all knowledge is *either tacit or rooted in tacit knowledge. A wholly* explicit knowledge is unthinkable"[33]. Therefore, for Polanyi, the "tacit dimension" of the fiduciary framework includes knowledge that individuals can and cannot tell others.

In his critique of Western philosophies of doubt, Polanyi criticised the possibility of universal doubt by illustrating the stability of the fiduciary framework. Polanyi cited the work of the anthropologist Edward Evan Evans-Pritchard on the Azande as an illustration of the limits of scepticism. Polanyi reported how Evans-Pritchard discovered that the Azande tribe in Central Africa had upheld their beliefs about the powers of poison-oracle against the refutations of witchcraft put forward by European colonialists.[34] Polanyi drew on multiple examples from the history of science to draw an analogy with the Azande. According to Polanyi, scientists, like the Azande, employ defence mechanisms and make *ad hoc* adjustments to protect their theories from being totally discredited. Polanyi concluded "Thus the programme of comprehensive doubt collapses and reveals by its failure the fiduciary rootedness of all rationality".[35]

Michael Polanyi's ideas on trust were later adopted by the historian of science Thomas Kuhn and members of the different sociological schools of scientific knowledge emerging in Britain in the 1970s. In the first footnote of his book *The Structure of Scientific Revolutions*, Kuhn acknowledged that Polanyi had "brilliantly developed" an idea ("tacit knowledge") similar to

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[32] The most common example given of "tacit knowledge" is bicycle riding or balancing with the bike, an ability that cannot be learnt and passed on through a set of explicit instructions but rather by practising often with others. Harry Collins offers a refined interpretation of this example as a case of "somatic" tacit knowledge, which could be written and executed by machines, as opposed to "collective" tacit knowledge, which Collins exemplifies with the case of car driving that includes an understanding of social conventions of traffic management and personal interaction that can only be learnt by socialisation. See Harry Collins, *Tacit and Explicit Knowledge* (Chicago/London: University of Chicago Press, 2010), p. 121.

[33] Michael Polanyi, "The Logic of Tacit Inference", *Philosophy*, 1966, Vol.41(155), p.7.

[34] Edward Evan Evans-Pritchard, *Witchcraft, Magic, and Oracles among the Azande,* (Oxford: Clarendon Press, 1937).

[35] Polanyi, *Personal Knowledge, p.*313.

Kuhn's notion of a "scientific paradigm".[36] With the concept of "scientific paradigm", Kuhn disputed the philosophers' arguments - particularly Karl Popper's - which science develops through critical tests and systematic scepticism. Instead, Kuhn argued, science is fundamentally produced through patterns of conventional activity (or "normal science" as Kuhn calls it) based on custom, acceptance of authority and trust. Kuhn responded directly to Popper by stating that "to turn Sir Karl's view on its head, it is precisely the abandonment of critical discourse that marks the transition to a science. Once a field has made that transition, critical discourse recurs only at moments of crisis when the bases of the field are again in jeopardy". [37]

In turn, Barry Barnes - the member of the Edinburgh School who has drawn most extensively on Thomas Kuhn to articulate the emerging field of the sociology of scientific knowledge[38] - used Kuhn's concept of "paradigm" to critique the Mertonian notion of organised scepticism. In one article co-authored with R.G. Dolby, Barnes criticised Merton for not providing empirical evidence of the existence of the four social norms of science. Regarding the norm of organised scepticism, Barnes argued that the Kuhnian concept of scientific paradigm is more useful for understanding the relative nature of scepticism - that is, the fact that scientists have a selective viewpoint which makes them sceptical of some results whilst trustful of others. Barnes and Dolby argue, "With the paradigm notion, too, one is able to delineate a *pattern* of scepticism— where scepticism increases as material conforms less to the expectations provided by a paradigm. Whatever one's final assessment of Kuhn's views may be, it is clear that his diagnosis succeeds in a way that Merton's does not, because it embraces the specific activities, theories and concepts of the scientist".[39]

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[36] Kuhn writes, "Michael Polanyi has brilliantly developed a very similar theme, arguing that much of the scientist's success depends upon "tacit knowledge," i.e., upon knowledge that is acquired through practice and that cannot be articulated explicitly. See his Personal Knowledge (Chicago, 1958), particularly chaps, v and vi" in *The Structure of Scientific Revolutions*, (Chicago: University of Chicago Press, 1962), p. 44.

[37] Thomas Kuhn, "Logic of Discovery or Psychology of Research?" in Imre Lakatos and Alan Musgrave, *Criticism and the Growth of Knowledge: Proceedings of the International Colloquium in the Philosophy of Science*. Imre Lakatos & Alan Musgrave, (Cambridge: University Press, 1970), p.6. For an overview of the debate between Thomas Kuhn and Karl Popper read David Bloor, "Two Paradigms for Scientific Knowledge"?, *Science Studies*, 1971, Vol.1(1), pp.101-115.

[38] Barry Barnes, *T.S.Kuhn and Social Science,* (London: Macmillan, 1982).

[39] Barry Barnes and R. G. Dolby, "The Scientific Ethos: A deviant viewpoint", *European Journal of Sociology*, 1970, Vol.11(1), p.11.

The sociologist Harry Collins – who in the 1970s pioneered another approach in the SSK[40] - has consistently worked on Polanyi's notion of "tacit knowledge" to explain the difficulties that scientists face in practicing scepticism exemplified by cases of experimental replications. As part of his long-term study of the efforts of an international group of physicists to detect gravitational waves, [41] Collins discovered that these physicists were able to replicate each other's experiments successfully only after they worked physically together and developed tacit knowledge about the apparatus used. Collins emphasised that laboratory visits and exchanges served the function of developing trust relations among these physicists. On the basis of seeing the experiment competently performed in the laboratory by trusted colleagues, Collins' physicists deemed it worthwhile to keep replicating it after failed attempts. Collins concludes, "Thus, though successful repetition of a result leads to trust, more importantly for the confirmation and spread of new techniques, *trust leads to successful repetition*". [42]

Over the last four decades, SSK scholars have sought to establish the legitimacy of the study of the social dimensions of scientific knowledge via the development of empirical studies that show how inductive and deductive sciences are dependent upon communal authority and trust relations between scientists. David Bloor in particular, draws extensively on the philosophy of Ludwig Wittgenstein to argue that all knowledge, including deductive mathematical knowledge, is a social convention.[43] In his sociological reading of Wittgenstein's book *On Certainty*, Bloor emphasises the importance of communal beliefs as the basis of scepticism and paraphrases Wittgenstein, "doubting is parasitic on trust"[44]. In turn, Steven Shapin - who was also part of the

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[40] The difference between the "Edinburgh School" and the so-called "Bath School" pioneered by Harry Collins lies in the causal role given to the material world to explain knowledge production. David Bloor, Barry Barnes and John Henry clarify the nature of their disagreement with Collins in this way "In contrast, where Collins insistently separates the 'natural' and the 'social' we see them as fused together; where he denies the relevance of the former, we insist upon it. For us, states of affairs in the physical environment have got to be taken into account in order to understand induction as a social process". In Scientific Knowledge: A Sociological Analysis, (Chicago; London: University of Chicago Press, 1996), p.76.

[41] Recently, Collins has published his conclusions in two books *The Gravity's Shadow: the Search for Gravitational Waves*, (Chicago; London: University of Chicago Press, 2004) and *The Gravity's Ghost: Scientific Discovery in the Twenty-First Century*, (Chicago: University of Chicago Press, 2011). Collins' previous book is *Changing Order: Replication and Induction in Scientific Practice*, (Chicago; London: University of Chicago Press, 1992).

[42] Harry Collins, "Tacit knowledge, Trust and the Q of Sapphire.", *Social Studies of Science*, 2001, Vol.31 (1), p. 82.

[43] Another important study of trust and deductive knowledge is Donald MacKenzie's *Mechanizing Proof: Computing, Risk and Trust (Cambridge, Mass.; London: MIT Press, 2001).*

[44] David Bloor, *Wittgenstein: A Social Theory of Knowledge*, (London: Macmillan, 1983), 162.

early Edinburgh School- cites Bloor's words in his book *A Social History of Truth: Civility and Science in Seventeenth-Century England* about the role of trust in modern English empiricism.[45] Despite claims from scientific institutions that knowledge derives from the rejection of authority (as encapsulated in the Royal Society's motto "Nullius in verba", which roughly translates as "take nobody's word for it"), Shapin showed that English empiricists resolved the problem of evaluating the credibility of competing claims by trusting "gentlemanly" sources of testimony. Steven Shapin draws on phenomenology and ethnomethodology to argue against the possibility of universal doubt and radical scepticism.[46] According to these sociological traditions, social and scientific life presupposes a trustful attitude towards others and the world (otherwise, as the sociologist Niklas Luhmann argues, "A complete absence of trust would prevent [one] even getting up in the morning"[47]).

Shapin illustrates the ineradicable role of trust relations, even in the sceptical search for individual and independent grounding of knowledge with a semi-fictional example. He speculates about the possibility that, as an undergraduate laboratory technician, he could have doubted the widely accepted fact that "DNA contains cytosine". Shapin explains that in order to pursue his scepticism about the chemical composition of DNA, he would need to take on trust other aspects of the experimental set-up, such as the identity of the solvent within the bottle labelled as "ethanol". Shapin concludes "It should be therefore obvious that each act of distrust would be predicated upon an overall framework of trust, and indeed, all distrust presupposes a system of taking-for-granted which makes *this instance* of distrust possible. Distrust is something which takes place on the *margins* of trusting systems. While actors' schemes may set trust and skepticism in opposition, the invitation to the analyst is to envisage a relationship trust and skepticism in which the character of skepticism depends upon the extent and quality of trust".[48]

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[45] Steven Shapin, *A Social History of Truth: Civility and Science in Seventeenth-Century England,* (Chicago ; London : University Chicago Press, 1994), p.29.

[46] Specifically, Shapin employs Alfred Schutz' concept of "natural attitude" from Schutz, *The Structures of the Life-World* (London: Heinemann, 1974) and Pollner's concept of "Mundane Reason" from Pollner, *Mundane Reason*: *Reality in Everyday and Sociological Discourse* (Cambridge: Cambridge University Press, 1987).

[47] Cited by Onora O'Neill, *A Question of Trust: the BBC Reith lectures 2002,* (Cambridge : Cambridge University Press, 2002), p.4.

[48] Shapin, *A Social History of Truth,* 19.

For these social constructivist scholars, scepticism in science is subsidiary (or "parasitic" in Wittgenstein's words) to the existence of trust relations among scientists, contrary to earlier claims made by rationalist philosophers and functionalist sociologists that science is all about individual or "organised" scepticism. Social constructivists regard universal scepticism as a source of cognitive, social and psychological disorder. As Luhmann puts it, an absence of trust paralyses everyday life and personal existence, and, I would add, prevents one from having any form of interaction with others and creating the common goods, such as knowledge, upon which we organise our societies.[49]

Whilst social constructivist scholars acknowledge trust-dependent scepticism as important to scientific knowledge, they also seem to regard scepticism as exceptional in science. Kuhn conceives scepticism as occurring in moments of crisis and as part of "extraordinary science". Shapin says that scepticism (or "distrust"; as he uses these terms interchangeably) is "marginal" to trusting systems and presumably to knowledge production.[50] As a result, research in the area of SSK and STS has focused almost entirely on the sociology of trust and has downplayed or at least neglected the empirical analysis of scepticism in science. My aim is to rehabilitate scepticism and reassert its importance (in its proper place as parasitic upon social trust) for a social constructivist account of science.

1.3. My Argument

The central argument of this thesis is that scepticism, as part of the *ordinary* practice of dendroclimatology, is dependent on trust. This idea is summarised in what I call the "parasitic view of scepticism".[51] I begin my argument with the supposition (shared in general terms with social

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[49] For a cognitivist understanding of social order read Chapter 4 of Barry Barnes, *Elements of Social Theory*, (London: UCL Press, 1995) and Massimo Mazzoti's edited volume *Knowledge as Social Order: Rethinking the Sociology of Barry Barnes*, (Aldershot : Ashgate, 2008).

[50] In another section of his book, Shapin also seems more explicit in his views about the exceptionality of scepticism: "We can, and many people do, distrust what some authoritative source says about the world, though such distrust is certainly a far less pervasive and systematic feature of natural scientific practice than some of the more fanciful textbook sociologies and philosophies would have us believe". Shapin includes a footnote afterwards, saying, "A number of sociologists of science have, for example, drawn attention to the relative rarity of experimental replication". Shapin cites Polanyi, Collins and a few Mertonian sociologists. *A Social History of Truth,* p. 21.

[51] [51] My use of the word "parasitic" draws on Wittgenstein's statement that "doubting is parasitic on trust". This choice of terminology and metaphoric language opens up different interpretations. As my colleague Anna Kuslits pointed out, the negatively connoted term "parasite" might suggest that the parasitic nature of scepticism is a bad thing. I do not make any normative claim in this regard, but as my supervisor Dr Emma Frow suggests, it is worth considering whether the "parasite harms the host" and whether the exercise of

constructivist scholars) that scepticism in science is enabled and limited by a pre-existing system of trust relations and taken for granted beliefs. Without a fiduciary framework, in Polanyi's words, there is no way that scepticism and mutually organised criticism contribute to the generation of knowledge. The novel aspect of my thesis lies, as I see it, in analysing how scepticism works as part of the normal science of dendroclimatology, especially in relation to trust.

One way to articulate the distinctive nature of my argument is to reformulate Shapin's general claim, quoted above, that states: "distrust (or scepticism; as Shapin uses these two terms interchangeably) is something that takes places on the *margins* of trusting systems". Instead, I want to emphasise the visibility and centrality of scepticism in science; my argument and hypothesis would be that scepticism is something that takes place *on the surface*, if not *at the core*[52], of trusting systems. In this thesis I advocate the need for a symmetrical study of trust and scepticism in science that examines the establishment and management of trust relations that enable scepticism to be productive in science, as well as the study of the purposes, forms and audiences of scepticism.

The questions that I have been asking in order to perform such a symmetrical account in the following empirical chapters have been: Who is trusting or being sceptical of whom and what for the purpose of conducting a specific task? What role does the exercise of scepticism play in securing trust? And to what extent does that exercise of scepticism itself depend upon the prior existence of trust? Throughout the empirical chapters, I respond to these questions by employing a series of concepts, which I define rather abstractly in this section, though I hope their meaning and relevance will become clearer as I deploy them in the empirical chapters.

In this thesis, I employ Steven Shapin's definition of *trust* as a moral bond[53] as it relates to the key sociological assumption of this thesis, namely that trust among individuals is crucial to

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scepticism damages the fiduciary framework or the trust relations upon which scepticism is based. Another interpretation is that the "parasite benefits the host", as Michel Serrès famously argued in his book *The Parasite* (Paris: Grasset, 1980). I thank my colleague Javier Guerrero for pointing me to Serrès' book.

[52] I thank my friend and colleague Michael Kattirtzi for suggesting "at the core". My reservation with the idea of scepticism being "at the core" of science is that it gives the impression that scepticism is hidden or invisible when I want to make precisely the opposite argument. If the explicitly visible aspect of scepticism is clear, I am happy to say that "scepticism is at the core of science" or that "scepticism is central to the work of scientific production".

[53] Shapin argues that all trust relations are moral because we can blame someone if he/she does not reciprocate our trust. He insists that inductively generated expectations about events in the world, such as "many people are ill in Edinburgh in the winter", also involve morality, in the sense that we can "personalise" this induction into "*you* (*and others*) *have told me* that this is the case". As a result, the philosopher Annette Baier argues that the feeling of betrayal (and not just disappointment) is the most common response when trust is broken. Baier, "Trust and Antitrust," *Ethics*, 1986, Vol. 96 (2), pp. 231- 260.

attempt to build a common body of knowledge. Shapin explains that "in order for that knowledge to be effectively accessible to an individual - for an individual to *have* it - there needs to be some kind of moral bond between the individual and other members of the community. The word I propose to use to express this moral bond is *trust*". [54] In this thesis, I talk about trust and trust relations or relationships interchangeably.

I use Michael Polanyi's term *fiduciary framework* to talk about the system of trust relationships sustaining the knowledge-making activities of a community, including sceptical practices. My use of this concept is in line with Polanyi's versatile definition, which refers to the "intellectual passions" and the "tacit dimension" of the fiduciary framework, including explicitly formulated knowledge such as "principles" and "theories". Likewise, as the notion of "framework" indicates (as a framework is constituted by multiple sticks), I conceive the fiduciary framework to be collectively constituted by individuals who align their action and beliefs to those of trusted peers.

Regarding the concept of *scepticism*, I distinguish it from the concepts of *mistrust* and *distrust*. Both distrust and mistrust imply an absence and misplaced attribution of trust. As the main assumption of this thesis is that there is no knowledge without trust, the concepts of mistrust and distrust are, *a priori*, not useful in describing the making of dendroclimatological knowledge. This does not mean that distrust and mistrust do not occur in this thesis. In Chapter 7, I describe an instance of mistrust whereby existing trust relations between participants in a controversy break down, and therefore, knowledge stops being generated.

I employ the concept of scepticism because its vernacular meaning does not preclude the existence of trust. The Oxford Dictionary of English defines "scepticism" as having "doubts about the truth of something",[55] which I interpret as being compatible with a moderate trusting attitude. Another reason for employing the concept of scepticism – even if this reason is rather disputable due to the meaning of words changing over time - is related to its etymology. According to Eric Partridge, the word "sceptic" is related to the English words *scope* (from the prefix "skop" of the Greek word for "skopein", meaning "to view"), "spectre" (to see) and "spectacle".[56] Interestingly,

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[54] Shapin, *A Social History of Truth*, 7.

[55] ODE, "scepticism", http://www.oxforddictionaries.com/definition/english/scepticism (Accessed 15 May 2015).

[56] Eric Partridge, *Origins:* A *Short Etymological Dictionary of Modern English*, (London: Routledge and Paul;1958 [2006]), p.4217.

these word associations support my argument developed below that scepticism is performed for others to see.

Besides the general concepts of trust and scepticism, I have developed a typology of different manifestations of scepticism that allows me to articulate the parasitic view of scepticism and to conduct an incipient sociology of scepticism.

First, I appropriate the concept of *organised scepticism* originally coined by Robert K Merton to refer to collective practices of scepticism. My use of this concept is different from Merton's because I disagree with his *privatisation* of the normativity of social norms. As a member of the functionalist tradition of sociology, Merton argued that the individual's expectation to conform to a norm such as organised scepticism derives from the fact that the scientist *internalises* the norm via socialisation and participation in a reward/sanction system. I agree with the sociologist Barry Barnes in that in the functionalist account "the externalities are wrongly located".[57] The source of normativity is not the individual private mind, but society. Whilst it might be true that the individual scientist might feel pressured "to conform to a norm" and to be sceptical, the source of this compulsion is not the "inner voice" of the scientist, as Merton suggests, but the people around the scientist who invoke the norm and negotiate what it means to act sceptically.

One aspect of Merton's sociology I agree with is his identification of the reward system that scientists create to mutually sanction and honour behaviour as the source of institutionalised practices of scepticism in science.[58] Therefore, rather than talking about organised scepticism as a superego that forces dendroclimatologists to act sceptically, I refer to organised scepticism in this thesis as the pattern of observable sceptical practices that result from mutual control and monitoring among scientists[59].

Second, I borrow David Bloor's term *civil scepticism* to refer to instances of organised scepticism that do not challenge and are based upon a fiduciary framework.[60] As part of the normal

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[57] Barnes, *The Elements of Social Theory*, 59.

[58] This point is more generally formulated by Barry Barnes in "Catching up with Robert Merton: Scientific Collectives as Status Groups", *Journal of Classical Sociology*, 2007, Vol.7 (2), pp.179-192.

[59] Susan Wagenknecht calls the sceptical exercises that render scientists accountable to each other "Dialoguing Practices and Explanatory Responsiveness" in "Facing the Incompleteness of Epistemic Trust: Managing Dependence in Scientific Practice", *Social Epistemology*, 2015, Vol. 29 (2), pp. 160-184. An excellent empirical study of sceptical monitoring is Jason Owen-Smith, "Managing Laboratory Work through Skepticism: Processes of Evaluation and Control", *American Sociological Review*, 2001, Vol. 66 (3), pp. 427-452.

[60] David Bloor uses this term to title his review of Barbara Herrnstein- Smith' book *Belief and Resistance: Dynamics of Contemporary Intellectual Controversy (Cambridge, Mass.; London: Harvard University*

work of science, scientists trust that colleagues will examine their arguments seriously, fairly and at face value. The scientist expects that colleagues will be sceptical about the "right" things that the community expects them to be sceptical about as well as trustful of the "appropriate" things that they should accept. The fiduciary framework shared by mutually trusted producers of knowledge delineates the plausibility of scepticism. Members who trust each other and contribute to the maintenance of a fiduciary framework agree on what constitutes courteous, reasonable and civil scepticism.

The practice of civil scepticism occurs in parallel to the definition with the boundaries of the trusting community of producers of knowledge. People who challenge the fiduciary framework are seen by mutually trusted parties to be conducting *uncivil scepticism* and to be outsiders to the community. Uncivil sceptics are not trusted by community members to be competently sceptical and trustful individuals and are thus excluded from the community of producers of knowledge. Most of the empirical chapters of this thesis exemplify the practice of civil scepticism. The chapter about a controversy in dendroclimatology is the only one that presents a specific instance of uncivil scepticism. In Chapter 7 I clarify the way the exercise of uncivil scepticism relates to the fiduciary framework and how the mutually trusted contributors to that framework react to this challenge.

Third, I coin the concept of *sceptical display* to refer to enactments of organised scepticism. In the empirical chapters I analyse specific instances of sceptical display on the basis of three characteristics: *conventions*, *audiences* and *situations*.[61] Conventions refer to the form in which scepticism is expressed in a particular setting, being those instances of verbally articulated scepticism or to what I refer as "scepticism-as-an-account" of particular interest; the audiences are the people whose interests the sceptical scientist aims to enrol or whose objections he/she might seek to pre-empt; the situation is the social context in which the enactment of scepticism adopts significance. Enacting scepticism serves to ensure that the knowledge produced has been properly tested and critiqued and to secure the internal credibility of knowledge insofar as it is exercised among trusted parties.

Among trusted peers, demonstrating that you are a competent sceptic reinforces trust in your sceptical competence, and ultimately, trust in your ability to contribute to the production of knowledge. Sceptical display might contribute to the maintenance and consolidation of existing

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Press, 1997) in David Bloor, "A Civil Skepticism.", *Social Studies of Science*, 1998, Vol.28 (4), pp.655-665. As far as I am aware Bloor has not developed the term civil scepticism either theoretically or empirically.

[61] I borrow this tripartite classification from Jan Golinski, *Making Natural Knowledge: Constructivism and the History of Science,* (Cambridge: Cambridge University Press, 1998), p.107.

trust relations, and consequently there is a degree of reciprocity between trust and scepticism. Yet, the primary relationship is of *unidirectional* dependency of scepticism on trust. Productive scepticism in science is not possible without trust, but scepticism is not a necessary and sufficient condition for trust, as there are other ways in which scientists establish and manage trust relations. As I show in Chapter 7, if trust relations have been broken or do not exist in the first place, certain forms of sceptical display might be considered uncivil scepticism and a reason for further distrusting.

The last concept I employ is that of *collective suspensions of scepticism* to describe instances whereby scientists defer the exercise of organised scepticism to other trusted people in order to consolidate knowledge. As part of existing divisions of labour in science, competent scientists are aware that there are necessarily limits to their competence as sceptics and to the time they have to validate other people's claims and they have to trust generally distant others[62] to exercise the relevant exercises of scepticism for them. Alternatively, if scientists were constantly sceptical about whether their colleagues are properly sceptical, they would end up not being able to use their knowledge, so scientists end up trusting their colleagues to be sceptical, which brings us back to the parasitic view of scepticism.

I differentiate between collective suspensions of scepticism and *collective suspensions of disbelief* or *fictions*[63]. The difference between the two is the *content* of the scientists' knowledge.

[62] In his study, Luis Reyes-Galindo also identifies similar instances of collective suspensions of scepticism (or "suspensions of doubt" as he calls them). He understands these instances as part of the continuum of different trust-based strategies that the physicists of his study employ to deal with communication across different scientific communities in "Linking the Subcultures of Physics; Virtual Empiricism and the Bonding Role of Trust", *Social Studies of Science*, 2014, Vol.44 (5), p.745.

[63] My characterisation of collective suspensions of disbelief and fictions draws inspiration from the fascinating book by Hans Vaihinger entitled *The Philosophy of "As-If": a System of the Theoretical, Practical, and Religious Fictions of Mankind* (London: K. Paul, Trench, Trubner and Co., Ltd.; New York: Harcourt, Brace and Company, Inc., 1924). Drawing on multiple examples, Vaihinger argued that, over centuries, humans have created knowledge of the natural and social world with an explicit awareness that their knowledge is incomplete, if not flawed. Vaihinger defined a fiction thus: "an idea whose untruth and incorrectness and therewith its falsity is admitted, is not for that reason practically valueless and useless; for such an idea, in spite of its theoretical nullity may have great practical importance" (p. vi). Vaihinger understood fictions to be the result of the individual mind not being able to comprehend the complexity of the world, and thus giving rise to the illusion "As-If" the world were being comprehended. Most importantly, Vaihinger emphasised the fact that, despite being false, fictions have a practical value as "intermediary mental operations" to generate knowledge about the world. He formulated the philosophy of "fictionalism" as an alternative to pragmatism, which he defined as sustaining the wrong idea that only truthful ideas are useful. My appropriation of the Vaihingerian notion of a fiction is sociological, rather

Whilst in cases of collective suspensions of scepticism, scientists regard some knowledge as approximately and provisionally true, collective suspensions of disbelief refer to knowledge that scientists explicitly say that they consider to be false, and yet use "As-If" it was true in order to make truthful claims about the world. Fictions are epistemological objects that producers of knowledge use for expediency to do a particular task. Fictions can be considered "black-boxes"[64] or widely accepted pieces of knowledge characterised by their widely accepted false and useful content. A fiction does not represent any collective suspension of scepticism because when scientists use fictions they do not make any truth claim in the first place. Fictions, in the few instances (only two) in which they arise in this thesis, are one of the reasons for scientists' scepticism; demonstrating that one knows about fictions is one of the ways in which scientists perform their scepticism with/for their peers.

1.4. My Methodology

The evidence I present for the role of trust and scepticism in the making of dendroclimatological knowledge derives from a three-year study of one dendroclimatological project carried out by a group of scientists based in the Department of Earth and Environmental Sciences at St Andrews University in Scotland. Rather than discussing about the making of dendroclimatological knowledge in the abstract, I describe in detail how Dr Rob Wilson and his team of collaborators in the "Scottish Pine Project" have produced a temperature reconstruction for Scotland dating back to 1200AD. The Scottish Pine Project has existed since 2006, and my case study covers the time

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than psychological and epistemological as formulated originally by the author. My interest is not in understanding the individual's mental operations or evaluating the degree to which fictions are at odds with reality. Instead, my interest is in describing how, through training and negotiation with colleagues, individuals come to regard certain knowledge as false, and crucially, how individuals' suspension of disbelief and their use of fictions "As-If" they were accurate representations of the social and natural world are related to communal practices and beliefs, which ultimately allows people to create knowledge that they could not create otherwise and constitute societies. A few contemporary philosophers employ Vaihinger's philosophy (see Arthur Fine, "Fictionalism", Midwest Studies In Philosophy, 1993, Vol. 18 (1), pp. 1-18). Currently, in the philosophy of science, fictionalism is being discussed in relation to scientific models. See Frigg, "Models and fiction." *Synthese*, 2010, Vol. 172 (2), pp. 251-268.

[64] Bruno Latour explains "Blackboxing is the way scientific and technical work is made invisible by its own success. When a machine runs efficiently, when a matter of fact is settled, one need focus only on its inputs and outputs and not on its internal complexity. Thus, paradoxically, the more science and technology succeed, the more opaque and obscure they become". Latour, *Pandora's Hope: Essays on the Reality of Science Studies* (Cambridge, Mass.; London: Harvard University Press, 1999), p.304.

period from April 2012 to September 2015. This period coincides with the duration of the doctorate of Rob's graduate student, Miloš Rydval, who is working on the Scottish Pine Project. In this thesis, I employ two types of data. The first type of data is observations of Rob and Miloš' work on reconstruction of past temperatures in Scotland (Image 1). The second type of data is the dendroclimatology textbooks and articles that I have compared against my observations in order to appreciate the historical and spatial distinctiveness of Rob and Miloš' work.

Image 1. My two research subjects (Rob on the left of the picture, me in the middle and Miloš on the right) during my first participation in the Scottish Pine Project fieldwork expedition.

To generate observations of Rob and Miloš' work, I have used the method of "participant observation", which consists of observing people's life and work while living and working alongside them. By becoming emotionally involved in the life and work of the research subjects, the participant observer hopes to acquire a near native understanding of their language and culture.[65] Scholars in SSK and STS have developed different strategies to study different scientific and technical communities.[66] In my research, I have followed Ervin Goffman's advice and I have been "willing to become a horse's ass".[67] That is, throughout the empirical chapters, I offer examples of how my "mistakes" in understanding the making of dendroclimatological knowledge (for instance in identifying trees to sample or in measuring tree-rings), have revealed the phenomenon of trust and scepticism that I was interested to study. The danger for the participant observer is "going native", and accepting the subject's accounts of their life and work as analytical explanations.[68] Indeed, one of the main difficulties I have faced in producing the evidence of this thesis has been to avoid reproducing Rob and Miloš' discourses, and instead generating analytical accounts that give an interpretation of their work that is different from their common sense understandings or rhetoric.[69]

As my research evolved, I adopted a progressively more passive role as a participant. I believe that this evolution is due to the nature of dendroclimatological knowledge and the fact that, at the later stages, Rob and Miloš only trusted a few people to be expert producers of knowledge. At the beginning of my research, I participated in two fieldwork expeditions (August 2012 and August 2013) in the Scottish Highlands with the members of the Scottish Pine Project, which

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[65] For researchers who use participant observation as a method, the resulting evidence is better than that produced by the researcher who very occasionally meets the research subjects to interview them or does not meet them at all, as is the case with surveys. Howard Becker and Blanche Geer, "Participant observation and interviewing: A comparison." *Human Organization*, 1957, Vol. 16. (3), pp. 28-32.

[66] For instance, Latour and Woolgar played the role of the "stranger" in *Laboratory Life: the Construction of Scientific Facts* (Princeton, N. J.; Chichester: Princeton University Press, [1979] 1986); Collins developed the term "participant comprehension" in "Researching Spoonbending: Concepts and Practise of Participatory Fieldwork" in: C. Bell and H. Roberts (eds.), *Social Researching: Politics, Problems, Practise* (London: Routledge and Kegan Paul, 1984), pp. 54-69. For a review of the use of ethnography in STS, read David Hess, "Ethnography and the Development of Science and Technology Studies" in: P. Atkinson et al. (eds.), *Handbook of Ethnography* (London: Thousand Oaks; New Delhi: Sage Publications, 2001).

[67] Erving Goffman describes participant observation as the "willingness to be a horse's ass" in "On Fieldwork", *Journal of Contemporary Ethnography*, 1989, Vol.18 (2), pp.123-132.

[68] Diane Forsythe forcefully argues that doing fieldwork is NOT just chatting with people and reporting what they say in "It's Just a Matter of Common Sense: Ethnography as Invisible Work", *Computer Supported Cooperative Work* (CSCW), 1999, Vol.8 (1), pp.127-145.

[69] In his thesis (*Expertise and the Fractal Model Communication and Collaboration between Climate-Change Scientists*, PhD thesis, University of Cardiff, 2013) Tiago Ribeiro Duarte very appropriately describes the social scientist's dual role as an insider and outsider to the research subjects' world as "alternation" in reference to Berger's use of this concept to talk about religion conversions in his beautiful book *Invitation to Sociology: A Humanistic Perspective*, (Hammondsworth, Middlesex, England: Penguin Books, 1963), p. 65.

means that I helped produce samples and I ate and slept in the same house as the expedition group for a period of a week each time. For a period of a year from April 2012 until April 2013 I also worked one day per week as a "voluntary technician" for Miloš helping him to prepare and measure tree samples and to generate tree-ring data in the laboratory in St Andrews. During this same year I also attended the undergraduate paleoclimatology course that Rob taught at St Andrews University.

From the moment Miloš and I finished measuring all the tree-ring samples in autumn 2013, I blended in the background.[70] I followed Rob and Miloš wherever they carried out their work, and, occasionally, I asked a few questions. From October 2013 onwards I started visiting Miloš at his house almost every week to observe how he created tree-ring data and chronologies. I was "present" for almost all the conversations between Rob and Miloš that took place either by email or at physical meetings in St Andrews and in Rob's house. I have copies of the email interactions where I was "cc'd" and I audio-recorded the face-to-face meetings. I was given access to the presentation slides and article drafts written by Rob and Miloš. I sat in a workshop organised by Rob in St Andrews in April 2013 where he and Miloš discussed their results with other colleagues and I audio-recorded this workshop discussion. I travelled with Rob and Miloš to Melbourne where they first presented their temperature reconstruction at an international conference. Before this conference, I attended a one-week training course of statistical dendroclimatology in Tasmania where I met other senior and junior dendrochronologists. I attended this course because I wanted to be taught by one of the main experts in statistical dendroclimatology (Edward Cook) and because I sought to understand how Rob and Miloš' work compared to the work of others. The only formal interview I conducted was with Edward Cook in Tasmania. I do not consider my recorded meetings with Rob and Miloš as interviews because I never summoned these meetings, and the conversations were unstructured. I never transcribed the entire recordings; I annotated fragments and expressions and their associated timing.

Throughout my research I always sought feedback from dendroclimatologists. In order to evaluate the plausibility of my ideas[71], I gave three talks in front of dendroclimatologists. Upon request from a few attendees of the training course in Tasmania, I accepted to give a five-minute

[70] Or as Alice Goffman (the daughter of Erving Goffman) puts it, I became "a fly on the wall" in *On the Run: Fugitive Life in an American City*, (New York: Picador/Farrar, Straus and Giroux, 2015), p.237.

[71] I follow Hammersley's suggestion that one of the main standards for assessing ethnographic research should be "plausibility" in terms of being reasonable to the research subjects and to the community of scholars "Standards for Assessing Ethnographic Research*", Reading Ethnographic Research: A Critical Guide, (New York ; London : Longman, 1998), pp. 58-77.*

talk on “what I have learnt during this week as a sociologist”. I also gave a talk at the dendrochronology conference in Melbourne in January 2014 and another in Aviemore in May 2014 at a postgraduate conference that Rob organised. In these two last presentations, I illustrated my research method with an analogy: one image of me on an expedition in the Scottish Highlands that shows me observing and taking notes on dendroclimatologists’ work, and another of the primatologist Jane Goodall taking notes on chimpanzees’ behaviour (image 2).

Image 2. In my presentations to dendroclimatologists, I explained my method of generating data of dendroclimatologists’ work by comparing it with the observational and participatory work of the primatologist Jane Goodall.

One of the most important aspects of the practice of conducting participant observation is note-taking. The sociologist Beatrice Webb described the “art of note-taking” as “an instrument of discovery that serves a similar purpose in sociology to that of the blowpipe and the balance in chemistry or the prism and the electroscope in physics”. [72] As with any other scientific instrument, the practice of note-taking requires training and creativity. I learnt a great deal about ways to conduct participant observation by reading published case studies as exemplars. The authors of these studies often reflect upon the practicalities of conducting participant observation, including note-taking. The books that I have found particularly inspiring are: *Street Corner Society* by William Foote Whyte[73]; *Doing Fieldwork: Warnings and Advice* by Rosalie H. Wax[74]; *Boys in White: Student Culture in Medical School* by Howard S. Becker, Blanche Geer, Everett C Hughes

[72] Cited in Robert Burgess, *Field Research: a Sourcebook and Field Manual*, (London: Allen and Unwin, 1982), p. 195. Also in Beatrice Webb, "My Apprenticeship”, (London: Longmans, Green and Co., 1926).
[73] William Foote Whyte, *Street Corner Society: the Social Structure of an Italian Slum*, (Chicago; London: University of Chicago Press, 1955).
[74] Rosalie Wax, *Doing Fieldwork: Warnings and Advice*, (University of Chicago Press, 1986).

and Anselm Strauss[75]; and *Laboratory Life*: *the (Social) Construction of Scientific Facts* by Bruno Latour and Steve Woolgar[76]. In my own practice of note-taking, I dealt with two challenges: knowing what and when to write in my notes and how to transform these notes into data.

Regarding the content of my notes, I wrote what I saw, I heard, I smelt and I thought while being in specific situations of fieldwork. Over time I discovered that certain circumstances were better than others to write notes. I learnt how to "fake" off-phase note-takings during meetings and workshops.[77] That is, sometimes I did not write notes on a specific situation or comment that I had just observed or heard because then people would know what it was that I was recording, which would disrupt their conversation. Instead, I would always try to write notes continuously so that people would not be able to detect when I was starting to take notes. I always asked permission to record the meetings. I did not take notes during meals, drinks or other social events. In these situations, I remembered a keyword related to the specific situation or comment that caught my attention and as soon as I was alone (I often went to the toilet to be alone) I would write down these words in my pad. I also pretended to send a message with my phone or give a call in order to write down these keywords. Other times, I took notes in a very explicit manner as a "signal" to dendroclimatologists. This strategy was useful when I participated as a student in the training course in Tasmania where I had to do the same work as my dendroclimatology colleagues in the group. I was worried that my colleagues would forget that I was also doing *my own* work as a sociologist, and so in order to establish some boundaries, I kept my recorder very visible and I attached a pen and a little piece of paper to my top with a clip.

To generate data about practices of trust and scepticism in the making of dendroclimatological knowledge, I adopted the strategy of asking "breaching questions" and actively intervening in different settings. This approach is inspired by the "breaching experiments" conducted by the sociologist Harold Garfinkel with his students.[78] Essentially, Garfinkel asked his students to distrust systematically what other people told them on an everyday basis. One student asked the bus driver, "Does this bus go down to Morgan Street?", and after the bus driver answered, "Yes", the student would then ask, "How do you know?" One

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[75] Howard Becker, et al., *Boys in White: Student Culture in Medical School*, (Chicago: University of Chicago Press, 1961).

[76] Bruno Latour and Steve Woolgar. *Laboratory Life: The Construction of Scientific Facts*, (Princeton University Press, 1979).

[77] Erving Goffman, "On Fieldwork", *Journal of Contemporary Ethnography*, 1989, Vol.18 (2), p.130.

[78] Harold Garfinkel, *Studies in Ethnomethodology*. Cambridge: Polity Press, 1967. p. 35.

of the most famous of these experiments is that of a housewife student who distrusted her husband's account of why he was home late the night before, which generated resentment between the couple long after she admitted the experiment. Garfinkel designed these experiments to show students the risks associated with the practice of distrust in everyday affairs and the trust-dependency of our relations with others.

During my research I often asked Miloš and Rob challenging questions that made visible the trust-dependency of their knowledge. For instance, during the early stages of my fieldwork I asked Miloš if he trusted a piece of knowledge (so-called "carbon dates") that he had obtained from another laboratory. He looked at me visibly perplexed and claimed "Of course!" My question revealed the routine trust that Miloš placed on carbon dating experts. My question also had the effect of giving the impression that I questioned Miloš' trustful behaviour. Therefore, after my question, Miloš explained extensively to me why he trusted these carbon dates. Miloš emphasised that carbon dating has developed for decades ("it's a whole area of science", he said) and that carbon dating experts have identified specific periods when the dating uncertainties are greater, and thus, dates are expressed in probabilistic terms or "date ranges". With Miloš'explanation, I generated what I later first considered to be evidence of collective suspensions of scepticism. As I was slightly surprised by the length of Miloš' explanation, I followed-up with another question: "Why do you think it is important for me to know all this information"? Miloš' responded, perhaps out of frustration on his part, "Because you asked before whether I trusted these dates, and I think it's important to show you the methodology to obtain them and all the uncertainties behind them". As a result of my breaching question, Miloš felt he had to *show* me his competence and scientific reasoning by *articulating* his scepticism of carbon dating and his understanding of the uncertainties of this method that he had learnt from trusted experts. Miloš'answer was the beginning of my theorisation of the notion of sceptical display and, specifically, of scepticism-as-an-account.

The first step I took in transforming fieldwork notes into data was to use the few words or jottings I had written in my pad or phone to evoke entire situations. This process of "evoking" would generally take me hours and days, and I never waited more than one day after fieldwork in order to minimise the risk of forgetting any details. Also, I never rushed the invoking process. I would start typing on the computer only after I had slept sufficient hours and eaten a meal. In these extended fieldwork notes, I recalled situations and transcribed in detail a few conversations that I had already noted in my pad as relevant. I transcribed both verbal and nonverbal actions like pauses, voice tones and facial expressions. I often included my own reflections on and interpretations of these events and utterances in the notes. I highlighted words or entire paragraphs that I thought would be worth checking again in the future. I organised my notes by date and whenever I wanted to look for a specific aspect of the data I used the keyword search in the word processor. As my research progressed, I started to recognise recurrent empirical themes and created different folder files for each theme in my computer. I copied and pasted vignettes or conversations from the notes to these files. Once I decided on the structure of the thesis, I started writing each chapter in two parts: the description of the events and my sociological interpretation of these events. I sent the descriptive chapters to Rob and Miloš to double-check my understanding of the events.

My approach to the drafting of chapters was that of "sacrificial writing".[79] I conceived every new draft as a "sacrifice", in the sense that I wrote it with the idea in mind that it would be a temporary version that I would need to rewrite or "kill" with the aim of writing an even better version. In this way, it was less "painful" to start drafting new chapters from scratch when my supervisors and I agreed that the current version was not good enough. The thesis draft started to become permanent and coherent both within and across chapters when I decided to flip the presentation of the terminology of this thesis (section 1.3). Instead of presenting the concepts as the result of my research and therefore including them in the conclusion chapter, I placed them at the start of the thesis to help me construct a more logical story. I set myself the task of showing retrospectively in each empirical chapter how these concepts had helped me to construct my epistemological and sociological narratives (see section 1.5). As a result, in the conclusions chapter I was able to think about the "bigger picture" and to characterise in more abstract terms the patterns of the narratives.

My strategy in theorising from the data was based on the comparison of exemplars or "indicators" of the more abstract phenomenon of trust and scepticism.[80] Specifically, I compared what I call the "carbon dates example" that I described above in relation to my breaching question and what I call the "ethanol bottle example" based on Steven Shapin's semi-fictional example of

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[79] Jane Calvert mentioned this concept in a seminar at our STS department at the University of Edinburgh in 2014. The idea of "sacrificial writing" came from the idea of "sacrificial design" that is part of design thinking that Jane was introduced to by people at the design firm IDEO. See "Sacrifical Concepts 2" in *Design Thinking*, 9 July 2008, http://design-thinking.blogspot.co.uk/2008/07/sacrificial-concepts-2.html (Accessed 8 September 2015).

[80] I followed this procedure instinctively and I was very pleased to discover afterwards that a sociologist I admire very much, Howard Becker, recommends it as a strategy in "Problems of Inference and Proof in Participant Observation", *American Sociological Review*, 1958, Vol. 23 (6), p. 653.

the limits of a sceptical experiment on the chemical composition of DNA. During most of my doctorate, I focused on characterising the differences and similarities between these two specific exemplars as a means to start building up a theoretical model of the data. The most obvious similarity I found was that both cases are an instance of an individual routinely trusting other experts (carbon dating and ethanol making experts) to be competent at their jobs (including being competently sceptical), and this division of cognitive and social order enabling the production of scientific knowledge. I reasoned that the most important difference was that in the carbon dating example, and as a result of my breaching question, Miloš felt compelled to justify and rationalise his trust to me by articulating his scepticism and awareness of the uncertainties underlying carbon dating. In conversations with my supervisors and colleagues, I progressively understood the novelty of this conclusion. As I wrote in my research notes in March 2014, "it seems that there is something else going on other than trust".

During most of my doctorate, and in line with social constructivist authors who have emphasised the importance of trust relations in knowledge production, I "just" focused on showing the importance of trust relations in the making of dendroclimatological knowledge. Retrospectively, I can see that the theory had "blinded" me to scepticism. The robustness of the social reality and my realist attempt to produce a faithful description of the social world of my dendroclimatologists "saved" me from producing a partial account that would have focused only on the role of trust in the making of dendroclimatological knowledge.

In September 2014 (just when I thought I would submit my thesis), I decided to re-read my field notes and to look for similar instances to the carbon dating exemplar in order to evaluate the frequency of scepticism in dendroclimatology. I discovered plenty of similar examples whereby Rob, Miloš and other dendroclimatologists articulated their doubts about specific aspects of their knowledge and I understood this attitude as rationalisations.[81] Later I decided that, in order to be faithful to my actors' accounts, I had to distinguish between dendroclimatologists saying that they acted "As-If" something they know to be false were true (which I call a fiction or collective suspensions of disbelief) and talking about something they know to be uncertain but still true (which I call collective suspension of scepticism). I also identified multiple cases in which Rob and

[81] Crucial to my interpretation of scepticism-as-an-account is the fact that I attended Barry Barnes' seminar in my department in March 2014 titled "Rationality as the power to rationalise". Barnes' lectures are available on Youtube mainly thanks to Valeri Wiegel: https://www.youtube.com/watch?v=b0vu2_7FnzY

Miloš actually practiced scepticism through experimentation and testing rather than just articulating scepticism-as-an-account.

Overall, my conclusion was that scepticism was a typical and widespread feature of the way Rob and Miloš made dendroclimatological knowledge, and therefore, I had to include scepticism in my analysis. I then re-read how and whether other scholars had tried to analyse the simultaneous relationship between trust and scepticism in science. Unlike other literary reviews I had conducted in previous stages of my doctorate, this time I approached the ideas of those authors I admired so much with less "respect" in the sense that I felt warranted disputing their claims on the basis of my empirical conclusions. I felt "brave" enough to develop a typology of theoretical conceptualisations of the relationship of trust and scepticism in science, which I later refined in conversations with my supervisors. I then discovered what I now regard as a "gap" in the sociological literature about science: Mertonian and social constructivist authors had not analysed scepticism empirically and I could use my thesis to start doing this.

One crucial aspect of the nature of the evidence I present in this thesis is that it is a result of the friendship I have developed with Rob and Miloš over time. Evidence of this relationship of mutual trust is multiple: I have invited Rob and Miloš to my house on many occasions; I have been invited to Rob and Miloš' houses for meals and a housewarming party; Miloš has invited me to his wedding; and I helped Miloš to move flats. Even though I have now completed my research, I still speak to Miloš and Rob often by email and Skype and I am planning to participate in the final fieldwork of the Scottish Pine Project in September 2015.

As evidence of the changing nature of my research relationship with dendroclimatologists is the evolution of the language they used to define me. In the very early stages of my research, Rob called me a "climate sceptic" as if I were intending to expose the next Climategate scandal. Initially I strived to correct their presentation of me, but later on I realised that Rob and Miloš meant it as a joke and I went along with it. Occasionally, Rob called me a "science communicator and I often responded "I am more of a communicator of science-in-the-making and the process of doing science than a science communicator". Rob also called me a "social scientist" and I was happy with that. Perhaps because Miloš and I had a closer working relationship and he knew my interests and work perhaps better than Rob, Miloš always referred to me as a "sociologist", which was the professional label I used to define myself. In the Tasmanian field week, someone (I did not write in my notes exactly who) started calling me a "dendro-sociologist", which I interpreted as a welcoming gesture to the community of dendroclimatology. [82] In my conference presentation in Melbourne I used the phrase “dendro-sociologist” as a joke to define myself and since then a few dendroclimatologists have called me this way.

Dendroclimatologists seemed to recognise the special relationship I had with Rob and Miloš. In March 2015, Rob forwarded to me an email that one colleague had sent him after I published a short article about my research.[83] This email included an image of Jane Goodall kissing a chimpanzee, and the email included a sentence saying “Meritxell and Rob? :)” (Image 3). This image referred to the visual analogy I used to illustrate my research methods, and it shows that dendroclimatologists were aware that my methodology, like Goodall’s, is not based on a distanced observation of research subjects, but on the establishment of relationships of affection with them. The sociologist Lisa M. Tillmann-Healy characterises “friendship as a research method” and she describes it as involving “the practices, the pace, the contexts, and the ethics of friendship. Researching with the practices of friendship means that although we employ traditional forms of data gathering (e.g., participant observation, systematic note taking, and informal and formal interviewing), our primary procedures are those we use to build and sustain friendship: conversation, everyday involvement, compassion, giving, and vulnerability”.[84]

[82] As I describe in Chapter 3, dendroclimatology is one of the branches or “applications” of dendrochronology. Therefore, by calling me a “dendro-sociologist”, I had the impression that a few dendrochronologists and dendroclimatologists regarded me as part of their community.

[83] Meritxell Ramírez-i-Ollé, “The Social Life of Climate Science”, *Method Quarterly*, 2015, Issue 2, http://www.methodquarterly.com/2015/02/the-social-life-of-climate-science/ .

[84] Tillmann-Healy, "Friendship as Method.",*Qualitative Inquiry*, 2003, Vol.9 (5), p. 734.

Image 3. This image was used by one dendroclimatologist as a “joke” about the friendship between Rob and me, which characterises the methodology of this thesis.

Considering friendship to be the methodology of this thesis is in accordance with my key argument, namely that trust relations are essential for the making of all forms of knowledge, including dendroclimatology and, I would argue, sociology. In this way, I have been able to generate knowledge about the role of trust and scepticism in dendroclimatology due to having established trust relations with Rob and Miloš in the first place. In the remaining part of this section I explain how I have created and maintained trust relations with them.

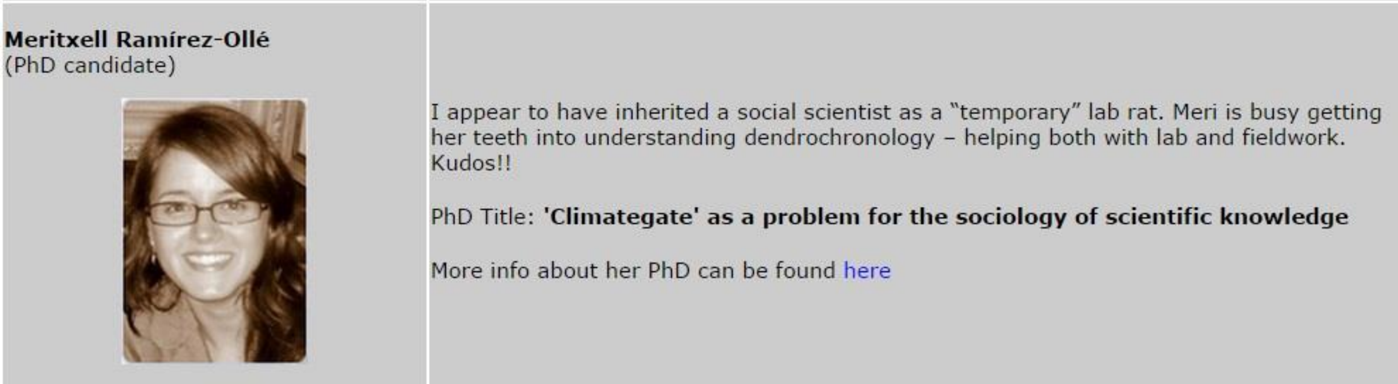
The first factor that I believe helped me engender trust with Rob and Miloš was my academic status as a doctoral student. Being a PhD student from the University of Edinburgh has given me, by default, a reputation for trustworthiness. I first contacted Rob in late 2011 during the aftermath of Climategate, and my supervisors and I were afraid that Rob would not allow me to observe his work. As a strategy to build up trust relations with Rob, we decided to emphasise my academic credentials and relations. We asked a climate scientist from my university, Dr Gabi Hegerl, to be a member of my first year doctoral examination panel and to act as a mediator with Rob. Dr Hegerl very kindly accepted our request. She gave me very useful advice during my board and she sent an email to Rob introducing my work. In my first email to Rob I also sent him a brochure of my work that included logos of my university and funding research body. I also included these logos in my presentations to dendroclimatologists, and I stressed my identity as an academic by introducing myself as a “social scientist” or a “sociologist of science”. My status as a PhD student has been particularly crucial to developing a strong sense of collegiality and friendship with Miloš. The first day I met Miloš, he told me about his concerns about producing a good thesis. Being a doctoral student myself, I understood Miloš very well and I also shared with him the fears I had about my work, including the risk that he and Rob would not allow me to observe their work. Since that first day and for three years, Miloš and I have given each other moral support in our respective journeys to finish our doctoral theses.

The second factor that probably enhanced my trustworthiness was my willingness to participate actively in the early stages of dendroclimatology work that Rob and Miloš carried out, rather than just being a passive observer. During the first year and a half of my research, I had a very active role as a participant: I learnt how to sample trees, how to prepare samples for measurement and how to conduct some basic analysis of these measurements under the supervision of Rob and Miloš. Instructions always involve a relationship of trust, as the student accepts the authority of the teacher, and in so doing, becomes trusted as competent by the teacher and by the community of experts that recognise the teacher as such. [85]

In the process of doing and learning dendroclimatology, I developed reciprocal trust relations with Rob, Miloš and other dendroclimatologists. When Rob introduced me to his colleagues, he always mentioned the fact that I had basic experience doing fieldwork and laboratory work. In the workshop in St Andrews that Rob organised, he introduced me by saying, “Meri knows the science; she has been doing lab work with us and she was in fieldwork with us too”. Another public demonstration of Rob’s trust in me was the fact that he included me as one of his students on the website of the Tree-Ring Lab in St Andrews. In his description, Rob emphasised my involvement in laboratory and fieldwork (Image 4). Even in the later stages of dendroclimatological work, when I became a passive participant, Rob considered my participation to be useful and a reason to continue trusting me. I found out that in the “Acknowledgement” section of his blue intensity paper (Chapter 4), Rob had included my name alongside others “for their comments and discussion on this work”.

[85] This is a point I illustrate more explicitly in the empirical chapters. I draw on Barry Barnes’ interpretation of Kuhn’s work on this idea. See Barnes, T.S. Kuhn and Social Science, (London: Macmillan, 1982), p. 16.

Image 4. This is the familiar description ("Meri") that Rob included of me as a "student" on his website for the Tree-Ring Laboratory in St Andrews emphasising my involvement in laboratory work and fieldwork.



The third factor that I imagine could have reinforced my reputation as trustworthy is my readiness to engage in civil scepticism about my work with Rob, Miloš and the community of dendroclimatologists more generally. My two presentations at the dendroclimatology conferences are two crucial moments that, I think, consolidated my trust relations with dendroclimatologists. In my first presentation in Melbourne in January 2014, I discussed one aspect of dendroclimatology (whether I could characterise tree-ring dating as a "ring-counting" activity, Chapter 3) where Rob had previously told me that he disagreed with me. I emphasised to the audience of my presentation that I was aware that they might find my interpretation slightly controversial, and during the question and answer session a few people made some suggestions for refining my account. To my surprise, at the end of the conference, the scientific committee awarded me a prize for one of the best student presentations of the conference. When I asked one of the members of the committee about the reasons for this award, he emphasised the fact that I was willing to establish a critical conversation with dendroclimatologists. More precisely, he said "we appreciated that you had the courage to present your work in front of us". In my second presentation in Aviemore in May 2014, I presented my account of the role of fictions in dendroclimatology. I had received a few objections from the reviewer of my abstract, which I later addressed during my presentation.

My cordial exchanges of mutual criticism with dendroclimatologists only became possible because I had previously established trust relations with many of them. The week before my presentation in Melbourne, I had the opportunity to build trust relations with many dendroclimatologists who later attended my presentation at the conference and were members of the scientific committee that awarded me with the prize. In fact, the chair of the conference session at which I presented my work, Edward Cook, had been my teacher on the training course, and knew me and my work quite well. With regards to my second presentation in Aviemore, I found out that many people knew of me in advance as “the sociologist who presented in Melbourne”. I did not know many of the conference participants, and I asked Rob if I could be the last speaker of the conference so that I had time to get to know, and crucially, to become known to participants of the conference before my talk.

Using friendship as a methodology involves one major risk, which is the possibility of betraying the research subjects. As my trust relations with Rob and Miloš grew stronger, I became wary that my use of breaching questions that explicitly implied a distrustful attitude towards their work could jeopardise my trust relations with them. At the beginning of my research, Rob and Miloš perceived questions such as the one I asked about carbon dating as relatively inoffensive and as a sign of *my* ignorance. As I became trained in dendroclimatology, I noticed that they were less tolerant of my questions, perhaps because they expected me to know the answer to these questions. Even though I never asked them to interpret their reaction reflexively, I was worried that Rob and Miloš interpreted my breaching questions as a sign of *their* incompetence as teachers. Therefore, during the later stages of my research, I came up with a “strategy of detachment” that would allow me to transform my breaching questions from a gesture of distrust to one of civil scepticism. Every time I asked a question, I explicitly said that I was wearing “my sociological hat” as a means to differentiate between my role as “Meritxell-the-sociologist” and “Meritxell-the-friend”.

Another form of betrayal I foresee relates to the risk of generating uncivil scepticism of my research subjects’ work from people whom I do not trust to interpret my evidence correctly. This risk is closely related to the issue of anonymity and ethics protocols. To my relief (as the possibility of providing full anonymity was limited), both Rob and Miloš have always insisted they do not want to be kept anonymous. As my research progressed and other individuals became involved in Rob and Miloš’ work, I always asked these individuals for permission to generate and use the data about their interactions with Rob and Miloš. Whilst I never asked these individuals to sign any consent form, I promised each of them verbally the possibility of anonymity and/or removal from the thesis, knowing that these two actions were feasible. One individual expressed an objection to being identified in this study, but all the others gave me initial consent.

Before submitting the final draft of this thesis, I have sought approval from Rob and Miloš and a few other individuals. Only Rob commented on and approved its content before the submission of the thesis because Miloš was very busy just before my submission. Instead, Miloš trusted that Rob and I would filter any potentially problematic section for him. During the evenings of the fieldwork expedition of the Scottish Pine Project in the first week of September 2015, I discussed with Rob the sections of the thesis he had commented on and made a few corrections on matters of scientific terminology among others. After agreeing on the final version, I sent the thesis to Rob's close collaborators. All except one agreed to be referred to by their real names and gave consent for me to include my description of their involvement in the Scottish Pine Project. The one individual who had previously requested that his identity not be disclosed asked, after reading the draft, to be removed completely from the thesis. Consequently, I edited the thesis draft and my fieldwork notes. As a publicly-funded researcher, I am encouraged to archive my notes so that they are accessible to other people.[86] I have not yet decided if I will release my fieldwork notes. If I decide to archive my notes, I will make all the necessary adjustments so that the identities of this one individual and a few others who have not yet given me their consent are protected. I use pseudonyms for those individuals whom I never asked for explicit permission to mention in the thesis (the reason being that they have a minor role in my argument).

1.5. The Structure of the Thesis

This thesis is structured in two parallel and interlocking narratives. One is an "epistemic narrative" about the production of dendroclimatological knowledge. To a certain extent, this epistemic narrative is similar to the story of scientific development that dendroclimatologists themselves typically tell in their textbooks and presentations. I have sought to be faithful to the dendroclimatologists' sense of order and I have titled each empirical chapter in the same way that dendroclimatologists themselves would refer to the stages of dendroclimatological work. Chapter 2 is called "Fieldwork" and discusses the creation of samples from living trees and preserved wood in the Scottish Highlands during fieldwork. Chapter 3 is titled "Tree-Ring Chronologies" and describes the production of carefully dated data from those wood samples in the form of tree-ring chronologies. Chapter 4 has the name of "Tree-Ring Parameters" and

[86] The UK Data Archive. http://www.data-archive.ac.uk/

outlines the development of a new method of generating climatic data from the parameters of tree growth. Chapter 5 is titled “Standardisation”, which is a term that dendroclimatologists use to refer to the process of cleaning tree-ring data from non-climatic factors so that the resulting data represent as clearly as possible the effects of climate on tree growth over time. Chapter 6 sets out the stage of “Reconstruction” and the establishment of extrapolations of unknown past climates from the cleaned tree-ring series. Chapter 7 is called “Controversy” and outlines the dendroclimatologists’ defence of tree-ring based climate reconstructions (including the Scottish one) as accurate accounts of historical changes in climate in the context of a controversy.

The artefactual chronology of the epistemic narrative - the stages of dendroclimatological work did not occur linearly but rather simultaneously and iteratively - allows me to develop a second “sociological narrative” about the roles of trust and scepticism at each stage of the scientific work.[87] As Rob and Miloš strove to generate a temperature reconstruction from wood samples, they faced different *epistemological conundrums* that they were able to resolve, partly and temporarily, by building up and mobilising trust relations and scepticism. The introduction sections of the following six chapters describe the specific epistemological conundrum and difficulty that Rob and Miloš faced at each stage of their work. The main section of each chapter presents the empirical material of the thesis and describes the work that Rob and Miloš carried out to resolve the conundrum. In writing about the specific work that Rob and Miloš conducted under specific circumstances in the past, and as such it is unlikely to be repeated, let alone become generalised, I employ the present tense. This literary device, called “ethnographic present”, will hopefully allow me to evoke more powerfully in the reader’s mind the epistemological conundrums in the way that *I think* Rob and Miloš experienced them.[88] The last sections of the chapters are an analytical discussion of the empirical data that clarifies the roles of trust and scepticism in resolving the initial conundrum.

The conclusions chapter is a reflection of the trajectories of the epistemic and sociological narratives and the theoretical and empirical implications of these overall narratives.

[87] Paul Atkinson defends, as I do, the need for social scientists to transform the “dense complexity of social life into a linear structure” to make it comprehensible for readers. For Atkinson, the dilemma is: “The more readable the account the more it corresponds to the arbitrary conventions of literary form: the more ‘faithful’ the representation (conventional though it still must be), the less comprehensive it must become”. *Understanding Ethnographic Texts* (Newbury Park; London: Sage Publications, 1992), p. 5.

[88] This writing strategy has been criticised by Joannes Fabian in *Time and the Other. How Anthropology Makes its Object* (New York: Columbia University Press, 1983) for providing ahistorical accounts of social life, but I hope readers will understand that my account is historically situated in the past.

The thesis finishes with a coda that describes the present state of the making of dendroclimatological knowledge of Scotland and the Scottish Pine Project, which brings us back to the field site. The only appendix to this thesis includes one piece of evidence I use in the conclusion chapter.

As I have organised this thesis as a chronological sequence of the cycle of dendroclimatological research, readers can proceed with this thesis as though it were a mystery novel.[89] They can look at the next three pages, which show two graphs representing the dendroclimatological knowledge that Rob and Miloš have created after four years of work, as if they were the last two pages of the novel where the author unveils the culprit. In this way, the reader will be able to observe how the author (me) constructs the story about the making of dendroclimatological knowledge in a logical way.

[89] According to the historian of science Lorraine Daston, this is the way many historians and sociologists of science proceed in explaining historical or contemporary scientific practice. *Canadian Broadcasting Corporation*, "How to Think About Science: Lorraine Daston", 2007, minute 3. http://www.cbc.ca/radio/ideas/how-to-think-about-science-part-2-1.464988

Dendroclimatological Knowledge

"Within the context of the uncertainty, recent warming [in Central-East Scotland] is not significantly greater in relation to other reconstructed warm periods."

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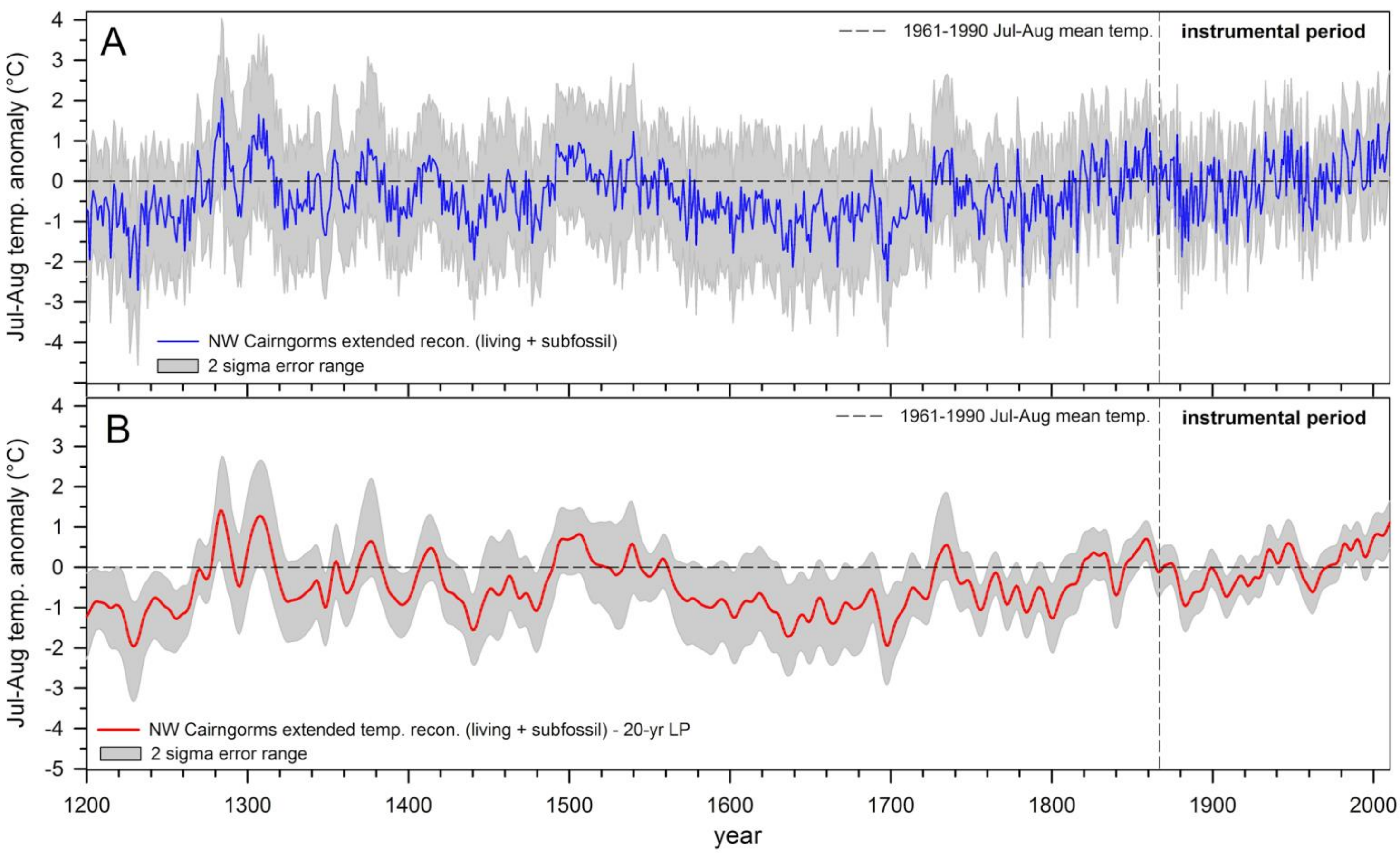
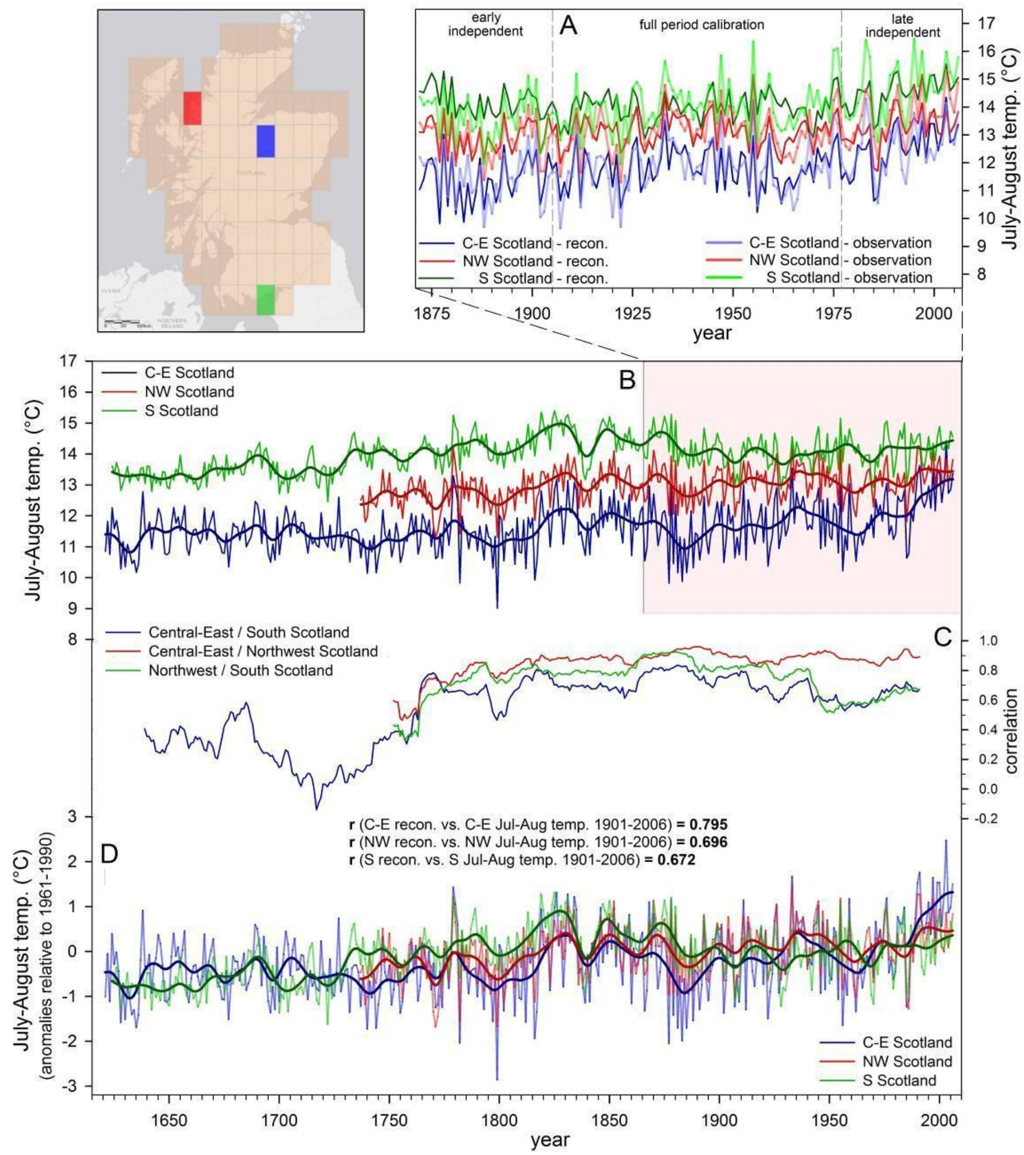


Source: Miloš Rydval, *Dendroclimatic Reconstruction of Late Holocene Summer Temperatures in the Scottish Highlands*, Unpublished PhD Dissertation, The University of St Andrews, 2015, p.118, 131.

"Temperatures are reconstructed as relatively higher from ~1730 until 1900 for the south of Scotland in relation to reconstructions from the two regions farther north, which also exhibit greater overall similarity. Additionally, the late 19th century stands out as a relatively cooler period in the central-eastern Highlands. Although 1799 is reconstructed as the coldest year for the July-August season in the nearly 400 year reconstruction for central-eastern Scotland, this negative departure is less prominent in the northwest version and virtually absent in the southern reconstruction. Furthermore, some differences in trend are apparent particularly in the southern grid reconstruction around the mid-20th century and also in the early and mid-18th century."



Source: Miloš Rydval, *Dendroclimatic Reconstruction of Late Holocene Summer Temperatures in the Scottish Highlands*, Unpublished PhD Dissertation, The University of St Andrews, pp. 94-95.

The Making of Dendroclimatological Knowledge

2 Fieldwork

2.1 The Production of Samples

Every year at the beginning of August, for the last six years in a row, Dr Rob Wilson has been busy putting the finishing touches on the annual fieldwork expedition that his team conduct in the Scottish Highlands. Rob is the leader of the "Scottish Pine Project", a dendroclimatological project with the aim of using Scots pine trees (*Pinus Sylvestris L.*) to reconstruct the environmental and climatic history of Scotland over the last 2,000 years and longer.[90] During fieldwork, the members of the Scottish Pine Project team and other occasional participants - myself included - collect pieces of Scots pine wood from forests, archaeological buildings and lakes across the mountainous region of Northern Scotland from which later on dendroclimatologists will generate knowledge about past climate change in Scotland.

Rob and colleagues give the distinctive scientific status of "samples" to these pieces of wood that they produce during fieldwork.[91] Rob and his team always talk about "collecting" samples as if they had "just" been found passively by dendroclimatologists. Instead, I talk about the "production" of samples to express the very active involvement of dendroclimatologists in producing appropriate samples for dendroclimatological purposes.

To count as a sample in the Scottish Pine Project, the wood has to yield useful information about changes in temperature from year to year, as reflected in the variation in the width of the layers of tree growth (what dendroclimatologists call "tree-rings"). Rob and other dendroclimatologists refer to samples that have variable patterns of tree-rings as being "sensitive" to climate as opposed to "complacent" samples that show a uniform sequence of wide and narrow tree-rings.

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90 R. Wilson, N.J. Loader, M. Rydval, H. Patton, A. Frith, C.M. Mills, A. Crone, C. Edwards, L. Larsson and B.E. Gunnarson, "Reconstructing Holocene climate from tree rings: The potential for a long chronology from the Scottish Highlands", *Holocene*, 2012, Vol.22 (1), pp.3-11.

91 The historian Robert Kohler suggests that the survey expedition is "a kind of scientific instrument" in in *All creatures: Naturalists, Collectors, and Biodiversity, 1850-1950, (Princeton, N.J.: Princeton University Press, 2006), p.137*. Kohler suggests that there is a "narrow" and a "broad" definition of a scientific instrument. The narrow definition refers to the physical instruments of laboratory or fielwork like balances, barometers or microscopes. The broad definition includes ships, museums and fieldwork expeditions that produce cartographic knowledge, taxonomies of science and samples respectively.

In the 1980s, a dendroclimatologist called Malcolm Hughes conducted the first dendroclimatology study of Scotland and discovered that Scots pine trees growing in the mountains of the Scottish Highlands are very sensitive to changes in temperature. Rob and his team, building upon Hughes' discovery, purposively sample this tree species (Scots pine). Rob and his team have also discovered that subfossil Scots pine wood in a few lakes and historical buildings of the Scottish Highlands is also relevant for dendroclimatology purposes. Around two thirds of the samples included in the Scottish Pine Project are from living and standing Scots pine trees and the other third are from preserved and subfossil Scots pine wood from archaeological beams and lakes.

The purposive sampling strategy of the Scottish Pine Project is based on the dendroclimatologists' acceptance of the assumption of the "principle of limiting factors".[92] This principle states that tree growth is predominantly limited by the single environmental factor (either temperature or rainfall) that is in least supply in a certain location. The limiting effect of climatic factors varies from year to year, as one year it may rain more or may be hotter than previous years. The resulting sequence of tree-rings reflects such yearly variations in temperature or rainfall. Trees growing in semi-arid regions are primarily limited by the availability of water, and consequently, they produce narrow rings in drought years and noticeably wider rings in rainy years. Trees growing on high elevations are instead mainly limited by temperature variations, and grow wider annual rings during warm periods and narrower rings during cold ones.

The second principle that Rob and his team accept as a basis for their sampling strategy is the "principle of ecological amplitude", which suggests that trees should only be sampled within their geographical distribution or "ecological amplitude".[93] Depending upon hereditary factors, some tree species have wider or more restricted ecological amplitude. For example, Scots pine trees are known to have wide ecological amplitude because they grow in dry habitats and sandy soils as well as peatland habitats like the Scottish Highlands. Trees growing near the edge of their ecological amplitude or "treeline" are more limited by climate (either temperature or rainfall, depending on the tree species and the location), and show more distinct tree-ring patterns. These are the trees that Rob and his team search in their fieldwork expeditions.

The dendroclimatologists' reliance on the principles of limiting factors and ecological amplitude is expressed in the term "site selection". In the first dendroclimatology textbook written by Harold Fritts and published in 1976, he claims that "dendrochronologists must apply the law of

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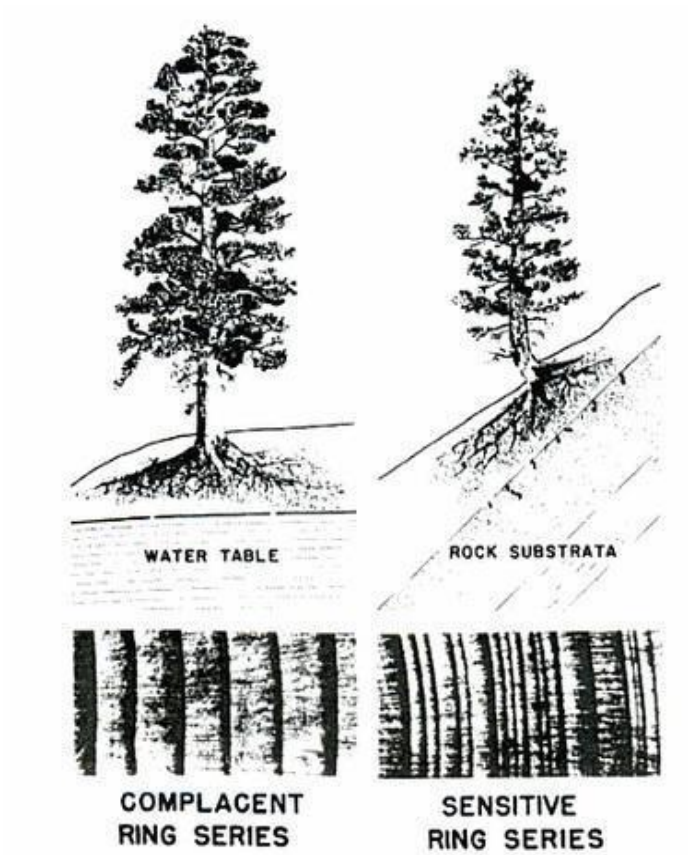
[92] Harold Fritts, *Tree Rings and Climate*, (London: Academic Press, 1976), p.15.
[93] Fritts, *Idem*, p.16.

limiting factors and the concept of ecological amplitude when they obtain their research materials in order to assure selection of trees which will give them the information they desire. This selection is referred to as *site selection*".[94] Fritts illustrates his claim with a drawing of two trees, one growing on a water-saturated ground and the other on a rocky dry slope. Below each tree, Fritts includes a photographic image of their respective "complacent" and "sensitive" tree-rings to illustrate the connection between tree-ring patterns and growing conditions (Image 5). In a more recent textbook published in 2010, Jim Speer defines site selection as a "principle" and explains its purpose in terms of "maximising" the desired (climatic) signal in the sampled trees.[95]

Image 5. With this image, the dendroclimatologist Harold Fritts seeks to justify the practice of site selection in dendroclimatology and the purposive sampling of trees like the one on the right that shows a "sensitive", variable and climatologically relevant pattern of tree-rings.



Source: Harold Fritts, *Tree Rings and Climate*, (London: Academic Press, 1976), p.17.

[94] Fritts, *Idem*, p.17.
[95] James Speer, *Fundamentals of Tree-Ring Research*, (Tucson: University of Arizona Press, 2010), p.21.

Rob has adapted the principle of site selection to the features of the Scottish environment. On the one hand, Rob and his team select those tree species (Scots pine trees) they know through Hughes' work that are most likely to be sensitive and limited by the lack of warm temperatures in Scotland. On the other hand, Rob and his team sample all the few Scots Pine woodlands and lakes where they suspect Scots pine wood might exist. As Rob puts it to me, "in a way, we're not selecting, we are sampling everything we can find that can help us to get the climate signal".

The "site" is both a theoretical concept[96] and a physical place where Rob and colleagues produce their scientific objects, namely dendroclimatological samples. The nature of a site is partly determined by the ecological characteristics of an area and its homogeneity in terms of the type and quality of the vegetation, soil and stand structure.[97] However, the boundaries of a site are not exclusively physical or limited by the features of woodland. The constitution of a site is partly dependent on the structure of the fieldwork expeditions and the logistics of accessing a site and transporting the gear and samples cross-country (in one of the fieldwork expeditions in which I participate, Rob decides not to include a new site because it is inaccessible). Also, the hybrid identity of the site is constituted by the dendroclimatologists' research interests and associated purposive sampling. When I ask Miloš - Rob's main PhD student involved in the Scottish Pine Project- about the number of sites included in the project, he responds that "what actually is a site is arbitrary; it depends on how you've sampled a forest". Miloš explains that he could sample a forest in one place and again in another place and either have two sites or just a single one depending on the "purpose of the sampling". Miloš tells me, "I would say we have almost fifty sites in Scotland, but it could also be a hundred".

Since the start of the Scottish Pine Project in 2006 (when Rob obtained the first of the two public research grants for the project), Rob's main aim has been to expand the geographical

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[96] A site is a "natural kind" as defined by the sociologist Barry Barnes in "Social Life as Bootstrapped Induction", *Sociology*, 1983, Vol. 17 (4), pp. 524-545. Unlike most philosophical accounts, which define natural kinds as solely reflecting the structure of the natural world (for instance, see the entry for "Natural Kinds" in Alexander Bird and Emma Tobin, "Natural Kinds", *The Stanford Encyclopedia of Philosophy* (Spring 2015 Edition), Edward N. Zalta (ed.), accessed 15 July 2015, http://plato.stanford.edu/archives/spr2015/entries/natural-kinds/), Barnes acknowledges the input of socialised humans in perceiving and labeling natural kinds.

[97] Glossary of Dendrochronology, "Site" entry. Swiss Federal Institute for Forest, Snow and Landscape Research WSL http://www.wsl.ch/dienstleistungen/produkte/glossare/dendro_glossary/Details_EN?id=274&language=English

distribution of sites and the number of samples. Through published historical sources[98], Rob has learnt that due to deforestation events since the Romans in Northern Scotland, only about 1% of the original area in the Scottish Highlands covered by Scots pine woodland during the mid-Holocene remains. Rob's ambition is to conduct an exhaustive sampling of this 1% of remnant pine woodlands in Scotland. On the website that Rob has created to publicise the Scottish Pine Project, he states, "Long term plan is to sample all remaining semi-natural pine woodlands in Scotland. We are almost there."[99]

To identify all the remnant pine woodlands in Scotland, Rob has enlisted the help of a government forester, Colin Edwards from the Forestry Commission, who is inventorying them. These inventories have shown that most Scots pine trees in Scotland are part of modern plantations with an average age of 225 years. For the purpose of creating a long temperature reconstruction for Scotland, Rob is interested in finding samples from old Scots pine trees, as reflected by the age of the rings when the tree lived. Rob likes to tell the story about how he discovered the oldest remnants tree (not the oldest living tree) in Scotland in autumn 2008. He was walking with his son around Loch an Eilein in the North-East of Scotland when Rob saw some logs emerging on the banks that looked to be from Scots pine trees. He asked his son ("who used to play rugby", Rob emphasises) to help him scout the submerged log. A year later, Rob returned to the same place for sampling the submerged log for carbon dating. The experts in the carbon dating laboratory told Rob that this log was very old (8,000 years). Since that day, Rob has prioritised the sampling of submerged and historical wood in a few lakes of the Scottish Highlands. As Rob states on the website of the Scottish Pine Project, the second objective of the project is "to extend living [tree-ring] chronologies using extant, historical or sub-fossil tree-ring material".[100] To identify and sample Scots pine wood preserved in lakes and historical buildings across Scotland, Rob works with various colleagues, students, and amateurs like me who participate in the Scottish Pine Project fieldwork expeditions.

For the last nine years, Rob and the Scottish Pine Project team have been building up a "network of sites" that they see as representing the geography of the Scottish Highlands. In May 2014, Rob sends me an email with two maps and the comment "this is where we were in May 2006

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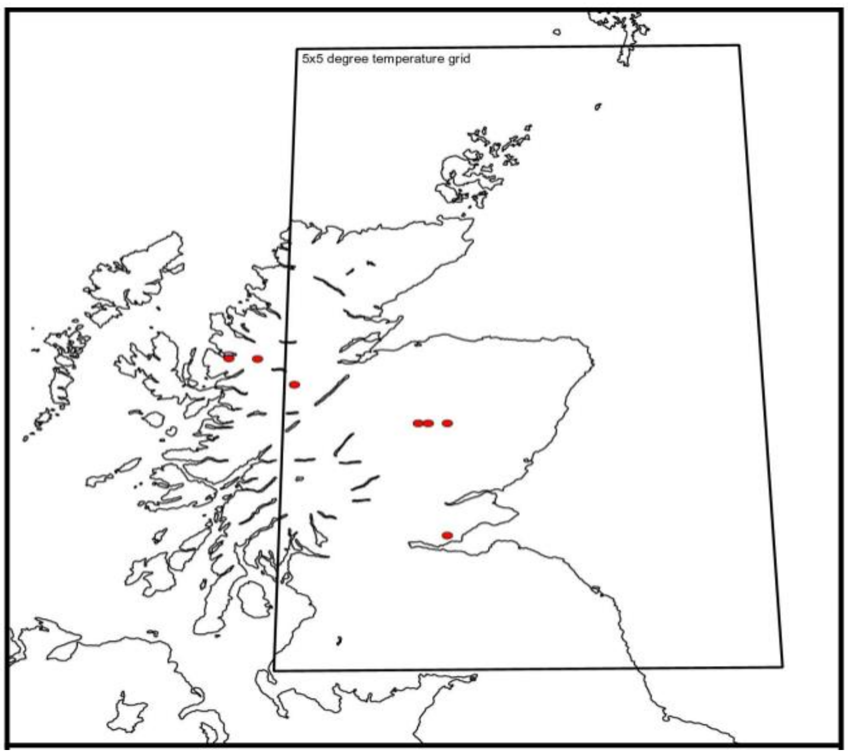
[98] Christopher Smout, Alan R MacDonald, and Fiona Watson. *A History of the Native Woodlands of Scotland, 1500-1920, (*Edinburgh: Edinburgh University Press, 2005).

[99] "Living Tree Ring Chronologies", *Scottish Pine Project* website, accessed 15 July 2015, https://www.st-andrews.ac.uk/~rjsw/ScottishPine/living.html

[100] "Project aims", *Scottish Pine Project* website, accessed 15 July 2015, https://www.st-andrews.ac.uk/~rjsw/ScottishPine/index.html

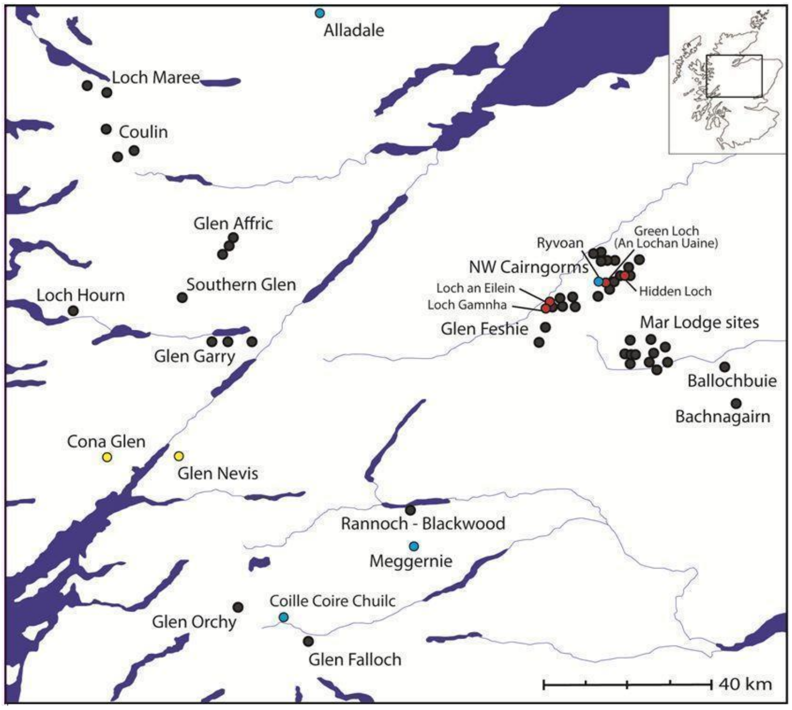
and this is where we are now". One map has seven dots, representing the sites that Malcolm Hughes sampled to create the first Scottish temperature reconstruction as published in the journal *Nature* in 1984.[101] Rob used these sites as a starting point in 2006 when he initiated the Scottish Pine Project. The other map that Rob sends me has 44 dots, and includes the names of all the new sites that Rob and his team have been able to sample to date (Image 6).

Image 6. With these two maps of the Scottish Highlands, Rob represents the progress in the number and geographical representativeness of the samples of the Scottish Pine Project since 2006. The first map (a) represents the starting point of the Scottish Pine Project, with a few sites (or dots) sampled by Malcolm Hughes in the 1980s. The second map (b) represents the existing "named" sites of the Scottish Pine Project in May 2014 with living trees (black dots), submerged trees in lakes (red dots) and prospective sites (yellow dots).



(a)

[101] Malcolm K Hughes et al. "July– August Temperature at Edinburgh between 1721 and 1975 from tree-ring density and width data", *Nature*, 1984, 308, pp. 341–343.



(b)

On the website of the Scots pine Project, Rob has listed all the information about the sites in a table.[102] The columns include the name of the site, which is related to the area in the Scottish Highlands from where the samples are generated; a three-letter acronym of the name of the site (for instance, the site of Glen Affric is abbreviated as GAF); the latitude, longitude and elevation coordinates that locate a site on a map; the year or age of the oldest sample and youngest samples found at the site; and the number of samples generated at the site, which ranges from 16 to 179.

The Scottish Pine Project includes the samples that Malcolm Hughes generated back in the 1980s. For instance, the North-Western site of Glen Affric has the highest number of samples (179) because it includes Hughes' data. Rob does not have access to Hughes' material samples, but Rob and everybody else can download the tree-ring data that Hughes generated from the "International Tree-Ring Data Bank" (ITRDB). The ITRDB is a publicly available archive of tree-ring data, which dendroclimatologists describe as a service to "the entire global scientific community".[103] Rob is proud to have been a previous "contributor" to this communal project and is planning to contribute again with the Scottish dataset. This is why the table that Rob has included on the

[102] "Living Tree Ring Chronologies".
[103] Henri Grissino-Mayer and Harold C Fritts. "The International Tree-Ring Data Bank: an enhanced global database serving the global scientific community.", *The Holocene*, 1997, Vol. 7 (2), pp. 235-238.

website of the Scottish Pine Project with the details of the sites also has an empty column for the "ITRDB code" that Hughes initiated.

The sampling design of the Scottish Pine Project is not only purposive but also iterative. Once Rob and his team select a suitable site, they often sample it more than once, especially when they find out later in the laboratory that trees in this site are particularly sensitive to climate. This is the case with the two lake sites - Loch an Eilein and Loch Gamhna - which Rob and his team have sampled on three consecutive fieldwork expeditions. When creating replicate samples or multiple samples from the same tree and site, Rob and his team uphold the "principle of replication". [104]

Rob claims to be a "great believer" in the principle of replication. This principle states that the climate signal that dendroclimatologists assume exists in trees can be "maximised" by averaging the data from replicate samples. The dendroclimatologists' expectation is that if the climate is strongly limiting the growth of trees over a geographical area (as the principle of limiting factors and ecological amplitude suggest), the data from all replicated samples within and among sites will show approximately the same ring-with variation, which they regard as evidence of environmental information. Dendroclimatologists believe that, if there is a climate signal in trees, this will become clearer if they increase the number of samples (Chapter 5). Rob also insists that there is also a "saturation point" and at some point the climate signal will not improve even if they generate more samples and data.

Dendroclimatologists acknowledge that the purposive selection and replication of sensitive trees raises questions about whether the resulting samples are truly representative of the wider natural world from where they are taken.[105] In a car conversation I have with Miloš on our way to St Andrews, he admits, "some people accuse us [dendroclimatologists] of biasing our sample". In his textbook, the dendroclimatologist Harold Fritts refers to a paper in which the author criticises dendroclimatologists for not using random sampling. Fritts responds to this critic by stating "such

[104] Fritts, *Tree-Ring and Climate*, p.23.

[105] In her historical study of a controversy in primatology, Amanda Rees refers more generaly to the problem of establishing judgements of similarity between the peculiarity of a site and the universality of the unknown natural world as the "fieldworker's regress", *The Infanticide Controversy: Primatology and the Art of Field Science*, (Chicago, IL: University of Chicago Press, 2009), p.4.

judgement fails to recognise that the dendrochronologist[106] has a particular strategy in mind which requires that his samples be affected similarly by a given set of growth-limiting factors".[107]

During the dendroclimatology training course I attend in Tasmania, the dendroclimatologist Edward Cook defends the inevitability of purposive sampling when I ask his opinion of the accusations of biased sampling directed at dendroclimatologists. Cook responds by telling me about an "incident" that happened to him at a conference in the early stages of his career. Cook explains that in the audience there was a "high level stats person" who asked Edward Cook if he had ever randomly sampled his trees. Cook replied to the statistician that "he had never sampled a tree randomly in his life" and he remembers the statistician replying "So, you never had a degree of freedom". "Degrees of freedom" is a conventional technique employed by statisticians among others who seek to certify the reliability of inferences about a larger population from a sample. When the statistician claimed that Cook did not have a degree of freedom, she was effectively accusing his dendroclimatological conclusions of being unfounded. Cook interprets the statistician's reaction as being the result of a different training and scientific tradition than that of dendroclimatologists. Cook says, "You see, this is the type of pure statistician response as classically considered with an experiment design with random sampling and control; that was the way she was educated". Cook concludes that "if you are doing a climate reconstruction, and time matters, we must try to select the oldest and most sensitive trees. There's no way around it". Rob is also particularly forceful about the inevitability of purposive sampling when he says that "you would not go to the tropics to study glaciers!"

Whilst Rob and other dendroclimatologists are convinced that purposive sampling is the most "efficient" and appropriate strategy for dendroclimatological projects (as Cook states "There's no way around it"), they have also been examining the possibility that this sampling strategy has some limitations. In particular, the dendroclimatologist Thomas Melvin has recently formulated the "Modern-Sample Bias",[108] a bias that is seen to arise from sampling old trees. Melvin has discovered that this practice might distort climate reconstructions over the modern period (hence the name of the bias).

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[106] I talk about the difference between dendrochronologists and dendroclimatologists in the next chapter. Essentially, the difference is that dendroclimatology (the study of climate from trees) is considered an "application" of dendrochronology (the creation of chronologies from trees).
[107] Fritts, *Tree Rings and Climate*, p. 17-18.
[108] Thomas Melvin, Hakan Grudd and Keith Briffa. "Potential Bias in 'Updating' Tree-Ring Chronologies Using Regional Curve Standardisation: Re-processing 1500 years of Torneträsk Density and Ring-Width Data." *The Holocene*, 2013, Vol. 23.(3), pp. 364-373.

The identification of the Modern Sample Bias is triggering other dendroclimatologists to examine the magnitude of this and other biases associated with sampling. At the international tree-ring conference I attend in Melbourne in January 2014, one of the most commented on presentations among the attendees whom I talk to[109] is by David Frank and his team of students working in Switzerland about the effects of different sampling strategies.[110] Dendroclimatologists more generally are afraid that differences in sampling strategy in the archived data could bias "follow-up" dendroclimatological studies like the Scottish Pine Project that rely partly or extensively on archived tree-ring data. Available tree-ring data in the ITRDB have been generated by researchers who had other purposes and employed sampling strategies other than dendroclimatological. For instance, these researchers may have generated samples in order to date archaeological and historical buildings or to determine the age of trees for forest management. In the case of the Scottish Pine Project, as Malcolm Hughes generated his samples with the purpose of producing a temperature reconstruction, Rob has never expressed any concern about a bias in the archived data. Whilst for some critics of dendroclimatology, the Modern Sample Bias renders archived tree-ring data largely useless[111]; dendroclimatologists are currently developing solutions to the Modern Sample Bias. Rob and the Scottish Pine Project team share in the concerns of their community, insofar as they all acknowledge that biases associated with purposive sampling need to be acknowledged, quantified and addressed.

The epistemological conundrum that Rob and other dendroclimatologists face at this stage of the creation of a climatic reconstruction is to ensure that despite the known limitations of

[109] One of the participants to whom I talked at the conference was Jim Speer, who days later wrote a blog entry on his *Dendrosabbatical* about the conference talks he thought had been most interesting. "There were many excellent presentations during the conference. A few stood out to be very interesting to me. We actually had a sociologist named Meritxell Ramirez-Olle who was studying Rob Wilson from St. Andrews University in Scotland. She was examining how dendrochronologists conduct their research, interact with students, and develop their ideas. Another presentation examined the effect of sampling design on climate response, climate reconstruction, and biomass calculation. David Frank and others had completed a 100% sample of a half hectare plot. Then they subsampled their data based on targeted sampling, different area plot sampling, and random sampling. They found some bias in response from targeted sampling in biomass calculation, but not in climate response". "WorldDendro Conference in Melbourne", 18 January 2014, http://dendrosabbatical.blogspot.co.uk/2014/01/worlddendro-conference-in-melbourne.html.

[110] The presentation I heard at the conference was eventually published in a paper: Christoph Nehrbass et al., "The Influence of Sampling Design on Tree-Ring-Based Quantification of Forest Growth." *Global Change Biology*, 2014, Vol. 20 (9), pp. 2867-2885.

[111] Jim Bouldin, "Sever analytical problems with dendroclimatology, Part One, *The Ecologically Orientated,* accessed 15 July 2015 https://ecologicallyoriented.wordpress.com/2012/11/10/severe-analytical-problems-in-dendroclimatology-part-1/

purposive sampling, their chosen strategy nonetheless produces the most reliable samples in order to create a sound starting point for making knowledge of past climates. For scientists in many other disciplines, the best way of avoiding bias is seen to be the adoption of random sampling, thereby eliminating any possibility that they are selecting samples that reflect their preferred point of view. Rob and other dendroclimatologists like Edward Cook are adamant that this strategy is not viable when sampling trees for climate records. Consequently they have to adopt other strategies to satisfy themselves and their colleagues that their samples are untainted by bias. The following empirical section sets out the material for elucidating those strategies that Rob and members of the Scottish Pine Project employ to resolve this conundrum.

This chapter describes the work that Rob and his team carry out during fieldwork in order to produce credible samples. More generally, this chapter is concerned with characterising the science that it is done in the field, as opposed to or in relation to the science that is done in the laboratory.[112] The following empirical section is thematically structured around a typical day of fieldwork in the Scottish Pine Project fieldwork expedition in the years 2012 and 2013. The purpose of this description is to illustrate the experience of fieldwork and the intellectual and emotional elements involved in the creation of dendroclimatological samples.

2. 2 the Scottish Pine Project Fieldwork Expedition

2. 2. 1. The Social Division of Fieldwork

In May 2012, a couple of weeks after I first approach Rob and Miloš with the idea of doing a sociological study of their work, I receive an email from Rob inviting me to participate in the Scottish Pine Project fieldwork expedition that will take place in August 2012. I receive a similar email from Rob a year after, as part of the preparations for the second fieldwork expedition in which I take part. This second invitation comes as less of a surprise. By the time I receive Rob's email, I have already "reserved" these dates in my diary. As fieldwork has previously taken place in the last week of August and first week of September (because of milder weather conditions and permission restrictions), the other fieldwork participants tell me that they have also organised their working year accordingly.

[112] For a historical examination of the distinction between the laboratory and the field in the specific case of biology, see Robert Kohler, *Landscapes and Labscapes: Exploring the Lab-Field Border in Biology* (Chicago: University of Chicago Press, 2002).

The setting up and running of the Scottish Pine Project fieldwork expedition depends on a substantial amount of effort in distributing responsibilities among participants. As the leader of the Scottish Pine Project fieldwork expedition, Rob is in charge of arranging people and their associated duties: to agree on an exact timetable for the expedition; to apply for and secure funding to cover the trip's expenses; to prepare all the necessary equipment; to request access to the sampling areas to landowners and to government agencies; to fill in the safety and insurance forms; and to find accommodation and travel for all fieldworkers.

The membership of the Scots pine Project fieldwork expedition is eclectic and changes slightly from year to year. In the first fieldwork in which I participate there are four people (including me); the year after, the same four people are joined by seven new people. Dr Björn Gunnarsson is, like Rob and Miloš, part of the regular team during the two expeditions I take part in. On the first day of my first fieldwork week, I accompany Rob and Miloš to pick up Björn at Edinburgh Airport. Meeting a fellow fieldworker at the airport is in accordance with the mood so characteristic of this location. Rob and Miloš are excited to reunite with their colleague and friend whom they usually only meet once a year during fieldwork as Björn lives in Stockholm (Sweden).

Rob considers Björn to be part of the "core" team of the expedition and the Scottish Pine Project more generally. Rob met Björn on a European project in early 2006 and since then has included him as "project partner" in the two funding applications for the Scottish Pine Project. Björn is an associate professor and head of a dendroclimatology laboratory at Stockholm University. He is famous for producing one type of tree-ring data ("density data"; see Chapter 4) and co-authoring a long temperature reconstruction of Scandinavia with his colleague Dr Hans Linderholm, a professor and head of a tree-ring laboratory in Gothenburg University. Indeed, Hans also participates in the second expedition I attend in August 2013. The same year, Björn is also accompanied by whom I refer as "Emily", one of his postgraduate students in Sweden, who is collaborating with Miloš on an interlaboratory experiment (Chapter 4).

Rob welcomes the fieldworkers' partners and amateurs like me. The year before I first joined the Scottish Pine Project fieldwork expedition, Miloš' girlfriend also accompanied them. For my second expedition, Emily brings her boyfriend because they are both planning to travel with their campervan around the Scottish Highlands after the expedition. In general, the participation of non-scientists seems to be one of key features of many field-based sciences.[113]

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[113] Henrika Kuklick and Robert Kohler, "Science in the Field", *Osiris,* 1996, Vol. 11, p. 4. The entire special issue was reprinted as *Science in the Field* (Chicago: University of Chicago Press, 1996).

As the other fieldworkers work and live in Scotland, we meet them directly at the cottage that Rob has rented in Aviemore after picking up Björn at the airport. In Aviemore, we meet whom I will refer hereafter to as "Leah", an independent researcher who uses trees to date archaeological buildings and to investigate the cultural heritage of Scotland. Before Rob started doing research in Scotland, Leah and a colleague of her were the only dendrochronologists in Scotland. Leah's participation in the Scottish Pine Project is as a postdoctoral research fellow. The other senior scientist that participates in the expedition, I refer to him as "Stewart", is like Rob, senior lecturer at the Geography Department in St Andrews University. Stewart is the only member of the expedition (besides the amateurs) who has no expertise in trees. He is an expert in sonar survey techniques; during fieldwork he brings his own team, who help him locate submerged logs in the lake. I do not meet Stewart or his team on either of the two expeditions in which I take part. In fact, Rob is the only person that knows Stewart and his team, as they conduct their fieldwork a day before we all arrive in Aviemore. At the cottage house, we also meet Rob's PhD student and technician, whom I call "Anne", who has driven from St Andrews with a pickup truck that Rob has hired to transport all the samples and gear.

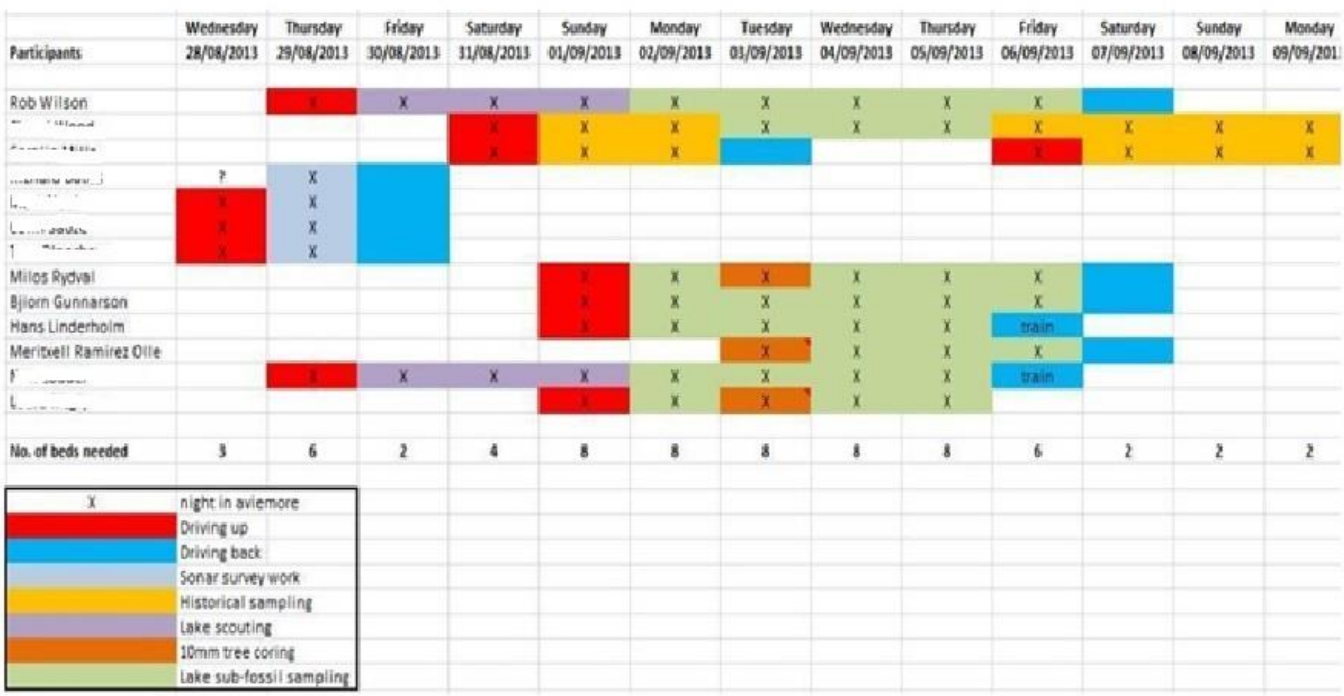
Although Rob does not include doctoral students as "project partners" on the website of the Scottish Pine Project, he considers Miloš, in particular, a crucial contributor to the project and to the fieldwork expedition. "The Scottish Pine Project would not have advanced this far without Miloš", Rob admits. In 2008, after Miloš had graduated in Geography at St Andrews (where Rob supervised Miloš' dissertation on the use of a forest in his native Czech Republic to assess the impact of sulphur dioxide pollution on tree growth) and had worked as a technician for the project in its early days, Rob asked Miloš if he wanted to become involved in the Scottish Pine Project as a PhD student. Rob describes Miloš as "one of the best undergraduate students I have ever supervised for a 4th year dissertation". Miloš accepted Rob's offer as, "I knew that Rob would be a great supervisor and I had enjoyed working with him as a technician". Miloš was also familiar with the Scottish Pine Project as a technician and thought it was an exciting topic. In 2009, Rob and Miloš applied for, and successfully obtained, a PhD studentship from a private foundation (The Carnegie Trust). Essentially, in his thesis, Miloš uses the samples generated by the Scottish Pine Project team during the period of his doctorate to extend and update Malcolm Hughes' climate reconstruction of Scotland.

Miloš understands his thesis as a "temporary" contribution to the Scottish Pine Project. He explains "My PhD is part of Rob's longer-term project and I am just helping him out at this stage".

Rob conceives Miloš' contribution as almost like a collaboration between equals as "our relationship has been more on par as colleagues rather than the normal student/supervisor." Anne also enjoys a similar collegial relationship with Rob, although her PhD topic is not related to the Scottish Pine Project. Rob was Anne's second supervisor and at the time of my second expedition, she has just been hired by Rob as a technician for the Scottish Pine Project.

Essential to the task of coordinating fieldwork is the division of tasks and people into different sub-teams. The social division of fieldwork is reflected in the timetable that Rob sends a few days before the start of the second fieldwork expedition in August 2013 where participants are distributed by days, teams and "number of beds needed" (Image 7).

Image 7. This timetable, produced by Rob, shows the degree of social coordination and division of fieldwork tasks carried out by fieldwork participants.



Participants	Wednesday 28/08/2013	Thursday 29/08/2013	Friday 30/08/2013	Saturday 31/08/2013	Sunday 01/09/2013	Monday 02/09/2013	Tuesday 03/09/2013	Wednesday 04/09/2013	Thursday 05/09/2013	Friday 06/09/2013	Saturday 07/09/2013	Sunday 08/09/2013	Monday 09/09/201
Rob Wilson		X	X	X	X	X	X	X	X	X			
[illegible]				X	X	X	X	X	X	X	X	X	X
[illegible]				X	X	X				X	X	X	X
[illegible]	?	X											
[illegible]	X	X											
[illegible]	X	X											
[illegible]	X	X											
Milos Rydval					X	X	X	X	X	X			
Bjiorn Gunnarson					X	X	X	X	X	X			
Hans Linderholm					X	X	X	X	X	train			
Meritxell Ramirez Olle							X	X	X	X			
[illegible]		X	X	X	X	X	X	X	X	train			
[illegible]					X	X	X	X	X				
No. of beds needed	3	6	2	4	8	8	8	8	8	6	2	2	2

X	night in aviemore
	Driving up
	Driving back
	Sonar survey work
	Historical sampling
	Lake scouting
	10mm tree coring
	Lake sub-fossil sampling

Sub-teams work independently from each other, each under the supervision of a different member of the expedition who is an expert in one specific aspect of fieldwork. Stewart is responsible for the "lake sonar survey team" whilst Leah is in charge of the "historical sampling team" with Anne. At the end of the day or the fieldwork week, both Leah and Stewart report their results to Rob. This form of reporting does not take place with the other two fieldwork sub-teams, as Rob is a member of both. Björn, Hans, Rob and Miloš are part of the "lake sampling" and "lake scouting" teams. Björn and Hans have extensive experience in sampling submerged wood in lakes in Sweden, and Björn is the only person with an official licence to use a chainsaw. Postgraduate students like Miloš and Emily and amateurs like Emily's boyfriend and me are part of the lake and living trees sampling teams.

Rob's ability as a leader and coordinator of the Scottish Pine Project lies in "translating" and formulating his research interests as an opportunity for collaborators to pursue their own respective research interests.[114] In this way, besides the common goal of generating samples that can be used to create a temperature reconstruction of Scotland, each fieldworker has a specific interest in the wood they collect. Stewart's interest in finding subfossil wood lies in refining his sonar survey system. Leah seeks to use preserved wood to continue her exploration of the cultural heritage of Scotland. Björn hopes to use the Scottish data to complement his own climate reconstruction of Scandinavia and to maximise his laboratory facilities with the experiments that Miloš and Emily are conducting.

2.2.2 The Moral Economy of Fieldwork

The production and consumption of dendroclimatological samples (and the data and the knowledge that dendroclimatologists generate from them) is regulated by a set of tacit norms of social organisation regarding access to the field site; authority over research networks and information, and allocation of rewards. In the "Moral Economy of Fieldwork", fieldworkers produce samples in exchange for labour, food, accommodation, information and data on the basis of a set of moral rules, which specificy what constitutes good and bad behaviour towards others.[115] The production of samples, unlike a market economy, is not an explicit exchange of samples for money or another commodity. Instead, samples are more similar to gifts in the sense that dendroclimatologists

[114] Michel Callon and John Law, "On Interests and Their Transformation: Enrolment and Counter-Enrolment", *Social Studies of Science*, 1982, Vol.12 (4), pp. 615-625.

[115] Drawing on Edward Palmer Thompson's work, many sociologists and historians of science use the concept of "moral economy" to refer to systems of distribution and exchange that are not market economies. (See Kohler in "Moral Economy, Material Culture and Community in Drosophila Genetics." in *The Science Studies Reader* (New York ; London : Routledge, 1999).

produce samples with the implicit understanding that their effort will be reciprocated.[116] As the leader of the Scottish Pine Project fieldwork expedition, Rob defines the framework of rules upon which these exchanges occur and the project develops. He refers implicitly to the morality of these exchanges when he tells me that "Basically, I share with everyone who deserves it".

Rob employs access to samples and the data generated from samples as a form of reward for his colleagues' voluntary workforce. This idea becomes clear to me when I ask Rob why Hans is participating in my second expedition. Among other reasons (one being "meeting with his good friends Björn, Rob and other members of the Scottish Pine Project"), Rob responds "Hans is interested in using the Scottish data to study the Summer North Atlantic Oscillation, a large scale pattern of climate variability". From April 2013, this collaboration has become more formal as Rob included Hans as project partner in the second funding application. Likewise, Rob also gives priority access to the Scottish data to other two researchers: one researcher participated in the Scottish Pine Project fieldwork a few years ago and the other one is a long-time collaborator of Rob. Rob is waiting for Miloš to finish his thesis before "releasing" the Scottish data and archiving the Scottish data to the ITRDB. In this way, Rob rewards Miloš with a temporary "monopoly" over the use of the Scottish data, to which Miloš has extensively contributed during his doctorate.

Unlike with the other fieldworkers, the relationship between Rob and Leah is based on a contractual obligation and not a gift-exchange as Leah is a research fellow in the Scottish Pine Project. Rob allows Leah to keep ownership of the material samples that she has generated on her own (with occasional help from Anne). When I ask Rob if he has ever discussed with Leah who should keep the preserved wood from historical buildings, he confirms the explicit nature of their exchange and adds "Anyway, my interest is in the data and not so much in the samples".

Rob has another reward system in place for undergraduate students who often participate in fieldwork expeditions as part of their dissertation projects. The type of dissertation projects that Rob assigns to undergraduate students are always related in one way or another to the Scottish Pine Project. Instead of granting students access to the data or to the samples they generate, Rob sometimes rewards those students whom he understands have done a particularly good job with

[116] W. Hagstrom talks about "Gift Giving as an Organizing Principle in Science" in *Science in Context: Readings in the Sociology of Science*, (Milton Keynes: Open University Press, 1982), p. 29. The anthropologist and sociologist Marcel Mauss defines a gift like "the present generously given even when, in the gesture accompanying the transaction, there is only a polite fiction, formalism, and social deceit, and when really there is obligation and economic self-interest" in Marcel Mauss, *The Gift: Forms and Functions of Exchange in Archaic Societies*, (London: Cohen & West, [1950] 2002), p.4.

article co-authorships. In publications where Rob uses the data generated by undergraduate students, he includes them as co-authors. One of the students that Rob includes as a co-author tells me she feels pleased by "Rob's generosity".

Undergraduate students understand that producing samples for their dissertations is part of their "duty" as students, and they regard Rob's gift in the form of article co-authorships as an act of "generosity". Rob insists that all his undergraduate students must join him on a fieldwork expedition before starting their dissertations. "It is important that they see for themselves where the data they'll generate comes from", Rob explains. For one of Rob's student whom I call Maria, the opportunity to become involved in the production of samples is an incentive to commit to her dissertation project. She says, "If it hadn't been for the effort I put in collecting these samples, I think I would have lost interest in the project altogether". For Chloe, another Rob's undergraduate students who eventually worked as a technician and participated as a "paid" fieldworker, being involved in Rob's project is not only a gift, but a "privilege". Chloe tells me, "I have a great sense of pride working as his technician and attaching my name to his in a way because I'm confident that he is an excellent, thorough and well respected researcher".

Money is also a currency that Rob employs to compensate fieldworkers for their free labour, to the extent that the fieldwork expedition team represents a sort of "financial union". Before Rob achieved funding for the Scottish Pine Project, Björn used his own funds to cover for the costs of travel and accommodation from Sweden. At the time of my participation (August 2012 and August 2013), Rob has achieved two different grants to cover for fieldwork costs and the salary of one technician (Anne). Therefore, whilst Rob does not pay fieldworkers for their work, he uses the "fieldwork budget" to cover the costs of their accommodation and for all. Rob also pays for the travels costs of the official partners in the funding application, but he does not pay for the travel costs of Emily, her boyfriend and I, and in this way he establishes distinctions between the "core" fieldworkers and occasional participants.

Rob has also established a system of exchange with government officials and the estate landowners, who allow the Scottish Pine Project team continued access to the protected areas and private estates where the sampling sites are located. In the two expeditions in which I participate, we mainly work in and around the forests and lakes in Rothiemurchus; the main Estate of the Cairngorms National Park located about 5 km (3.1 miles) south of Aviemore. In exchange for more permissive access to the sites, Rob writes annual reports on the conditions of the woodlands and participates in talks organised by the Rothiemurchus Estate landowner.[117]

The relationship of courtesy that Rob has established with landowners is the source of his sense of responsibility towards the field sites and their owners. In one of the many conversations we have while walking towards the site, Rob tells me about a group of dendroclimatologists who went on a fieldwork expedition to a foreign country and did not contact the local scientific team that was conducting dendroclimatology research there. Rob explains that he would feel "offended" if other dendroclimatologists did the same in Scotland. "While everybody is free to come and exit Scotland", Rob explains, "It would be discourteous not to tell me anything".

2.2.3 The Ethos of the Heroic Fieldworker

The timetable of a day of work at the field site is similar to a working day in Rob's office or in the tree-ring laboratory in St Andrews (roughly from 9am to 5pm). The main difference is that fieldwork involves hard physical labour. The hardship associated with fieldwork becomes clear to me during my first breakfast with the team when Rob advises me that I should change the normal low-caloric breakfast I usually have every morning in Edinburgh if I want to survive the day of fieldwork: "Hey, I don't want your supervisors to accuse me of mistreating you. Eat something more substantial or your energies will drain in the field". Rob then offers me porridge, bread, chocolate, jam, and biscuits. After breakfast, each fieldworker prepares a lunch box with a couple of sandwiches and cereal bars to ingest as fast as possible in the forest while avoiding being bitten by the Highland midges (the small flies that are characteristic of the Scottish Highlands from late spring to late summer). These insects make our fieldwork difficult. We have to wear nets to avoid their bites, but doing so is uncomfortable and reduces our ability to see things around us. Even though the weather is mostly pleasant (as we all expect for this time of year), light rain sometimes falls, making our fieldwork more miserable.

Adverse weather conditions and midges are only collateral elements of the main strenuous activities of the day: getting to the site, extracting wood from the trees or logs, and returning the samples, boxes and equipment to the pickup truck. On my first day of fieldwork - as I sit comfortably in the car that Rob is driving to bring us to Loch Gamhna and Loch an Eilein on the

[117] On the website of the Scottish Pine Project in the "publications" section there is a list of the eight reports prepared by Rob: http://www.st-andrews.ac.uk/~rjsw/ScottishPine/publications.html

Rothiemurchus Estate - I feel relieved that we now have car access to the lakes. Rob tells me that before he had established a courtesy system with the estate owners, fieldworkers had to walk a minimum of one hour a day to get to the site. Nowadays, we do not walk more than ten minutes from the place where we park the pickup car to the lakes. However, when we sample living trees in forests we often need to walk five or six hours a day to enter and return from these sites. Sampling living trees growing on slopes requires considerable trekking skills. As Rob is an avid mountain runner, he looks physically prepared and motivated for the fieldwork hikes. Those who struggle to hike up mountains often joke that they "should have gone to the gym" to prepare for fieldwork. I feel that doing fieldwork is like doing some sort of team sport, and we all wear sports or hiking clothes and footwear that allow us to do our work in the site in a practical manner.

Fieldwork has its own implicit rules of attire that set this activity aside from the aesthetic norms of everyday life. As in any other social activity where we use uniforms to display and recognise professional groups, the sports clothes we all wear during fieldwork identify us as part of a same group. All fieldworkers express an attitude of disinterest with regards to our physical appearance. None of us seem to care whether or not we are dirty, stink or look terrible in the field. I believe that if I had ever taken care of my physical appearance (by wearing makeup, perfume or high heels to go to the field), my fellow fieldworkers would have thought that my attitude was inappropriate. Outside the field, when we are at home during dinner or when we occasionally go out to the pub for a meal or a drink, most of us dress "normally" again. We wear shirts and jeans; we are all washed, perfumed and well-groomed. I remember being surprised the first evening I saw my colleagues not wearing fieldwork clothing. Someone made a joke to Björn about how "classy" he looked in his leather boots in comparison to the orange plastic boots he wears in the field.

Despite all the physical hardships, we all seem to appreciate the fact that the walks to and from the site are an opportunity to socialise and to have fun together. We get to know each other a bit more and we talk about different aspects of work and our personal lives in natural surroundings that we all agree are stunning. Stereotyped comments about Scottish scenery and history are commonly heard during these walks, especially when Rob mentions that the 17th century Scottish literary hero,[118] Rob Roy McGregor, lived somewhere near Loch an Eilein where we do most of our lake sampling. To me, these daily walks do not feel very different from the ones I often take with friends and family. The significant difference is that the fieldwork walks have a purpose other

[118] The Scottish writer Sir Walter Scott published "Rob Roy" in 1818, which has been the basis for multiple novels and films about Rob Roy; the last one, in 1995, starred Liam Neeson and Jessica Lange

than celebrating and constituting bonds of friendship among researchers. Fieldwork is about producing samples, and consequently, during our walks to the site, Rob often reminds us of the "target" and the number of samples he expects to "collect" on that day.

Fieldworkers state that fieldwork expeditions in dendroclimatology are relatively simple and affordable in comparison to other disciplines in paleoclimatology, like ice-core analysis.[119] They describe the "modesty" of the equipment used in dendroclimatology as an advantage in terms of allowing dendroclimatologists to sample trees more flexibly. As a result, fieldwork almost becomes like a hobby and an activity that blurs with their personal lives. Rob tells me about one dendroclimatologist who is known to go on holiday with a hollow drill or "corer" in his suitcase "just in case he finds some good trees to sample". Rob explains that his wife, Andrea, has "forbidden" him from sampling trees when they are on holiday, but, he jokes "I am always tempted to take my corer".

The relative simplicity of fieldwork equipment is an advantage that facilitates the training and participation of amateurs like me. The first day I see the equipment used to sample living trees, I feel relieved and say to myself "I can do it". Rob shows the equipment: a corer; masking tape used to attach a label to the sample; wide plastic straws in which cores are stored and a marker for writing the sample code. The corer is the most idiosyncratic tool for dendroclimatological fieldwork (Image 8). Because the corer is relatively pricey (approximately £300), most PhD students, including Miloš, do not own a corer. For this reason, at the conference in Melbourne, the student prizes are corers. Rob owns ten corers; he lends them indiscriminately to fieldworkers as no one uses any specific corer.

[119] The complexity of ice-core expeditions in Antarctica is well described by Martin Skrydstrup in "Modelling Ice. A Field Diary of Anticipation on the Greenland Ice Sheet" in Hastrup, Kirsten, and Martin Skrydstrup (eds.) *The Social Life of Climate Change Models: Anticipating Nature*, ed. Kirsten Hastrup and Martin Skrydstrup (London; New York: Routledge, 2012).

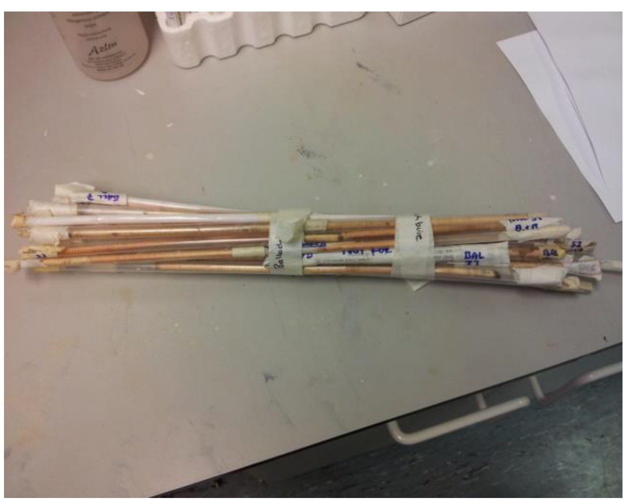
Image 8. The corer is an instrument of both work and professional identity for dendroclimatologists.

Sampling living trees is an activity that amateurs like me are allowed and encouraged to do by Rob. On my first day of fieldwork, Rob demonstrates to me how to sample trees. First, he shows how to insert the corer into the tree at breast height (and he insists not to sample “near the base where we can find missing rings” because the rings are more splayed out) and how to turn its handle in a clockwise direction. Rob advises me “to make sure that the corer is firmly attached to the tree or you’ll regret it later on in the laboratory because the tree-rings will look twisted”. At the time that Rob gives me these two pieces of advice I do not understand how tree-rings can look twisted or missing (a few weeks later when I am in the laboratory, I understand what he means).

I listen, observe and imitate what Rob does in the same tree that he is sampling. As Rob and I insert the corer into the tree, the friction increases and it becomes harder for me to turn the handle (image 9a). Once Rob has inserted more than half of the corer into the tree, he extracts the piece of wood that he calls “core” from inside the corer. At this moment, if the highland midges allow him, Rob quickly inspects the patterns and numbers of rings on the wood in order to decide whether this core could be used as a sample. Rob expects to find a minimum of 50 variable-looking rings to accept a core as useful. He inserts the core into the plastic straw and labels the straw with the code of the site and a number (image 9b). Rob finishes the sampling of the tree by turning the manual corer anti-clockwise. Because of Rob’s insistence that samples should be replicated, Rob asks me to repeat this procedure twice for each tree (at different points of the tree) for a minimum of twenty trees per site. As a result, my arms feel quite sore at the end of the day.

Image 9. To produce a sample from living trees, amateur fieldworkers like me are trained by Rob to identify the relevant trees visually; to core a tree (a); to check the pattern and number of rings; and to store the cores or samples inside straws and label them (b).

(a)



(b)

In comparison to sampling living trees, dragging and extracting pieces of subfossil wood out of the water is a more time consuming and collective activity. Whilst sampling one site of living trees could take us two to three hours, sampling the banks of Loch Gamhna and Loch an Eilein has taken Rob and his team three consecutive years of fieldwork expeditions. Sampling submerged logs requires a much more specialised division of expertise and team effort. Rob is often the one responsible for being inside the lake and scouting in search of submerged wood with snorkels and masks (Image 10a). Once he has identified a tree, he fastens the submerged log with a grabber, which is in turn tied to a rope. Those waiting on the banks pull the log out of the water with a winch and a pulley (Image 10b). Björn gives instructions to Rob, Hans and Miloš on how to position the log so that it is easier and safer for him to cut a slice of wood with the chainsaw (Image 10c). The existence of risky activities such as chain sawing is one the main reasons why Rob has purchased an accident insurance. At the end of the fieldwork expedition, Rob is happy to be able to joke "Another field trip and no deaths".

Image 10. To produce a subfossil sample, fieldworkers engage in a sequence of arduous and time-consuming steps, starting with the identification of logs with their feet (a); the extraction of logs from water (b); the cutting of slices of wood with the chainsaw (c); the discussion about the quality of the sample (d), and labelling of samples (e).

(a)

(b)

(c)

(d)

(e)

Embracing the physical and risky hardships of fieldwork is an aspect that fieldworkers consider as part of their identity, in particular Rob, who refers to it as a professional virtue. Two vignettes illustrate how Rob employs what I call the “Ethos of the Heroic fieldworker”[120], which emphasises

[120] Inspired by Bruce Hevly, "The Heroic Science of Glacier Motion”, *Osiris*, 1996, Vol.11(1), p.66.

direct experience of nature and (manly)[121] values of sacrifice. The first vignette occurs after six hours of trekking and sampling trees, when Rob discovers a “promising lake” that he wants to scout. Emily, Miloš and Anne do not look particularly pleased with Rob’s idea and Emily exclaims “I feel bad that I am not as excited as you, Rob, but I am tired and I want to go back to the car!” Emily’s guilt indicates her unease in not being able to live up to the expectation of what it means to be a good fieldworker, as personified by Rob who is always enthusiastic and ready for a new sampling opportunity. The second example occurs when we are all busy pulling out a log from the lake and Rob tells me “You see, this is the difference between those like Mann who sit at a desk and use archived tree-ring data, and those like us who create data at the site”. At the time Rob utters this comment, he is involved in a scientific controversy (chapter 7) with a paleoclimatologist called Michael Mann whom Rob regards to as an “armchair scientist”.[122]

2.2.4 The Calibrated Body of the Fieldworker

Fieldwork is a very stimulating sensory activity that depends on the bodily perception of objects through the use of senses. Like any other instrument, the body of the fieldworker needs to be “calibrated” and adjusted to the standard for fieldwork practice in the community.[123] The “Calibrated Body of the Fieldworker” serves to develop a very personal knowledge of the samples sites. The existence of experiential knowledge becomes evident in our daily walks to the sites, when Rob and the other fieldworkers give snippets of information about the area, the layout of the forest, and the characteristics of the trees that surround us. When one day in the field site I ask Rob to articulate the importance of participating in fieldwork to doing dendroclimatology, he responds that “We've been in the sites, we've done the data, and we know them so well”. The connection that

[121] My observations indicate that mono-gender was the stylistic rendering of everyone in the Scottish Pine Project expedition. More generally, dendrochronologists themselves have also been intrigued by the lack of gender distinctions in their work. I thank Carolyn Copenheaver for referring to me to her co-authored paper, Carolyn Copenheaver, A, Kyrille Goldbeck, and Paolo Cherubini. "Lack of Gender Bias in Citation Rates of Publications by Dendrochronologists: What is Unique about this Discipline?." *Tree-Ring Research*, 2010, Vol. 66. (2), pp. 127-133.

[122] The boundary-work distinction between expeditionary and “arm-chair” scientists is one that the historian Lawrence Dritsas also documents as occurring in the 19th century in “Expeditionary Science: Conflicts of Method in Mid-Nineteenth Century Geographical Discovery” in Charles W. J. Withers and David Livingstone (eds.), *Geographies of Nineteenth-Century Science* (Chicago: University of Chicago Press, 2011).

[123] For the idea of the body as a scientific instrument, read Jan Golinski, *Making Natural Knowledge: Constructivism and The History of Science* (Chicago: University of Chicago Press, 2008) p. 133.

Rob establishes between the verbs “being”, “doing” and “knowing” is crucial to understanding how the active body of the fieldworker becomes a source of knowledge. In particular, fieldworkers in the Scottish Pine Project employ the senses of sight, touch and smell.

Crucial to the production of samples is the visual identification of the relevant individual trees species to sample; this is a skill that experienced fieldworkers deploy automatically, but it needs to be explicitly formulated to neophytes like me. After demonstrating how to use the corer to sample living trees, Rob tells me: “I will sample 10 trees from here to the right, and you will sample another 10 from here to the left. We will meet at that fence over there in a couple of hours”. I feel daunted by the imprecision and openness of Rob’s instructions. In front of me, there are an unquantifiable number of trees that look very much alike to me. I ask Rob: “How do I know which trees to sample?” and Rob responds “You need to sample the trees that look healthy and alive”. I do not find this answer conclusive and so I ask him again: “How does a healthy tree look like?” He then gives me a list of canonical indicators while pointing to some examples around us: trees without scars, without resin and with large and green canopy are the relevant criteria. [124] After these instructions, we start work and each of us is responsible for extracting, at least, 20 pieces of wood in two hours.

The identification of relevant pieces of submerged wood requires the use of the entire body and, crucially, the sense of touch, as Rob detects a submerged log by feeling the presence of stumps with his feet. Rob’s collaboration with Stewart consists of co-developing a survey sonar system that could potentially replace the use of the sense of touch with the sense of sight, as the sonar would produce images that show the presence of submerged logs without having to scout the lake.[125] At this time, the sonar method is in development and so Rob still resorts to his feet to identify trees. In fact, Rob tells me that even if the sonar one day might work well, “We’ll always need this [manual approach]”.

Rob is the fieldworker who “is in the water” almost every day, and seems to have developed an expert tacit knowledge in identifying the presence of submerged logs with his feet. When I ask him to describe his skill to me he replies that “you have to feel the wood”. Rob suggests that I should try “to feel the wood” for myself and I accept his challenge. I put on the dry suit and I go into the water. Initially, I am scared of what lies below my feet and I am not enjoying the

[124] John Law and Barry Lynch. "Lists, Field Guides and the Descriptive Organization of Seeing: Birdwatching as an Exemplary Observational Activity.” *Human Studies*, 1988, Vol. 11.(2), pp. 271-303.
[125] Wilson, Rob, and Bates, Richard. “Lake sonar surveys and the search for sub-fossil wood”, *Dendrochronologia* 30 (2012), pp. 61–65.

experience, a feeling that Rob captures in a picture (image 11). Days afterwards, Rob attaches the picture of my discomfort in the lake to an email that he sends to all fieldworkers as a summary of the expedition. He writes “This fieldwork has been harsh for all - but perhaps it has been harsher for Meri :)”. While in the water, everything starts to make a little more sense when Rob joins me and asks me to emulate his movements with his feet. Rob tells me, “Here you should feel where the log ends, so I will attach the grabber here”. The crucial aspect in sampling logs is not only identifying them with the feet, but also avoiding branches and roots that are seen by dendroclimatologists as providing a biased climate signal.

Image 11. Rob takes this picture of me sampling submerged logs in the lake and uses it as an illustration of the arduous experience of generating samples during fieldwork.

As the process of sampling one single submerged log often takes up a minimum of half an hour of work, we are all excited to see the results of our effort. The main difference from the sampling of living trees is that we do not know whether the submerged log is a Scots pine tree until Björn cuts a piece of wood. In fact, we cannot strategically select the identity of subfossil material in advance as we do with living trees. Rob and his team do not know the exact origin of the logs they sample, and assume that they are from nearby forests.

To ascertain the quality and the identity of the wood extracted from submerged logs, fieldworkers engage in collective visual examinations and negotiations over the meaning of certain features of the wood. These discussions presuppose a familiarity with the Scottish environment and an ability to imagine how the past ecology of a specific site could have affected the growth of trees. In a matter of seconds, they examine the slice of wood that Björn has cut; Rob is often the first to utter comments such as “this is a very sexy sample”, “this is worth 200 years” or “this is shit birch, let’s get rid of it”. As with living trees, Rob and Miloš have established that a sample with fewer than 50 rings is not “worth” keeping because it makes the subsequent laboratory work too uncertain and laborious (the dating of tree-rings is difficult with so few rings).

The decision about the threshold for sufficient number of tree-rings is relative to the features of the local woodlands. Björn tells me that in Sweden they do not accept any piece of wood with less than 100 rings because the Scots pine trees there are known to be much older than in Scotland. All dendroclimatologists agree that a bad sample is one that shows very uniform tree-ring-rings, like the pieces of wood from birch trees for instance that we occasionally encounter and immediately throw back in the water.

On the basis of the observable features of the wood, Rob and the rest of fieldworkers often embark on speculation about the ecological or historical events that might have marked the life of the tree. These exercises of recreating past ecologies and forests are also crucial to discern the potential of the wood they could find on a site. When I ask my fellow fieldworkers how they know all this information just by looking at the pieces of wood, Rob and colleagues point to perturbations on the wood that they call “axe or fire marks” and refer to published references that confirm the existence of fires or other events. Indeed, the specific task that Rob assigns me during lake sampling is to write down in a notebook the samples that show axe or fire marks.

Another type of marks - the ones that fieldworkers leave when they core a tree - is of emotional importance to Rob and others. When we revisit the site that they had sampled a couple of years ago, one of the first things the fieldworkers do is to look for evidence of previous cores on trees or logs. “You see this hole here?” Rob asks me as he shows me a tree, “This is from a couple of years ago, when we first came here; it was raining cats and dogs!” I also experience a similar feeling on my second expedition, when I recognise the areas and trees that we sampled the year before. The sharing and re-enactment of sensorial memory related to the field is a feature of the collective experience of fieldwork.

The sharing of olfactory experiences, like with the senses of sight and touch, also contributes to a sense of community. Another of my responsibilities in the field is to tape the slice of subfossil wood and write the site code and the sample number on the tape. I then put the slices of wood into plastic bags, making sure that there is no air inside, as it accelerates the production of fungus and mould. Samples generally stay inside these bags for months until Miloš works with them in the laboratory in St Andrews. In a few months' time, the samples will stink very badly, but at this stage, their smell is captivating. I say something about the smell of fresh pine and Rob assures me that "only foresters or dendrochronologists who work with trees would recognise this smell". Another example of the role of smell happens at the end of the fieldwork day when we are all tired, look pretty miserable, and stink quite badly. At the end of my first day of fieldwork, when we jump into the car I notice a distinctive stink of algae and sweat. I immediately roll down the windows to let some fresh air in, and Rob looks at me amused and he says "You just need a bit more of time to get used to the smell of fieldwork".

2.2.5 The Rituals of Domestic Intimacy

Around 5pm, like in most offices in the UK, we finish our work in the field site. Rob gives us instructions to start bringing all the gear, bags and boxes of samples back to the pickup car. If we are sampling lakes, we often have to make multiple trips to the car. If we are sampling living trees, we always end up far away from the car and we need to walk more slowly in order to carry all the samples and the gear at once. Once we are in the car, our attention shifts from the rituals of sample generation to the rituals of recreation and preparation of the evening meal. The evening meal is the time of day when we return to the cottage and we share a meal that one of us (either Björn or I who have gladly become the official cook of the expedition) prepares.

The evening meal is the main socialising event of the day and Rob gives it special priority. He tells me that one of his main criteria in renting a self-catering cottage is to have a big dining table next to or as part of the kitchen. The kitchen and the dining table are the main spaces where we eat, chat, drink and play games when we are not working on the site. In fact, one of the distinctive features of fieldworks more generally is that it combines occupational and recreational activities.[126]

[126] The historian and sociologist of science Robert Kohler argues that, historically, the association between work and fun during fieldwork results from the activities of the middle-class culture of "nature-goers" in late

The pivotal role of the evening meal is precluded by a series of routine activities. On our way to the cottage, we stop at a supermarket to buy all the necessary ingredients for the evening meal and the next day's breakfast and lunch. I am very impressed by how efficient our shopping is, considering there are so many of us. We coordinate to find all the products we need. We buy food that we have all agreed to eat in a previous conversation in the car. In 20 minutes, we all meet at the till where Rob is waiting to pay for the shopping with his card and research funds. In less than half an hour, we are all back in the car. When we arrive at the rented cottage, in turns, we take a shower. I have priority because as the cook I can then start cooking the meal. Those waiting for their turn for the shower gather around the dining table, grab a beer or any other drink, and recapitulate the main events of the day.

The main topic of conversation before the evening meal is often the number of samples we have produced. On an average day, we might have generated between 30 and 40 slices of wood from lakes and up to 100 cores from living trees. These numbers generally decrease during the latter days of the expedition when we are more tired. To symbolise the daily achievements, we often place the cores in the middle of the dinner table. Anne or someone else checks that the labels on the samples are correctly copied into the notebook and she transfers the information to the computer (as I was always busy cooking the meal I realised later on that I never took a picture of the samples on the table). We leave the disks of subfossil wood outside the house, because of their smell and size. The duration of the pre-dinner conversation depends on the time that the cook needs to prepare the meal, which is usually between one and two hours. During this time, the scientists clean and prepare the equipment for the following day and they converse about results or research in which they are involved individually or as a group. I am told that Rob and Björn come up with the idea of doing an experiment together (Chapter 4) during one of these pre-meal conversations.

We start the evening meal with a sense of justice and reward, because we all feel that we have worked very hard in the field and deserve this meal. The meal generally consists of a high calorie main course (pasta, rice or meat with vegetables) and a dessert with generous amounts of wine and beer. The cook receives a public appreciation from the group at the beginning of the meal, and a lively chat, steered by Rob who always sits at one of the heads of the table, develops throughout the meal. The topics of conversation are often specific researchers and the latest developments in the field.

19th century America who sought to combine physical work and intellectual activity outside urban settings. Kohler, *All creatures: Naturalists, Collectors, and Biodiversity, 1850-1950* (Princeton, N.J.: Princeton University Press, 2006), p. 67.

The evening meal and fieldwork in general is a space where gossip circulates[127] and dendroclimatologists express shared opinions about colleagues. They often look at me worried, and apologise "for being so gossipy". I promise my fellow fieldworkers that I will never disclose anything I hear tonight. During or after the meal is also a time when they recall and transform the daily arduous experiences in the field into adventurous stories at which we all laugh. My unpleasant experience in the lake is a recurring case of good-natured banter. Some of these shared experiences are preserved in pictures, which Rob and Miloš later include in their presentation slides in conferences and talks.

In many dendroclimatology conference presentations I have seen, dendroclimatologists include pictures taken during fieldwork that show fieldworkers doing an activity that they recognise as heroic, funny or embarrassing. One of the functions of these group pictures is to re-create in the minds of conference attendee the hard conditions of the field that they cannot directly witness or experience.[128] Another function of these group pictures displayed for others to see is to represent the sense of collegiality and friendship that results from and is entangled with the production of dendroclimatological samples.

During the final hours of the fieldwork day, when we often play cards and drink Scottish whisky, we also learn about each other's characters. Rob sarcastically tells me that "This is the time when you see the real character of everyone". Before we all go to sleep around 11pm, Rob reminds us of the objectives and distribution of tasks for the day after. This reminder is a way to make sure that everybody is ready to go back to work after a few hours of recreation around the dining table. We all go to sleep in gendered bedrooms and I share the room with Anne. Fieldworkers are together 24 hours a day, even when we sleep. Such is the intensity with which the fieldwork imprints on me that every single day of the fieldwork expedition I dream about the day's events in the field.

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[127] Karin Knorr Cetina uses the concept of "gossip circles" in *Epistemic Cultures: How the Sciences Make Knowledge,* (Cambridge, Mass. ; London: Harvard University Press, 1999), p.201.

[128] The sociologist and historian of science Steven Shapin uses the concept of "virtual witnessing" to describe the literary technologies (including pictures) that scientists use to gain credibility from people who are not themselves involved in the scientific experiment or activity in "Pump and Circumstance: Robert Boyle's Literary Technology", *Social Studies of Science,* 1984, Vol. 14 (4), pp. 481-520.

2. 3 Discussion

To resolve the conundrum of producing samples that their colleagues and others will regard as genuine providers of information about past climates, Rob and his team build up and maintain a fiduciary framework and a system of trust relations during fieldwork that upholds their expertise and judgement as producers of samples. As the leader of the Scottish Pine Project, Rob is responsible for ensuring that the team members exercise the right competence and exhibit the right moral character, which underpins the production of samples. Rob also trains many of those neophytes participating in the fieldwork and inculcates his own dendroclimatological knowledge, skills and judgement. Ultimately, fieldwork serves to guarantee the trustworthiness of the samples within and beyond Rob's group insofar that it generates a culture and economy of trust, this confirms and valorises Rob's personal scientific judgement – his decisions about which trees to sample, which samples to retain and which to reject.

The members of the Scottish Pine Project participate in a larger fiduciary framework and system of trust relations with all other dendroclimatologists who consider site selection to be the most appropriate approach to sampling. Rather than removing themselves from the process of deciding where to sample and what samples to use, generations of dendroclimatologists have vindicated the collective belief and practice that the purposive selection of trees sensitive to climate is the most adequate sampling approach to the extent that it has been constituted explicitly as a "principle". The sturdiness of this explicit belief constituting the fiduciary framework is expressed by Edward Cook when he states "There's no way around it". This dogma is immune to criticism from statisticians and others who accuse dendroclimatologists of being "biased" and not basing their conclusions on "degrees of freedom". With the aim of testing and reinforcing the robustness of their fiduciary framework, dendroclimatologists are currently engaged in organised and civil scepticism about the potential biases associated with the "Modern Sample Bias".

The members of the Scottish Pine Project participate in the fiduciary framework sustaining the practice of site selection insofar that they build upon the sampling strategy initiated by their colleague Malcolm Hughes. In the same way as Hughes originally did for the first Scottish reconstruction, Rob and his team target Scots pine trees growing on the mountains of the Scottish Highlands. More generally, the existence of the Scottish Pine Project and Rob's aim of expanding the number of sites and samples from Hughes' original work depends on the fact that Rob and his team trust Hughes' skill in having produced trustworthy samples. Rob and his team are not the only participants in this fiduciary framework, as all users and contributors to the communal dataset in the International Tree-Ring Data Archive are linked to each other by relations of trust.

The production of new and credible samples for the Scottish Pine Project is dependent on the maintenance of pre-existing trust relations among fieldworkers through the shared experience of fieldwork, including the heroic rituals of fieldwork and domestic intimacy. These rituals are about trusting one another to take their share of the hard work and other hardships of fieldwork, to fulfil the jobs that Rob expects of them as part of the divisions of labour in the field site and at home, and to bear everything with goodwill. All these work and recreational rituals - particularly those associated with the evening meal - form an important basis for group cohesion and reinforcement of trust relations between fieldworkers, which ultimately serve to provide mutual recognition of each other as competent producers of samples and to certify the quality of samples.

Each of these pre-existing trust relations has a different dynamic and history. Rob's relationship with Björn is the longest as they worked together on a European project in 2006. The length and the intensity of these interactions may be the reason why Rob regards Björn so highly. Rob draws on his trust relation with Björn to expand his relations with others and to accept occasional fieldworkers such as Hans and Emily who are themselves trusted by Björn as a colleague and student respectively. Rob's collegial relation with Miloš also extends for a long time. Miloš was first "one of the best undergraduate students" Rob ever supervised; and then, Miloš was later a technician until Rob considered him competent and trustworthy enough to become his PhD student. Rob's trust relations with Stewart and Leah are less intimate than with the rest of fieldworkers. This difference in the degree of intimacy could be related to the fact that Leah and Stewart have different expertise, and accordingly, have never worked alongside the other members in the field site or elsewhere.

Being and working *physically* in the field gives the opportunity for fieldworkers to observe and confirm their colleagues' competences in producing samples. Fieldwork is one of the few times during the entire process of the creation of the dendroclimatological knowledge of Scotland when the whole team comes together.

Mutual examinations of each other's work in the field are particularly important for Rob as they help to establish the competence of neophytes like me in knowing how to produce a sample and establishing new trust relations. This is why Rob insists that all his students participate in fieldwork. My own experience as an amateur fieldworker shows that the learning taking place in the field site constitutes examples of civil scepticism, which in turn are parasitic on existing trust relations. When I express my doubts to Rob about identifying "good trees" visually and with my feet, I do so on the basis of my acceptance of his authority and expert knowledge as a teacher. When Rob, Björn and Miloš discuss the value of certain subfossil samples that they have just extracted from the lakes, they rely on their mutual trust as competent fieldworkers.

As a student, being or becoming a competent fieldworker also involves demonstrating to your main audience - that is, a teacher like Rob or your fieldwork colleagues - an adequate level of sceptical display. Showing awareness that not all areas or trees are adequate for sampling and that not all samples are equally useful is perhaps one of the most important skills of a fieldworker. The convention of this type of sceptical display depends mostly on the empirical features of the natural world, and therefore the specific situation in which this teaching relation occurs. The way an individual would enact his/her scepticism regarding the quality of samples is necessarily different in Sweden – where, Björn tells me, because of the greater age of Scots pine trees, good samples are considered those that dendroclimatologists agree are more than 100 years old – from that in Scotland, where Rob and Miloš employ 50 rings as a threshold.

One crucial aspect of the way Rob effectively secures the trustworthiness of the samples his team produced is by transmitting his deep personal knowledge and perceptual intimacy with the full range of sites covered by the Scottish Pine Project to the other team members in the day-to-day work of producing samples. This transmission occurs during our daily walks to the sites, when Rob gives different snippets of information about the historical and ecological history of the area or during the exercises of "recreation" whereby Rob and others imagine how the forest looked like in the past.

The credibility of Rob as the leader of the expedition and the Scottish Pine Project also depends, among other things, on the fact that Rob has developed his intimate knowledge as a result of establishing trust relations with estate owners. As a result, the team members and outsiders to the team willingly give assent to the quality of Rob's scientific judgement and grant him a sense of moral ownership of the field sites and the samples produced there.

As part of the Moral Economy of Fieldwork, Rob's system of rewards is a mechanism to reciprocate the team's trust in him and to demonstrate his trust in the competence of the other fieldworkers. Trust is effectively a gift, in the sense that fieldworkers - including myself - expect to receive Rob's trust in exchange for our participation in fieldwork. In my case, Rob, Miloš and the other fieldworkers reward my involvement in fieldwork with the establishment of trust relations that have been essential for the success of my PhD. As for the other fieldworkers, the trust that Rob grants them will also allow them to continue their respective individual projects and generate scientific knowledge. Stewart will be able to continue refining his sonar method; Leah will continue using her samples to examine archeological work; Miloš will publish a temperature reconstruction of Scotland; and Hans and Björn will use the Scottish data as a complement to the Scandinavian reconstruction.

The emergence of an intimate community of mutually trusting fieldworkers and dendroclimatologists will be crucial, as shown in the following chapters, to the collective efforts of creating dendroclimatological knowledge. In Rob's opinion, participating in fieldwork and being involved in the strenuous production of samples in the field places the fieldworker in a privileged position as a trustworthy knowledge-maker. For this reason, Rob's answer to my question about the importance of fieldwork ("we've been in the sites, we've done the data, we know them so well"), includes the pronoun "we". Dendroclimatological samples become credible scientific objects that Rob and Miloš will be warranted to use in future work, insofar as these samples are regarded as the result of a group of mutually acknowledged expert fieldworkers. Rob and Miloš' samples will subsequently be put to the test when the data and inferences that they draw from those samples are sceptically scrutinised by the wider dendroclimatological community, as I will show in later chapters.

3 Tree-Ring Dating

3.1 The Creation of Tree-Ring Chronologies

On our last day of fieldwork - while driving back from Aviemore to Edinburgh where Rob, Miloš and I live and where Björn and Hans catch their planes to return home - we stop in St Andrews to deposit the samples that we have produced in the field. The only samples that do not end up in St Andrews are those that Leah produces from beams in historical buildings. As agreed with Rob, she keeps ownership of those. “Here, we have the Scottish Highlands”, Rob says, pointing to the drawers of cores and the piles of slices of subfossil wood that the Scottish Pine Project team has accumulated over the years. Since Rob accepted the position of senior lecturer at St Andrews University, he has been negotiating to obtain an exclusive laboratory room to store and work with samples. However, much to his regret, all he has achieved is a bigger office room that he uses as a storage room (Image 12a). What Rob calls the “St Andrews Tree-Ring Lab” is a shared space with colleagues and students from other disciplines in the School of Geography and Geosciences at St Andrews University (Image 12b). Inside the school building, there are no signs or labels referring to the St Andrews Tree-Ring Lab, perhaps as a sign of the institutional invisibility of Rob’s research and laboratory at his university.

Image 12. After fieldwork, Rob's office (a) becomes the storage room for the samples, and the shared laboratory in the School of Geography and Geosciences at St Andrews University (b) is the space where Miloš employs the samples to generate data.

(a)

(b)

The St Andrews Tree-Ring Lab has a few pieces of equipment for which Rob has paid out of his own pocket. Rob proudly tells me that one of the microscopes is a “Soviet relic” that he acquired years ago when he set up a “domestic” tree-ring laboratory in Regensburg (Germany). In 1995, after meeting his German wife in Tasmania and she became pregnant with their son, Rob and Andrea returned to Germany. Rob had gone to Tasmania to pursue a postgraduate diploma in Antarctic Science after graduating in Geology from Durham University in 1993. In Tasmania, after failing the medical test to participate in explorations in the Antarctica, Rob met Edward Cook and Brian Buckley who taught him about dendroclimatology. Rob decided to train as a tree-ring laboratory technician, but his career was temporarily truncated by his unexpected parenthood at the age of 24. To support his family after their return to Europe, Rob worked as a geologist for a building company in Munich for two years. During this time, Rob did some dendroclimatology at home. Rob tells me that with a “modest budget” anybody can set up a tree-ring laboratory. What brings fame to a dendroclimatologist is to be the leader of a well-resourced tree-ring laboratory. Rob often says that he feels “jealous” of other better-off laboratories.

The St Andrews Tree Ring Lab has a much more distinct identity on the Internet than in the physical world. Thanks to its online existence, I found out about the St Andrews Tree-Ring Lab during the early stages of my doctorate when I was browsing on the web for potential tree-ring laboratories in Scotland. Shortly after he became senior lecturer, Rob created a website that “welcomes” people to the St Andrews Tree-Ring Lab and includes the university and school logos; a picture of Loch an Eilein and a short excerpt from a novel by Jack Vance called the “Miracle Workers” (image 13) that talks implicitly about dendroclimatologists: “I have wondered about trees. Trees are sensitive to light, to moisture, to wind, to pressure. Sensitivity implies sensation. Might a man feel into the soul of a tree for these sensations? If a tree were capable of awareness, this faculty might prove useful." Rob also uses this literary excerpt about the “miraculous” faculty of dendroclimatologists as an email signature.

The website of the St Andrews Tree-Ring Lab delineates the boundaries of Rob’s physically disperse community of collaborators. The website lists Rob’s projects; his publications; the names of his undergraduate and postgraduate students and the titles of their dissertations; and links to the online profiles of Rob’s collaborators in the Scottish Pine Project. Rob also employs the name of the laboratory as a platform to liaise with other individuals and laboratories. Rob announces on the front-page of the website “NEWS: St Andrews Tree-Ring Laboratory hosts TRACE 2014”, which is a conference for postgraduates that Rob will be organising in Aviemore in May 2014.

Image 13. This is a screenshot of the website that Rob has created to a give distinctive online identity to the St Andrews Tree-Ring Laboratory and to his community of collaborators.

From the moment we leave the hundreds of samples in Rob's office, Miloš becomes the main person responsible for working with them to create data. As part of his PhD, Rob expects Miloš to generate original data. Miloš agrees with Rob's expectation, but he also often expresses feeling slightly "overwhelmed" by the amount of samples that he needs to process by himself. Indeed, I offered my services to Miloš as his "voluntary technician" the first day I met him after hearing him commenting about the considerable amount of data he would not to produce.

The data I help Miloš produce from samples is called a "tree-ring chronology". Each tree sample represents a single tree-ring chronology or a tree-ring series. The sequence of tree-rings results from the fact that the tree generally grows a layer of wood every year (as I explain later, it is possible that some years trees produce no ring and wood at all). The size of this layer is influenced by climatic conditions (in warmer years it is wider and in colder years it is narrower). In the slices of wood like those that Rob and his team cut from submerged logs during fieldwork, the layers of wood look like concentric rings (image 14a), hence the name "tree-rings". When the pieces of wood are extracted from living trees ("cores"), the tree-rings look like a series of narrow and wide lines (image 14b).

In textbooks, scientists employ various metaphors to explain how they use tree-rings as a source of data. Jim Speer in his textbook compares cores to a "barcode with varying widths of lines representing each year".[129] Andrew Douglass - the acknowledged founder of the first laboratory of tree-ring research - compares the pattern of narrow and wide tree rings to the Morse telegraph code of dots and dashes. Harold Fritts follows up this metaphor: "in much the same way, the sequence of narrow (dots) and wide (dashes) rings in a sensitive ring series conveys messages about the life of the tree".[130] The dendroclimatological work that Rob and Miloš conduct in the following chapters is aimed at "decoding" the climatic message that they believe exists in trees.

[129] Speer, *Fundamentals, p.*12.
[130] Fritts, *Tree Rings and Climate,* p. 19.

Image 14. These two highly stylised pictures were created by the dendrochronologist Henri Grissino-Mayer and have become "iconic" illustrations in many popular publications in dendrochronology, including one of mine.[131] The first image (a) is a slice of wood with annual tree-rings of growth disposed concentrically, whilst the image (b) below shows a series of polished cores next to each other that show tree-rings sequentially.

Source: Henri D. Grissino-Mayer, The University of Tennessee, Knoxville.

[131] Ramírez-i-Ollé, "The Social Life of Climate Science".

Dendrochronology is the science that identifies the precise calendar year of each tree-ring in order to produce tree-ring chronologies. This is a procedure that specialist scientists or so-called "dendrochronologists" refer to as "tree-ring dating". Nowadays, tree-ring chronologies are used for different purposes or "applications", each developing into the constitution of subfields within dendrochronology. Textbook authors enumerate five "subfields" that have been named by keeping the base of the word "dendro" (the Greek word for "tree limb") and adding a prefix to refer to the application.[132] As a result, tree-ring chronologies are used to infer past climates (dendroclimatology); to research past dwellings and societies (dendroarchaeology); to study ecosystems, fire occurrences, death of trees and insect outbreaks (dendroecology); to determine land movements (dendromorphology); and to estimate levels of pollution and presence of chemicals in soils and the atmosphere (dendrochemistry).

Each of the subfields within dendrochronology has its own specialised textbooks, journals, techniques, tradition of knowledge and authors of reference. In the two dendrochronology conferences I attend, talks are arranged into streams that correspond to the subfields. Likewise, plenary sessions always include a speaker representing each of the areas of dendroclimatology, dendroecology and dendroarchaeology. Despite all attempts at equitability, a few dendrochronologists to whom I talk at these conferences express their resentment to what they perceive as the predominant position of dendroclimatology. One senior researcher, who defines himself as a "pure dendrochronologist", says that he is "bored" of going to conferences and just listening to colleagues talk about "low and high frequency", which are terms that only dendroclimatologists use. One dendroarcheologist complains that "now it seems that chronologies are worth nothing unless you use them for climate reconstructions". One junior dendroecologist complains that ordinary people associate the use of trees with climate reconstructions, and insists that "there is much more to learn from trees than just climate".

Rob aims to integrate the subfields of dendrochronology within the Scottish Pine Project. The composition of the project team itself represents two of the specialised fields of expertise within dendrochronology: Leah is a dendroarcheologist; and Rob, Björn and Miloš are dendroclimatologists. They all call themselves "dendrochronologists", and with the exception of Leah, they also identify as "dendroclimatologists".

The five aims of the Scottish Pine Project, as stated in the website of the project, also

[132] Speer, *Fundamentals, p.*5.

represent both the collective and specialised research interests of the team.[133] The first two aims are related to dendrochronology and the creation of plentiful and longer tree-ring chronologies from all existing Scots pine woodlands in Scotland and subfossil pine wood. The third aim relates to dendroecology: it is to improve "the understanding of the future response of the native pinewoods, and therefore to assist in future management strategies". The fourth aim relates to dendroarchaeology and is "to extend the application of native pine dendrochronology for cultural heritage research, including dating and provenance native Scottish pine timbers in buildings and archaeological sites". The final objective involves dendroclimatology and dendrochemistry and is "to reconstruct, using physical and chemical methods, the climatic and environmental history of the region for the last 2,000 years and possibly back into the early Holocene".

What brings together all specialists in dendrochronology is the shared practice of tree-ring dating, which involves the counting of tree-rings and, crucially, the comparison of these counts across samples in a process that dendrochronologists refer to as "cross-dating". Once dendrochronologists determine with certainty the total number of rings in each sample, they assign the calendar year to each tree-ring by counting backwards towards the centre of the tree from the outermost ring, laid down in the year when the tree is sampled. All dendrochronologists emphasise in their textbooks and presentations that cross-dating is the foundational method of tree-ring dating. In my early stages of analysis, I checked my interpretation that tree-ring dating involves establishing a count of tree-rings with Rob; he disputed my account by responding, "Dendrochronologists don't count, we crossdate. Foresters count".

Rob is not the only one who refuses to describe tree-ring dating as a ring-counting activity and characterises ring-counting as unscientific.[134] The website of one of the first tree-ring laboratories in the world, at the University of Arizona, explicitly states that dendrochronology is not about counting rings, but about cross-dating.[135] In one of the evening lectures of the training course I attend in Tasmania, a local forester gives a presentation about a few chronologies that he has created with local timber. When I ask one of the attendees for his opinion about the talk, among other things, he says "Clearly, he is not doing the same that we do. I don't think he did any cross-

[133] "Project Aims", Scottish Pine Project Website, accessed 15 July 2015, https://www.st-andrews.ac.uk/~rjsw/ScottishPine/

[134] The concept of boundary-work refers to the rhetorical distinctions that scientists make between their (scientific) knowledge and that of others with the purpose of achieving authority. See Gieryn, "Boundary-Work and the Demarcation of Science from Non-Science: Strains and Interests in Professional Ideologies of Scientists", *American Sociological Review.*

[135] Laboratory of Tree-Ring Research, Arizona, accessed 2 July 2015 http://ltrr.arizona.edu/about/treerings

dating". My characterisation below of tree-ring dating as "Counting Tree-Rings" subsumes the dendrochronologists' insistence that in order for a ring-count to be accurate, it needs to be compared and validated across different samples.[136]

The epistemological conundrum that all dendroclimatologists face at this stage of the production of dendroclimatological knowledge is the production of accurately dated tree-ring chronology, given all the uncertainties in counting and cross-dating tree-rings. Generations of dendrochronologists working in different subfields and geographical areas have developed a variety of methods to resolve the uncertainties associated with tree-ring dating. In this chapter, I describe how Miloš and Rob draw on some of those methods developed by colleagues, whilst at the same time creating methods specific to the material samples from Scotland as well as to the research aims of the Scottish Pine Project and the features of the Tree-Ring Laboratory in St Andrews. I characterise the way Miloš and Rob establish a definitive count of tree-rings and create tree-ring chronologies in three steps: by interpreting tree-ring patterns on the wood; by representing these tree-ring patterns in numbers; and by comparing tree-ring patterns between samples.

3. 2 Counting Tree-Rings

3.2.1 Interpreting Tree-Rings

Miloš and I do not just simply "see" tree-rings with our naked eyes. We need to carry out laboratory work and develop our skill in order to "read" and interpret the patterns of dark bands we perceive from the wood. The types of laboratory practices involved in reading tree-rings are both preparatory and observational. These two sets of practices are different depending on whether we work with cores from living trees or slices of subfossil wood.

In terms of preparatory work, we subject samples to a process aimed at "upgrading the visibility"[137] of tree-rings. The enhancement of the visual apprehension of rings consists of a series of manual steps that I learn by observing and imitating what Miloš does. If we work with cores,

[136] The dendrochronologist Fritz Schweingruber also explains in his textbook that the creation of tree-ring chronologies requires an enumeration of rings, *Tree Rings-Basics and Applications of Dendrochronology* (D. Reidel Publishing Company, 1988), p.47.

[137] Michael Lynch, "Discipline and the Material Form of Images: An Analysis of Scientific Visibility.", *Social Studies of Science*, 1985, Vol. 15.(1), p. 51.

Miloš starts by submerging them in bottles of acetone in order to eliminate the resin from the wood (image 15a). He is very careful to attach the core to its code with a thread to ensure that the cores do not become unidentifiable and lose their connection with the sampling site that Miloš seeks to represent.[138] Months later, when I have the opportunity in Tasmania to know more about other dendroclimatological projects, I realise that submerging wood into acetone is a specific step in the new methodology that Rob and Miloš are developing to create tree-ring data (chapter 5) and that most tree-ring labs in the world do not follow this procedure. When the core is dry, Miloš glues it to a prefabricated wooden mount onto which he writes the core code to keep track of its identity. Miloš often has to break the core into pieces so that the tree-rings look “straight”. As Rob warned me in the field (Chapter 2), tree-rings often come out “twisted” as a result of the corer tip not being sharp enough or the fieldworker not applying sufficient pressure in boring the tree with the hollow drill. More generally, the need to align tree-rings is an example of how (bad) fieldwork shapes laboratory work, and how the demands of the latter shape the former.

Like in the field, we carry out laboratory work and prepare samples with the aim of pre-empting future problems. For instance, Miloš insists that we have to make sure that the “shiny side” of the rings are on the sides and the “dull side” face up and down. “Otherwise, you will be in trouble when trying to see the rings under the microscope later on”, Miloš warns me. Being a competent laboratory worker means being able to anticipate and avoid future problems and unnecessary steps. One undergraduate student in the laboratory realises this when she complains, “If I had paid more attention on how to glue the core I would have saved so much time now!” Once the glue is dry, Miloš sands the core using progressively finer sandpaper until the core ends up with a flat surface (image 15b). At the time I conduct my observation, the methodology for preparing subfossil samples is not as elaborate as with the cores, we do not have to treat the slices of wood with acetone, or glue and cut them into pieces to align the rings. With subfossil samples, Miloš shows me how to prepare the slices of wood; he uses a small blade to remove part of the rotten surface so that the surface is smoothen and the two transversal paths of rings are more visible (image 15c).

[138] Bruno Latour refers to this connection as “Circulating Reference” in *Pandora’s Hope: Essays on the Reality of Science Studies,* (Cambridge, Mass. ; London : Harvard University Press, 1999). I expand on this idea about the circularity of dendroclimatological knowledge in the conclusions chapter.

Image 15. Seeing tree-rings requires preparational work that upgrades their visibility. We treat cores with acetone (a), and transform round-shaped cores into flat surfaces (b). With subfossil samples, we air dry them and remove part of the rotten surface (c).

(a)

(b)

(c)

For observational work, Miloš employs machines (a microscope, a computer and a scanner) to magnify the view of tree-rings (image 16). While Miloš is slightly shocked to hear my breaching question about whether he has ever considered the possibility that the microscope might not be a faithful mediator to analyse the wood (he emphatically answers, "No way!"), he does have some doubts about the role of the scanner. Miloš is concerned that the digitised image of tree-rings does not represent the original colour and visibility. His concerns are partly motivated by the fact that the scanner is a new instrument for visualising tree-rings. Miloš employs it as part of the methodology he is co-developing with Rob and others and therefore, it is not applied in standard dendrochronology work (Chapter 5).

Miloš tells me that he is aware that his peers will scrutinise his methodology, and that "in order to create a more robust methodology I need to identify its biases", including those related to the scanner. Miloš develops a series of tests with the aim of determining the effect of the scanner on the resulting data. He darkens the colour of the box that he uses to surround the scanner and to prevent light contamination, and he also tests for differences between different angles and positions in which cores are scanned.

Image 16. These are the two instruments that Miloš and I use for visualising tree-rings. The computer screen where we can see a digitised image of a core is on the left and the microscope and the measuring stage are on the right.

There are other aspects of the new methodology involving the scanner and the use of digitised images that Miloš takes for granted. I ask Miloš if he has ever examined the role of the calibration card, which is an object that is explicitly used to avoid colour or size distortions between the scanned image and the original object. Miloš justifies the fact that he does not test the reliability of calibration cards by explaining that others might have done so for him. Miloš says that "These calibration cards have been calibrated according to international standards, and so I assume that other people have done many tests with them and they have concluded that these cards work fine". Miloš adds that even if he detected any problems with the calibration card, he would not know how to fix them, "so I prefer to use this card as it is".

Even after preparing samples for visual examination with the microscope or with the computer, seeing rings involves many uncertainties with regards to the exact boundaries of a ring. On an ordinary day in the lab, one of the most frequent questions I and other undergraduate students ask Miloš is: "Is *this* a ring?" If I am measuring rings in subfossil wood with a microscope, I use a pointer to mark the exact ring that I am doubtful of. Beginners are asked to count tree-rings in groups of 10 following an established code among dendrochronologists (one dot every 10 rings, two dots every 50 rings and three dots every 100 rings). This "highlighting" strategy[139] is particularly useful when I have to show Miloš any problematic ring. I find it much easier to discuss my doubts with Miloš in front of the computer screen rather than in front of the microscope because we can both use our hands and body[140] to indicate the boundaries of the ring on the digitised image:

1. Meritxell: Is this {pointing to the computer screen} a ring?
2. Miloš: Mm, let me see
3. Meritxell: {stands up to let Miloš sit in front of the screen}
4. Miloš: {sits down}
5. (pause of 3 seconds)
6. {slight turn of the head}
7. {zoom in and out the picture}
8. This is very tight {approaches towards the screen}
9. Meritxell: Yes, I know
10. Miloš: (pause of 6 seconds)
11. I would say this {pointing to the screen} is a ring
12. Meritxell: Really?
13. Yes, you can see the dark band of the ring here {moving the finger in circles in front of the screen }
14. Meritxell: Okay
15. Miloš: If you have any doubt, you could always validate the ring under the microscope.

Another way in which I learn how to see rings is through the use of diagrams and exemplary images. In one of his lectures, Rob uses a drawing that includes some arrows pointing to the distinct boundaries of a ring (image 17). Rob employs these images to re-create in the minds of students the act of identification of tree-rings that takes place in the laboratory, which students cannot directly witness in the classroom.[141] All the verbal, bodily and written demonstrations and instructions that we, as students, accept from Miloš and Rob organise our perception of the ring

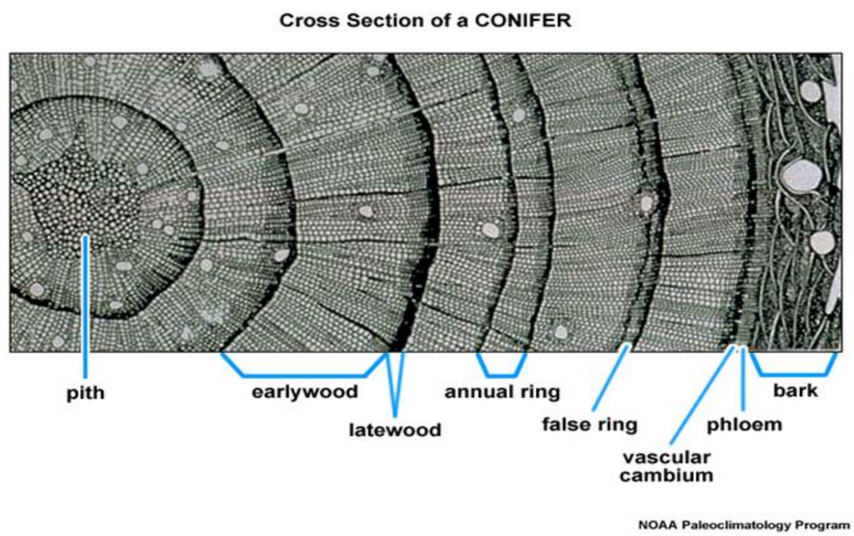
[139] Charles Goodwin, "Professional vision", American Anthropologist, 1994, Vol.96 (3), pp.606-633.

[140] Similarly, in her ethnographic study, Janet Vertesi describes how members of the Mars Exploration Rover team use their bodies to interpret the images that robots return from the Martian surface in "Seeing like a Rover: Visualization, Embodiment, and Interaction on the Mars Exploration Rover Mission", *Social Studies of Science*, 2012, Vol.42 (3), pp.393-414.

[141] Shapin, "Pump and Circumstance".

patterns on the wood and constitute our professional vision as dendrochronologists so that we are able to classify a dark band as a "tree-ring" or "non-tree-ring".

Image 17. Rob uses the diagram below to recreate in the novices' minds the process of interpreting wood patterns and tree-rings that takes place in the laboratory.



Source: National Centres for Environmental Information Paleoclimatology Program.

3.2.2 Representing Tree-Rings

I also help Miloš generate measurements that represent certain physical properties of the ring. The numbers that we produce from samples will partly replace the wooden samples and their scanned images at later stages of the reconstruction. Miloš will only go back to the material samples or digitised images if he struggles to find a matching between measurements of tree-rings (See Section 3.2.3).

Once samples are measured, they often become a "nuisance" for dendrochronologists, who struggle to find space to store them. For now, storage is not an issue for Rob because he stores the samples of the Scottish Pine Project in his big office and uses them, among other things, for "display" in interviews with the media (image 18). However, Rob's trip to Canada in March 2015 was aimed to resolve the storage problem of his doctoral supervisor, Brian Luckman. Despite the fact that Luckman's samples are among the oldest in the Northern Hemisphere, Rob tells me that

Luckman has not been able to convince his superiors to keep and archive the samples after he retires. As a result, Rob has accepted Luckman's offer and will "inherit" his samples.

Image 18.Whilst samples lose most of their value - and thus become a "nuisance"- once tree- rings are represented in numbers, they do become objects of professional representation, as in the picture below that shows Rob posing with samples for a television interview.

Miloš teaches me to employ two methods for measuring and representing tree-rings into numbers: a manual method to measure the width of rings in subfossil samples and a semi-automatic method to measure the reflectance of tree-rings from cores.

To measure ring-width manually, we have to make multiple (and often implicit) decisions with respect to the start and end of the tree-ring boundaries. We place the slice of wood on a measuring stage that works in conjunction with a microscope with cross-hairs and a recorder device. We move the stage with a little handle and when the crosshair coincides perpendicularly with the ring boundary, we press the button and the recording device records the distance travelled by the stage in millimetres, which is later on sent to a software program installed on a computer. A dendrochronology textbook author notes that whilst the recording of measurements is now the job of the computer, this is a task that humans used to do in the past.[142]

Miloš insists that I should measure the ring at the point where it looks more "proportional", meaning the area where the width of the ring looks equally wide and narrow (as trees do not grow uniformly in all directions and tree-rings are not perfect concentric circles). The subjectivity involved in the measurement of tree-rings is the basis upon which the dendrochronologist Michael Baillie – member of a team responsible for the construction of Ireland's long oak dendrochronology and the calibration of the radiocarbon timescale[143] - claimed in an article in the newspaper *The Guardian* that he has property rights over the ring-width measurements he produces because "the ring pattern of a tree-ring sample carries the 'intellectual fingerprint' of the dendrochronologist who measured it".[144]

To measure the reflectance of tree-rings, Miloš and I employ an automatic function from a relatively new software program called "Coo Recorder". This software is able to detect the ring-boundaries and to place measurement points automatically. With a handheld device (a mouse), we click one point on the first ring on the digitised image and with the use of coordinates, the programme detects and measures the subsequent rings. The program makes some calculations that result into a number representing the reflectance of the ring. Rob discovered Coo Recorder through an international mailing forum for dendrochronology to which its software developer, Lars, had sent an announcement. Through email, Lars and Rob agreed that Miloš would experiment with Coo Recorder as Rob intended to use it as a cheaper alternative to an existing one that Rob could not afford (Chapter 5).

In early 2012, before Miloš started experimenting with CooRecorder, automatic methods

[142] J. R. Pilcher explains in one textbook that "Douglas recommended having an assistant write down the measurements as the measurer calls them out" in Edward Cook and L.A. Kairiukstis, *Methods of Dendrochronology: Applications in the Environmental Sciences,* (Kluwer Academic Publishers, Dordrecht, The Netherlands, 1990), p.47.

[143] Michael Baillie beautifully writes the story of these two research projects in *Tree-Ring Dating and Archaeology* (Chicago, Ill.: University of Chicago Press, 1982). As part of the project for "An Oral History of British Science", the British Library commissioned an extensive interview with Michael Baillie where he described his career. The transcript can be found here: http://sounds.bl.uk/related-content/TRANSCRIPTS/021T-C1379X0085XX-0000A0.pdf

[144] Michael Baillie made this claim in an article in the Guardian: "Tree-ring patterns are intellectual property, not climate data", *Guardian*, 19 January 2013, accessed 1 July 2015, http://www.theguardian.com/environment/2010/may/11/climate-science-tree-ring-data as a response toDavid Holland, who asked for Baillie's data to be released through a Freedom of Information request, and whose requests prompted the Climategate affair (Chapter 1).

of measurement became the source of a dispute among a few dendrochronologists on the same international mailing list where Rob discovered CooRecorder. The conversation started when someone asked the members of the list about automatic methods of measuring tree-rings. One of the contributors to this conversation was Rob, who openly expressed his distrust in relying *exclusively* on automatic measurement methods as a mechanism to carry out tree-ring dating. Rob said, "I would not trust ANY automated option to identify and measure rings. Cross-date and then measure (from scanned images or through a microscope with stage) is really the only careful way to go".[145] Rob's comment was followed by a comment from another researcher who used a metaphor to distinguish between automatic and manual methods of measuring tree-rings: "The difference is a bit like being a guitarist composing music and a non-guitarist using plug-ins to compose music".

Miloš is aware of the concerns of his supervisor and the wider community with regards to automatic methods of measurement. Therefore, he insists that I should always double-check whether CooRecorder has placed the points correctly. Whilst concerns about proportionality are at the centre of the measurements we produce for ring-width, when we measure reflectance, Miloš emphasises that we must be careful in delineating the darkest parts of the rings and avoiding sections that look brighter. As part of the testing that Miloš is conducting with the new methodology, he experiments with the settings of CooRecorder to estimate their different effects on the type of measurements produced.

Whatever method of measurement we use, Miloš always evaluates and corrects my measurements following a criterion of "relative accuracy". Miloš compares my measurements against his and interprets the degree of similarity between the two as an indicator of the accuracy of my measurements. Because Miloš recognises the subjectivity involved in measuring tree-rings, he does not look for exact similarity. When he considers the difference in micro millimetres to be too big (generally when this difference is larger than 0.001 millimetres) he asks me to adjust my measurements to resemble those of his. On one occasion, Miloš makes explicit the criterion of relative accuracy, when he congratulates me by saying, "Well done! This almost looks like if I had done it myself!"

The strategy of assessing the quality of measurements in relation to the expertise of expert authorities seems to be a common practice in dendrochronology. In his textbook, Harold Fritts

[145] Rob Wilson, "Epson Expression 10000XL Scanner and Memory Issues," *ITRDB Dendrochronology Forum*, 2 February 2012, http://listserv.arizona.edu/cgi-bin/wa?A2=ind1202&L=itrdbfor&T=0&P=570 (accessed 5 September 2013, no longer available).

mentions a quantitative method that he has devised (the "test of measurement accuracy"), which consists of "comparing measurements of particular operators to those of experts".[146] Rob tells me that there is also software that is designed to do such tests, but he has never used it, as "one must have some faith", he says. Similarly, Rob does not use any "accuracy league" displayed on the wall to compare his students' measurements; as J. R. Pilcher reports in a textbook published in 1990 that some laboratories do. [147]

3.2.3 Comparing Tree-Rings

Miloš insists that I should not use the calendar years that the measuring machines automatically assign to tree-rings as evidence of tree-ring dating. Both CooRecorder and the other software connected to the measuring stage and microscope automatically attribute calendar years to the measured tree-rings by counting backwards from the year the tree is sampled. However, Miloš insists that these datings are uncertain because they do not account for the potential presence of anomalous rings or what he calls "false" and "missing" rings. A false ring is a duplicated ring created when two rings grow in the same year, whilst a missing ring is an absent ring that has not grown during a year.

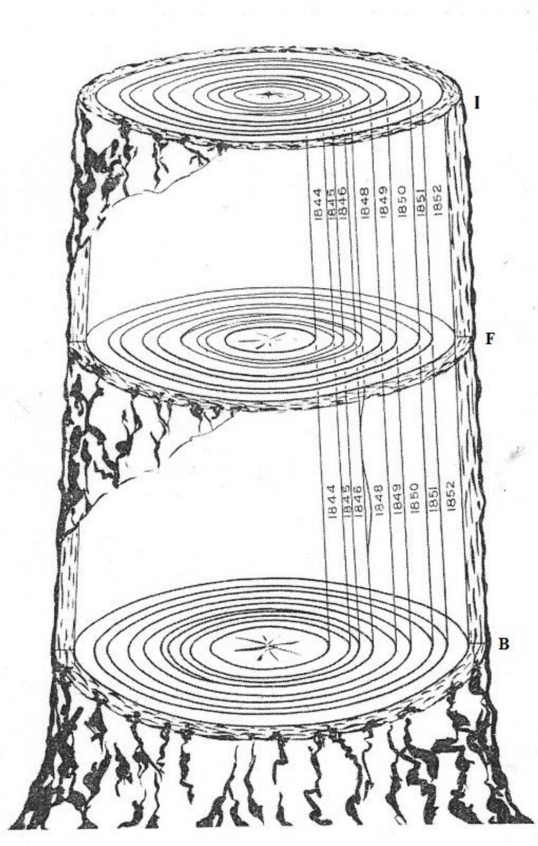
As dendrochronology is based on assigning calendar years to *annual* tree-rings, the hypothetical existence of false and missing rings renders tree-ring counting problematic for dendrochronologists. In dendrochronology textbooks, false and missing rings are said to have a physiological explanation. Warm conditions scattered throughout the growing season cause trees to grow for multiple periods and hence produce one or more false annual rings. Missing rings, meanwhile, result from a lack of growth hormone, generally due to cold weather or very dry conditions. In years with extremely little growth, a ring might only appear at points of stress, such as the downhill side of a trunk or under the branches.

Dendrochronologists explain that because they normally core trees at chest-height, cores often have missing rings that cannot be detected unless they are compared against cores extracted from near the top of the tree (image 19). This is why, in their textbook, Stokes and Smiley argue that the most appropriate term should be "locally absent rings" to denote that a missing ring only

[146] Fritts, *Tree Rings and Climate*, p.250.
[147] Cook and Kairiukstis, *Methods of Dendrochronology, p.* 45.

exists at the point sampled.[148] The existence of locally absent or missing rings also explains why in the field Rob insists on taking replicate cores from different parts of the same tree, but avoiding the area near the roots where missing rings often appear. One source of uncertainty in ring-counting specific to the Scottish Pine Project arises from the fact that they work with samples of submerged wood. With this type of sample, Miloš explains, it is often impossible to determine the exact year in which the tree ceased growing and the last ring was laid down, which is the basis of ring-counting.

Image 19. With the image and caption below, dendrochronologists Stokes and Smiley "make visible" missing tree-rings and justify taking multiple cores from different parts of the tree.



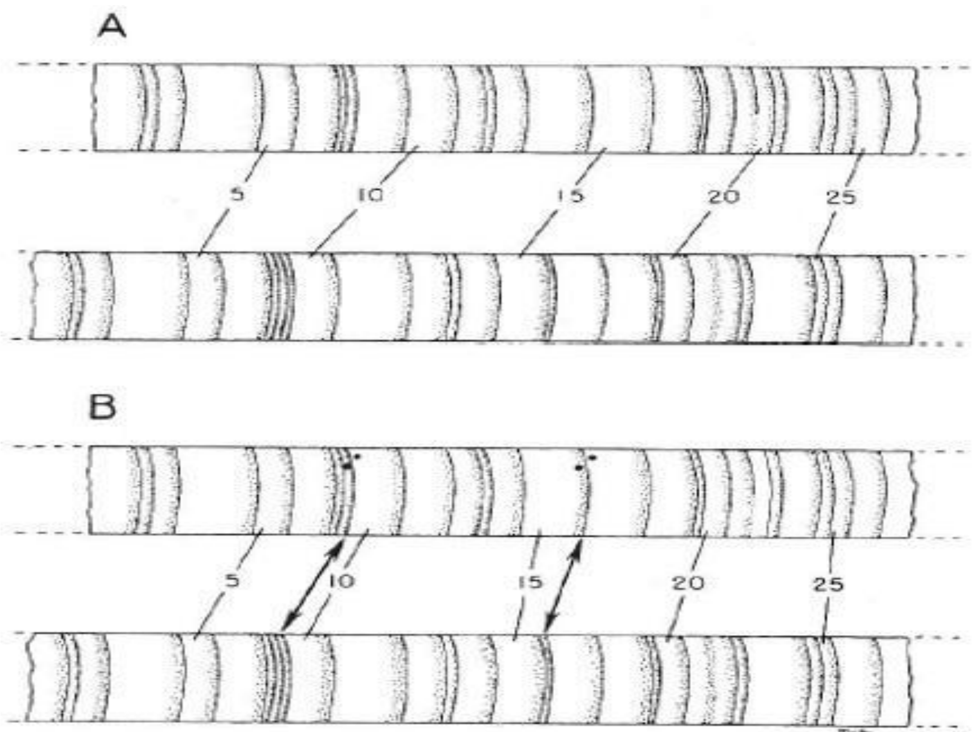
"Figure 7 (after Glock) diagrammatically illustrates the base portion of a tree stem. It shows three levels of cross-sectional surface, and each corresponding ring is connected with a vertical line. The ring representing 1847 is missing in the lowest section, appears as a lens between B and F and shows as a smaller ring in section F and I.[149]

[148] Marvin Stokes and Tarah Smiley, *An Introduction to Tree-Ring Dating*, (Tucson: University of Arizona Press, 1969), p.

[149] Stokes and Smiley, *An Introduction*, 14.

To overcome all these uncertainties, Miloš and other dendroclimatologists compare the synchronicity in the patterns of tree-ring growth – in particular, sequences of relatively wider and narrower rings – between different samples as an indication that tree-rings are well dated. Dendrochronologists refer to this procedure of comparison as "cross-dating". As described and illustrated by Harold Fritts in his textbook (image 20), the work of cross-dating consists of *both* pattern-matching and correcting anomalous asynchronies to a state of "normal" and expected synchrony.

Image 20. Harold Fritts uses the image and the caption below to describe cross-dating and the inference of missing and false rings on the basis of the discovery of consistent asynchronies between cores and ring-patterns.



"Every fifth ring is numbered in the diagram and in A the patterns of wide and narrow rings match until ring number nine, after which a lack of synchrony in pattern occurs. In the lower specimen of A, rings 9 and 16 can be seen as very narrow, and they do not appear at all in the upper specimen; while rings 21 (in the lower) and 20 (in the upper) show intra-annual growth bands [false rings]. In the upper specimen of B, the positions of inferred absence [missing rings] are designated by two dots, the intra-annual band in ring 20 is recognized, and the patterns in all ring-widths are synchronously matched".[150]

[150] Fritts, *Tree Rings and Climate, p.*23.

Dendrochronologists' expectation to find synchronicity between patterns of tree-rings comes from the fact that cross-dating has become the most important "principle" in dendrochronology, as defined by Harold Fritts in his 1976 textbook.[151] The discovery of consistent and similar tree-ring patterns between tree-rings from different trees, and thus the creation of cross-dating as a "principle", has developed over time with dendrochronologists' continuing success in achieving cross-dating. In an article in 1937, Andrew Douglass explains that he first discovered the phenomenon of cross-dating in the trees of Northern Arizona.[152] 100 years later, one reputed dendroclimatologist and one dendroecologist, Edward Cook and Neil Peterson, claim that "admittedly, cross-dating is not universal among all tree species. However, its occurrence over a broad range of taxa [types of trees] growing in extremely diverse habitats worldwide indicates that cross-dating is a property of tree growth".

The consistent empirical confirmation and assemblage of cases of pattern-matching is seen by dendroclimatologists not only as a validation of the principle of cross-dating, but also of the fundamental assumption on which cross-dating is based, namely that different trees respond in a similar way to common climatic influences. Harold Fritts concludes in his textbook that "the fact that cross-dating can be obtained itself is evidence that there is some climatic or environmental information common to the sampled trees".[153] Similarly, Malcolm Hughes (the dendrochronologist who created the first Scottish climate reconstruction) affirmed in 2012 that "so far as a cause for these common patterns is concerned, the prime suspect is climate variability. So, in turn, where tree-ring samples 'cross-date' (share massively replicated patterns of variability), a prima facie case for their containing a climate signal has been made".[154]

Miloš uses statistical packages to evaluate the degree of synchronicity between tree-rings. Essentially, these programs generate "correlation coefficients" that are the result of statistical comparisons of chronologies. Due to the availability of powerful desk computers, Miloš is able to use these statistical packages from home. At this stage of the creation of tree-ring

[151] Fritts, *Tree Rings and Climate*, p. 20.
[152] Alexander Douglass, "Tree Ring Work", *Tree-Ring Bulletin*, 1937, p. 3.
[153] Fritts, *Tree Rings and Climate*, p.21.
[154] Malcolm Hughes, "Dendroclimatology in high-resolution paleoclimatology." in Malcolm Hughes, Thomas W. Swetnam and Henry F Diaz (eds.), *Dendroclimatology: Progress and Prospects* (Springer, 2011), p. 31.

chronologies, Miloš' place of residence becomes his (and my) place of research.[155] Once per week I go to Miloš' house and observe how he does cross-dating work for a couple of hours. Miloš uses an MS-DOS (Microsoft Disk Operating System) software called "COFECHA" to work with the measurements from subfossil samples, and a more recent program called CDendro - developed by Lars, the software designer of CooRecorder - to work with measurements from cores. The difference between the two packages is that CDendro has a graphic line function that represents visually the tree-ring measurements, which, Miloš says, "makes it easier for me to compare the data".

The traditional cross-dating method described in all dendrochronology textbooks as "skeleton plotting" was also created to facilitate visual comparison between tree-rings. This method was developed by dendrochronologists working with trees in the South-West of the US. According to the textbooks written by these authors, skeleton plotting consists of plotting by hand the length of the successive narrowest rings of each core (the "marker rings") onto graph paper. The decision of narrowness is based on a process that authors refer to as "mental standardisation", whereby for each core, the dendrochronologist assesses the absolute individual growth of each ring in terms of its relative similarity to the three rings on either side of it.[156] These dendrochronologists achieve cross-dating by sliding one skeleton plot past another to look for synchronicity. When I ask Miloš if he has ever used skeleton plotting, he emphatically says "No" and explains that unlike in the Southern US, trees in Scotland do not produce sensitive tree-rings that can be compared visually. Miloš adds laughing, "Also, if I had to draw a plot for all the samples we have, I would not be able to finish my PhD in three years!"

The reasons that Miloš gives for not employing the qualitative and manual method of cross-dating are in line with the historical trend in the field of dendrochronology. In the context of a growing community of dendrochronologists[157], researchers believe that skeleton plotting hinders the exchange of data between them. In their textbook published in 1969, Mervin Stokes and Terah

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[155] This situation is similar to 17th century English science, Steven Shapin explains that the residences of gentlemen were places of scientific work in "The House of Experiment in Seventeenth Century England", *Isis*, 1988, Vol. 79 (3), p. 373.

[156] Speer, *Fundamentals*, p. 13.

[157] In his textbook published in 2010, James Speer reports that over the past 50 years the number of publications on dendrochronology "have risen exponentially"; multiple international organisations and journals on tree-ring research have become established; the internet forum has more than 600 members from 32 countries; and more than 2,000 chronologies are archived in the International Tree-Ring Data. See Speer, *Idem*, pp. 40-41.

Miley, claim that "while skeleton plotting technique is an excellent tool for tentative dating, it is an unsatisfactory form for permanent storage or transmission of data". [158] Also, skeleton plotting is perceived by dendrochronologists to devalue the status of their knowledge as scientific. In a collective textbook published in 1992, J.R. Pilcher claims, in relation to skeleton plotting that "although the human brain is very efficient at cross-dating, the process lacks the objectivity demanded of a scientific discipline".[159] As a result, throughout the 1970s and 1980s, dendrochronologists developed computerised and quantitative methods for cross-dating that are able to replicate plots created by humans. In his textbook, Jim Speer writes that the fact the skeleton plotting can be duplicated by a computer program shows that "it is not a purely subjective process".[160]

In 1983, Richard Holmes created COFECHA, which is the statistical package most widely used nowadays for cross-dating by dendrochronologists, including Miloš. In his paper, Holmes emphasises that COFECHA is meant to be an "aid" to validating (manual) forms of cross-dating like skeleton plotting.[161] Quantification is seen by Michael Baillie as a solution to the problems with the external credibility of dendrochronologists; he says, "It is not that individuals cannot find the correct matching visually; they can and often do with considerable expertise. The problem for the observer, for example the archaeologist, is in knowing whether any particular dendrochronologist possesses the ability, hence some mathematical quantification of each visual match is necessary".[162] As Miloš tells me, numbers also resolve the challenges of working with samples, like the ones in Scotland that do not produce very distinctive ring patterns.

The predominance of computerised and statistical methods for cross-dating is a concern for many dendrochronologists. Jim Speer, in his 2010 textbook, complains, "COFECHA was never intended to be the only attempt to date a sample of wood or to replace cross-dating".[163] Speer illustrates the dangers of relying exclusively on quantitative methods by offering a personal vignette. He once attended a conference where he met a colleague and asked about his dating. Speer says that "he replied that he had not yet checked the quality of dating with

[158] Stokes and Smiley *An Introduction to Tree-Ring Dating, p.*53.
[159] Cook and Kairiukstis, *Methods of Dendrochronology, p.*46.
[160] Speer, *Fundamentals*, p. 13.
[161] Richard Holmes, "Computer-Assisted Quality Control in Tree-Ring Dating and Measurement." *Tree-Ring Bulletin*, 1983, Vol. 43.(1), pp. 69-78.
[162] Michael Baillie, *Tree-Ring Dating and Archaeology*, p. 81.
[163] Speer, *Fundamentals*, p. 116.

COFECHA and gave no indication that the samples were dated by any other means". Speer concludes that "because of this lack of time spent dating the samples, the researcher made an inaccurate conclusion and extrapolated it to the hardwood forest".

In the dendrochronology field week course I attend in Tasmania, a senior dendrochronologist whom I will call Bob and is an expert in skeleton plotting, complains that students will not do any "real cross-dating". These field weeks are training courses that combine educational and recreational activities such as fieldwork, evening talks, presentations, dinners and parties whereby neophytes get to learn from reputed dendrochronologists like Bob, different aspects of dendrochronology. One of the key aspects that we learn is tree-ring dating. Bob is concerned that students like me will be taught the technique of skeleton plotting just for "illustration" purposes to show how cross-dating works, but will not use skeleton plotting ourselves for the project that we have to conduct as part of the training. Bob's concerns are justified. I observe that students are first trained to crossdate samples entirely without a computer until they become familiar with the procedure.

At the root of many senior dendrochronologists' concerns lies the fear that students will employ computer programs as "black boxes" and will not develop the necessary judgement to know whether the correlation coefficient is evidence of "real" cross-dating. In his 1995 book, Michael Baillie is adamant that correlation coefficients should be used as a "guide" and that the final decision must always rest with the dendrochronologist.[164] Similarly, as early as 1943, Andrew Douglass writes that "there is no mechanical process, no rule of thumb, no formula, no correlation coefficient, to take the place of this personal comparison between different ring records; the operator does not dare to seek relief from his responsibility."[165] The relationship between quantification and expert judgement is not of a sum-zero, but of complementarity; Baillie explains that "a dendrochronologist's suggested match, if *not* backed up by a signification computer correlation, may well be suspected!"[166]

As a member of his community, Miloš shares the concerns about statistical cross-dating and the need to develop an understanding of what the statistics really mean. In his work, Miloš engages in critical evaluation of the correlation statistics that both COFECHA and CDendro produce for the

[164] Michael Baillie, *A Slice Through Time: Dendrochronology and Precise Dating* (London: Batsford, 1995), p. 21.
[165] Alexander Douglass, "Notes on the Technique of Tree-Ring Analysis", *Tree Ring Bulletin*, 1943, p. 7.
[166] Baillie, *Tree-Ring Dating and Archaeology,* p. 85.

agreement between sections of tree-ring measurements among samples. Miloš' approach can be summarised by his constant advice to me that "you cannot allow statistics to manipulate you".

In particular, Miloš is wary of the recommendations that both CDendro and COFECHA make of "best" statistical matches. Both packages suggest an "offset" number of missing or false rings that, if included or subtracted, increases the synchrony among rings and the level of correlation. Miloš tells me that both programmes establish 0.33 as the default correlation coefficient threshold, but he is not content to accept this threshold as evidence of successful tree-ring dating. "You cannot just go with what the program tells you to include or to remove", Miloš says, "You need to understand how reasonable the recommendation made by the computer programs is".

The criteria of "reasonability" that Miloš employs to decide whether a statistical match can be accepted as evidence of a "real" match depends on whether he is working with cores or subfossil wood. With samples from living trees, Miloš starts by comparing all the measurements of pairs of replicated samples (for instance, 1A against 1B; 2A against 2B) with the expectation that their ring patterns must be very similar because both samples are from the same tree. After all these multiple comparisons, Miloš says he can "have a feel" for the correlation benchmark he can expect for the samples from this specific site. Miloš uses this threshold to distribute the datasets of individual measured cores into what he calls the "good" and "bad" folders. The good folder includes those individual measured cores (for instance, 2A, 3B, 10A..) that Miloš thinks have an adequate quantitative match against the average of cross-dated pairs of replicated samples (the average of 1A-1B; 2A-2B;3A-3B...) or "master chronology". The iterative comparison and matching of tree-rings between new and replicated samples is a process that Miloš calls "chronology building". In front of the computer screen, Miloš describes to me how this process works and how to interpret the two graph lines that CDendro generates:

1. Meritxell: How do you know if these two chronologies crossdate?
2. Miloš: Well, first I look at how well these {points to screen} two lines match. I have the correlations below to know exactly how similar the measurements are.
3. (pause of 3 or 4 seconds)
4. Miloš: You see, here {points to screen} the correlations break down and the lines do not match so well.
5. Meritxell: How do you interpret this?
6. Miloš: There could be many explanations. Maybe we have not measured a ring...

7. {Miloš opens a file of a digitised image of a core, and he zooms out and zooms in the image}
8. Miloš: Mm
9. Here {points to digitised image} is where the correlations break.
10. Do you see any other ring that I don't see?
11. Meritxell: I don't think so
12. Miloš: Yeah, I think there isn't any problem with the measurements.
13. Meritxell: So, what else do you think could be the reason for this low correlation?
14. Miloš: Maybe there's a missing ring that we don't see
15. Meritxell: How do you know that there is a ring if you don't see it?
16. {Miloš laughs}
17. Miloš: Yes, it sounds a bit weird
18. Miloš: Well, CDendro says so
19. If I add two rings...
20. {Miloš uses the handheld device and the cursor to click twice at the section of one of the graph lines where there is a low correlation}
21. You see {Miloš points to a little box with numbers and the signs of plus and minus}, the correlations are positive and the lines match
22. (pause of 3 or 4 seconds)
23. Miloš: But I won't be adding any rings just yet.
24. {Miloš opens a file of the digitised image of a core}
25. Miloš: I don't see anything strange going on here
26. {Miloš points to the section of the digitised image where the correlations break}
27. Miloš: There are no compressed rings or anything
28. Miloš: I am going to leave this core to the bad folder for the moment and come back to it later when I have seen the other ones.

Whilst Miloš is doubtful about the possibility of finding pattern-matching, the work of cross-dating is driven by the expectation that the lines *should* match. Miloš employs a series of interpretative repertoires and solutions for the asynchronies we observe. In CDendro, Miloš notices a "mismatch" at sections of the graph where the two lines representing the measurements of the individual core and the correlation coefficient are deemed to be exceptionally low. When this happens, Miloš always goes back to the digitised image and tries to identify those sections of rings where the statistical correlations "break" or the asynchrony occurs. As in the dialogue above, Miloš sometimes does not find any measurement problem. At other times, he discovers that he or someone else (me) has missed measuring a ring and includes a new measurement point. He also often re-measures the section if there are very narrow or diffuse rings, or what Miloš calls "compressed areas". After making some re-measurements, Miloš does not expect to achieve a perfect matching (correlation of 1) between cores from different trees. He argues that "trees never grow in the same way", thus he sees imperfect correlations as a more "authentic" expression of the diversity among individual trees and the "messiness" of tree-ring patterns.

Only after checking for measurement errors and seeing no improvement in correlations does Miloš consider the possibility of the existence of false and missing rings, recommended by the statistical software as "offset years". Miloš' concern in adjusting tree-ring chronologies for false and missing rings is "not to force the data" and to make corrections that are in agreement with what he sees on the wood. "If you see a compressed section of rings", Miloš explains to me, "It is plausible to think that there could be an extra missing ring that we can't see". Alternatively, Miloš explains, "[I]t does not make sense to take out one false ring in places where you don't see diffuse rings". Miloš infers where rings may be absent or duplicated by identifying the exact location of an asynchrony among rings. The ring count would be one year off after this point, unless Miloš corrects this lack of coincidence by inserting or removing a ring or a measurement point at the place in the sequence. In this way, the existence of false and missing rings helps Miloš to "normalise" the asynchronies among ring width patterns and achieve cross-dating.

The other factor that determines the success of cross-dating - besides the expectation that cross-dating is possible - is the structure of the wood. It is precisely for this reason that dendrochronologists in general struggle to crossdate rings from tropical trees. The diffuse ring boundaries produced by the climate in tropical regions is currently an impediment to identifying annual tree-rings of growth, and Jim Speer in his textbook refers to the possibility of cross-dating tree-rings from tropical trees as "frontiers of dendrochronology".[167]

In Scotland, Miloš occasionally finds samples that he does not manage to crossdate. If he finds series of measurements that require "too many tweaks" or adjustments, he considers them an anomaly and "uncrossdatable". This is the case with the samples from the "Alladale" site in the North- West of Scotland. As I was responsible for producing the measurements of Alladale, I am concerned that my skills are in doubt. To my relief, after making some adjustments, Miloš decides to blame "nature" rather than me as "the Alladale site is, on the whole, exceptionally

[167] Speer, *Fundamentals,* p. 253.

noisy", he says. Miloš explains that this site has been affected by extensive logging in the past, which could explain why he is unable to match tree-ring patterns. Miloš tells me, "[W]e have to accept that certain trees just don't crossdate".

Miloš resorts to different solutions depending on the scope of the anomaly. In the case of the Alladale site where a large number of samples do not crossdate, Miloš and Rob are developing a specific methodology to eliminate the widespread "distortion" that logging has on tree-ring patterns (Chapter 5). With individual "difficult" samples that cannot be dated, Miloš excludes them from the master chronology. In line with the dendroclimatologists' maximisation aim of creating a good climate signal, Miloš argues that "what matters is that the average chronology stays strong". Thus, excluding series of measurements that do not date with the master or averaged chronology and could damage the overall climate signal of chronologies is seen by Miloš as normal. Because of the principle of replication, Miloš believes that other replicated and similar samples will compensate for the excluded ones. Rob insists on the point that they have not been able to create any data or tree-ring chronologies from a "small minority of living samples" and that "we have used ALL the data we have developed for Scotland".

With subfossil samples, Miloš warns me of the higher risk that statistical matches are "spurious" and the need for "extra-checks" to make sure that correlations are real crossdatings. Miloš uses the metaphor of a "jigsaw" to explain that in the creation of tree-ring chronologies from subfossil wood "anything could match with anything". Miloš ask me to imagine that "you've a jigsaw of 10,000 pieces, but the picture you're creating only involves some of those pieces, which are the subfossil samples that we have from Loch Gamhna. You might be only able to create a little part of the jigsaw out of the thousands of pieces. But you don't know what pieces you have; you don't know how they fit together or if they actually fit together, and to be precise, you don't know either what the total number of jigsaw pieces should be. In fact, every year, this number gets bigger and bigger as we collect more samples." Miloš' main doubt is that subfossil samples do not necessarily come from the same forest or tree (logs could have been dragged from other areas and end up in the lake) and he cannot assume that cross-dating will occur.

Miloš partly resolves the uncertainties surrounding the dating of subfossil samples by relying on a technique called "carbon dating", which offers an estimate of the years of the wood based on the analysis of the concentration and decay of radioactive carbon isotopes (14C). The calculation of these estimates requires expensive machinery and specialised skills that Rob and Miloš do not possess. Rob sends a selection of samples to an external laboratory that specialises in producing carbon dates and pays a considerable amount of money (£300 per sample) for them.

When I ask Miloš a breaching question about whether he trusts these estimates to be correct, he looks at me, visibly surprised, and he says, “Of course!” After a few seconds of silence, he explains, “The methodology involved is pretty robust; it’s a whole area of science. We don’t do it ourselves. It’s not part of our job. We just need to know what it is and how to interpret it”. A few seconds later, without any further probing from me, Miloš gives me a five-minute explanation of the methodology of carbon dating and its uncertainties. He concludes his explanation by referring to a few articles that “you can read if you want more information”. As I am very surprised by the length of Miloš’ explanation, I ask him why he thinks it is important for me to know all this detailed information. Miloš responds, “[B]because you asked before whether I trust these dates, and I think it’s important to show you the methodology to obtain them and all the uncertainties behind them”.

One reason that might explain why Miloš is relatively unconcerned with the black boxed nature of carbon dating is that carbon dating is not the only source of evidence for cross-dating. Miloš triangulates and compares carbon dates against the cross-dating results that he has agreed on with Rob. At this stage of the creation of tree-ring chronologies, Miloš seeks Rob’s help as a “double-check” to establish the exact dates of subfossil wood. Essentially, Rob and Miloš replicate each other’s chronologies: each creates subfossil chronologies separately on the basis of common criteria, and they compare the resulting chronologies to see whether they agree. Rob and Miloš place those that are not in agreement into the “undated pile”.

Rob has created a web-based spreadsheet where he and Miloš can simultaneously upload and discuss the dates of their replicate chronologies (image 21). One of the columns of the document includes the carbon date (“14C date) and the degree of similarity between chronologies (“Agreement with Miloš?”). Miloš explains that over time he and Rob have decided to use a more “stringent” correlation coefficient (above 4.0, Baillie T-value) for accepting the overlap of subfossil samples over a long period of time to be an indication of a real dating. In early 2015, I learn that Rob has adopted another “stringent” requirement, which is to expect a long overlap in time between correlations from two types of tree-ring measurements (ring-width and Blue Intensity). He explains that “Miloš has been replaced by Blue Intensity”. This additional requirement is meant to give another form of validation to Rob that the statistics reflect an accurate dating.

Image 21. This table shows the negotiation between Miloš and Rob about cross-dating tree-rings of subfossil samples (“Agreed with Miloš”, right column) as well as their reliance on carbon dating (“14C” date columns).

	A	B	C	D	E	F	G	H	I	J	K	L
1	FLO									95% C.L. Calender Date range		
2	COF	Sample	Cal BP (1950) from C14	No. of measured rings	Pith Offset	Floating Chrons	Xdate innner	Diff between Cal and 14C date	Mean 14C date	Early	Late	Agreement with Milos?
3	2008	LEW3	7859	74		Float3	-5846	-62.5	-5,909	-6007	-5810	Agreed with Milos
4	2008	LEW7	7850	80		weak XD with Float3			-5,900	-6001	-5799	
5	2008	LEW5	7845	56 and 39	35?	Float3	-5839	-56	-5,895	-6002	-5788	Agreed with Milos
6	2008	LEW2	7839	40		Float3	-5855	-33.5	-5,889	-5991	-5786	Agreed with Milos
7	2008	LEW4	7825	130		Float3	-5875	0.5	-5,875	-5984	-5765	Agreed with Milos
8	2008	LGW10	7807	84 and 120		Float3	-5894	37	-5,857	-5976	-5738	Agreed with Milos, but possibly missing ring in 79 from pith?
9	2012	EIN14	7753			Ein2			-5,803	-5886	-5720	
10	2008	LGW17	7749	76		no XD with anything			-5,799	-5890	-5707	
11	2012	GAM85x	7349			Float9	-5399	0.5	-5,399	-5476	-5321	Agreed with Milos
12	2008	LGW8	7346	108		Float9	-5395	-0.5	-5,396	-5472	-5319	Agreed with Milos
13	2008	LGW2	7126	169 and 164	40+?	Float11	-5176	0	-5,176	-5301	-5051	Agreed with Milos
14	2012	EIN1	7121			Ein1			-5,171	-5293	-5049	
15	2012	LEF118	7022			Float12	-5072	0.5	-5,072	-5207	-4936	
16	2012	LGF219	6855			Float12	-4996	91	-4,905	-5011	-4799	
17	2008	LGW9	6076	102 and 133		Float17			-4,126	-4255	-3996	
18	2012	GAM76	5873			Float5	-3922	-0.5	-3,923	-4037	-3808	

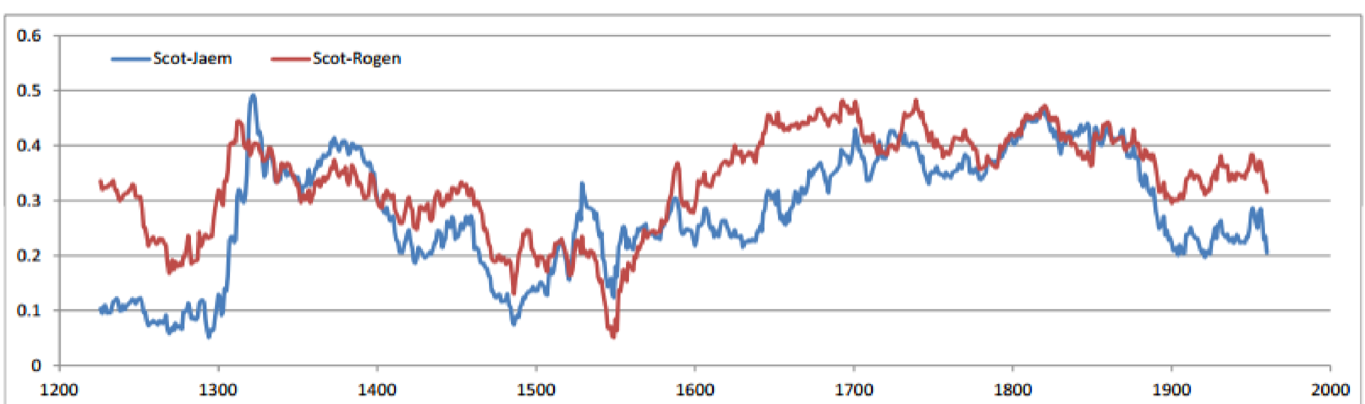
Rob and Miloš’ ultimate purpose is to find a long and strongly correlated overlap between the so-called “floating chronologies” from Loch Gamhna and Loch an Eilein and the chronologies from living trees in the Cairngorms. By overlapping successively older tree-ring chronologies, Rob wants to extend Hughes’ original chronology. As I shall describe in Chapter 6, the importance of creating long tree-ring chronologies is crucial for dendroclimatology: the longer the chronology, the longer the climate reconstruction. Rob is keen to create a tree-ring chronology that goes back to 1413, which is the year that St Andrews University was established. With the prospect of the upcoming 600th anniversary celebrations in St Andrews in 2013, Rob hopes that achieving a temperature reconstruction for the time the university was founded could help to promote his work among university officials.

Creating a long tree-ring chronology is perhaps the most important source of reputation for dendrochronologists. A long chronology takes years of work. Even though long tree-ring chronologies are the result of the effort of many people, chronologies become associated with individual dendrochronologists. The names of the most famous dendrochronologists are associated with the names of their chronologies: “Douglass’ Aztec-Pueblo Bonito chronology”, “Baillie’s Irish oak chronology” and the “Schweingruber network”. On a couple of occasions, I hear people referring to “Wilson’s Scottish chronology”. When Rob meets his colleagues in conferences and meetings, they monitor the length of his chronology and often ask him: “How far are you back to now?"

In January 2015, Rob sends an email to the members of the Scottish Pine Project team, announcing that he has finally achieved the creation of an 800-year-long chronology for the Scottish Highlands. In the last few months, while Miloš has been busy finishing his thesis and working on other aspects of the climate reconstruction, Rob has been trying to crossdate subfossil samples that remained undated or "floating". For months, Rob has been unable to find a satisfactory overlap between non-dated chronologies of subfossil wood and living chronologies in the period from the 1440s to the 1540s. "This is the period of weakest replication and has been a headache for me over the past few months", Rob explains in the email. "To fill the gap" for this period, Rob had decided that he would focus the previous 2014 fieldwork expedition on generating replicate subfossil samples from Loch an Eilein and Loch Gamhna.

In the email, Rob explains how he uses the synchronicity of geographically distant tree-ring chronologies as a confirmation of the dating of the Scottish chronology. Rob explains that "the process has been facilitated by the fact that reasonably replicated Scottish BI chronologies 'crossdate' with Jaemtland MXD and Rogen BI data in central Sweden." (Image 22). The "BI chronologies" are tree-ring chronologies generated with reflectance measurements, whilst the "Jaemtland MXD" and "Rogen BI" are the chronologies that Björn - the Swedish member of the Scottish Pine Project team - created from Scots pine years ago.

Image 22. In the email that Rob sends to the Scottish Pine Project team, he illustrates his confidence in the dating of an 800-year chronology of the Scottish Highlands by comparing its synchronicity against two other independent Swedish chronologies. (Scot-Jaem and Scot-Rogen).



3.3 Discussion

Rob and Miloš resolve the numerous uncertainties involved in the production of tree-ring chronologies by simultaneously trusting and subjecting to scepticism the method and the people who carry out cross-dating.

Cross-dating is an instance of a fiduciary framework, insofar as it is trusted by all dendroclimatologists as the foundational "principle" of dendrochronology. The production of tree-ring chronologies depends partly on dendroclimatologists' expectation that cross-dating is possible. Miloš' individual work of cross-dating, including the inference of the existence of missing and false rings, is sustained by this collective expectation shared by all dendrochronologists. This fiduciary framework is the basis for, and is constituted through dendrochronologists' development of longer and more heavily corroborated chronologies. The possibility of cross-dating first within the Scottish chronologies and later against the Swedish chronologies is partly based on the entanglement of trust relations between individuals like Rob and Björn who regard each other as competent dendrochronologists and producers of tree-ring chronologies. Dendrochronology, as a collective assemblage of tree-ring chronologies, is the result of these self-referring and reinforcing ties of personal trust and knowledge.[168]

Yet, the work of cross-dating and the possibility of extending the Scottish chronology is not only the result of Miloš and dendrochronologists' self-fulfilling beliefs. The fact that Miloš admits with resignation "We just have to accept that some samples just don't cross-date" (in reference to the samples from the Alladale site or the tropics) is an indication that cross-dating and the nature of the fiduciary framework is also determined by the features of the natural world, as well as by the features of the social world (trust relations). The fact that pattern-matching has been consistently achieved across many different tree species and regions reinforces not only the dendrochronologists' trust in the principle of cross-dating, but crucially, their trust on the fundamental assumption on which cross-dating and dendroclimatology are based, namely that "the prime suspect" behind the common patterns of tree-rings (as Malcolm Hughes puts it) is climate.

[168] This conclusion draws directly upon Barry Barnes' account of social institutions as "bootstrapped inductions" or self-fulfilling prophecies. Barnes, Barry. "Social Life as Bootstrapped Induction". Drawing from Barnes' account, I would argue that tree-ring chronologies are social institutions in the sense that they are self-referential and normative.

Crucially, the existence of well-dated tree-ring chronologies is the result of subjecting the fiduciary framework and the principle of cross-dating that sustains the production of these chronologies to civil and organised scepticism. No competent dendrochronologist would ever accept a tree-ring chronology simply because previous generations of dendrochronologists have concluded that cross-dating is possible unless he or she can be sure that the specific tree-ring chronology has been subjected to appropriate scrutiny. Over time, as the community has grown in number and geographical distribution, dendrochronologists have developed and used different quantitative and computerised methods and technologies (COFECHA and correlation coefficients importantly) that have allowed them to practice scepticism with each other's work at a distance. Likewise, no competent dendrochronologist would ever accept a chronology that is simply the result of ring-counting. As Rob insists, dendrochronologists, unlike foresters, "don't count, but crossdate". The seemingly widespread insistence by dendrochronologists that tree-ring dating is not ring-counting is an indication of the way scepticism is not only vital to the construction of collectively-accepted chronologies, but also serves the larger purpose of establishing the identity of dendrochronology as a distinctively scientific enterprise.

The three stages that I have identified in the creation of the Scottish tree-ring chronology (interpretation, representation and comparison of tree-rings) essentially consist of exercises of civil scepticism. With regards to interpretation, Miloš asks me as a neophyte to count and mark each ring individually to make sure that I examine each ring carefully. I also consistently interrogate the boundaries of each tree-ring as exemplified by the recurrent question ("Is this a ring?") that I ask Miloš in the laboratory. In the task of representing tree-rings, Miloš evaluates the correctness of my measurements against his and he insists that I should double-check that the automatic method of measurement has represented tree-rings accurately. Miloš himself tests the mediating effects of different technologies like the scanner and CooRecorder. Regarding the comparison of tree-rings, Miloš' advice to me is that "you cannot allow statistics to manipulate you"). Miloš' scepticism of statistical methods seems to be part of the training of neophyte dendrochronologists more generally, at least in the field week in Tasmania, where students are asked to generate skeleton plots by themselves before using automated software. The purpose of this activity is to make sure that students are not excessively trustful of the software and that understand the plausibility of correlation coefficients. Miloš and Rob also engage in sceptical replications of each other's cross-dating work with subfossil wood.

All the sceptical interactions I have detailed above between students and teachers in the process of tree-ring dating are parasitic on existing trusting relationships. I learn and I become relatively competent at preparing samples; distinguishing rings from non-rings, and producing "proportional" and "accurate" ring-width and reflectance measurements by trusting the authority of a more expert dendrochronologist like Miloš, who is himself trusted by Rob as a competent teacher. As a doctoral student himself, Miloš trusts his supervisor Rob as a very competent partner to discuss via the online spreadsheet the particularly difficult process of cross-dating subfossil wood. All these examples of instructions suggest that training is crucial for ensuring that students are competent at producing the work that their community expects them to do; this includes being competently trustful and, crucially, sceptical of the "right" things.

Miloš is aware that there are necessarily limits to his scepticism and he has to trust others to exercise the relevant exercises of scepticism for him. In particular, Miloš trusts the accuracy of the microscope, the carbon dating, and the calibration card among many other aspects of tree-ring dating because he knows that these technologies have been certified by specialised laboratories and generations of scientists (or as Miloš claims in relation to carbon dating, "it's a whole area of science!"). The alternative to trusting all these auxiliary technologies is practically unfeasible, as Miloš lacks the expertise to practice scepticism by himself. Miloš explicitly acknowledges this point in relation to calibration cards when he says "Even if I detected some problems, I would not know how to fix them, so I prefer to use this card as it is". If Miloš was constantly sceptical about whether calibration cards experts are properly sceptical, he would not be able to use their cards, so Miloš ends up trusting his colleagues to be sceptical, which brings us back to the parasitic view of scepticism. By using carbon dating and calibration cards, Miloš honours the relationships of trust he has established with experts whom he entrusts to be adequately sceptical, as well as with peers who might also suspend their scepticism, which altogether form the basis upon which Miloš is able to use carbon dating and calibration cards for the creation of tree-ring chronologies.

To become trusted as a competent dendrochronologist by his supervisor Rob and his peers, Miloš needs to display that he is a competent sceptic and has subjected the chronologies to appropriate scepticism. Miloš is aware of the scepticism of his colleagues towards the use of the scanner and CooRecorder, and he seeks to pre-empt their criticism through different instances of sceptical display. One form that Miloš uses to enact his scepticism is through experimentation and the practice of scepticism. Miloš justifies to me the tests he conducts with the scanner and CooRecorder because "in order to create a more robust methodology I need to identify its biases".

Another conventional form of sceptical display is expressed verbally as “scepticism-as-an-account” when Miloš answers my breaching question about carbon dating. My question and my presence triggered Miloš to articulate the reasons for his suspension of scepticism about the uncertainties regarding carbon dating. By showing to me that he is aware of the uncertainties of carbon dating identified by experts, Miloš seeks to restore any potential “breach of trust” that my question could have produced. In his own words, Miloš wants to “show” me that his routine trust in carbon dating is communally justified and justifiable by referring to relevant literature (hence his comment to me that “You can read if you want more information). This sceptical enactment is, in turn, parasitic on the trust that Miloš places on carbon experts.

4 Tree-Ring Parameters

4. 1. The Generation of Estimates of Climate

Rob and Miloš often refer to trees as "climate proxies". Scientists more generally talk about climate proxies as sources of climate information that stand for what they consider to be the most "direct" source, namely meteorological records. In many countries, people did not start taking systematic measurements of temperature and precipitation until the late 19th century. Thus, scientists use different forms of climate proxies for generating knowledge about past climates prior to the existence of meteorological records. The sciences of paleoclimatology - of which dendroclimatology is a part of - use "natural" climate proxies (trees, ice cores, sub-fossil pollen, corals, speleothems and lake and ocean sediments) and "man-made" climate proxies (historical documents and phenological records) to produce knowledge about past climates.

The dendroclimatologist Harold Fritts uses the metaphor of a "window" to explain how certain properties of tree-rings can be used as sources of climatic information. He writes that "a tree can be thought of as a 'window', which, by means of physiological processes, passes and converts climatic input into a certain ring-width output that is stored and can be studied in detail, even thousands of years later".[169]

Historically, dendroclimatologists have developed three methods for deriving climate estimates from the growth of tree-rings. These methods are based on three distinct physical properties of tree-rings: ring-width, wood density, and concentration or relative abundance of isotopes. The development of tree-ring parameters is a history of dendroclimatologists' search for complementarity, as for decades they have striven to develop new parameters that complement and resolve the limitations of existing ones.

For a long time, ring-width has been the main parameter for deriving estimates of climate from trees. Andrew Douglass and his students Waldo S. Glock, Edmund Schulman and Harold Fritts, working at the University of Arizona, are acknowledged in all textbooks as the first dendrochronologists to employ ring-width for dendroclimatological purposes during the first half of the 20th century. Before then, dendrochronologists had mainly employed ring-width chronologies to date historical buildings. Dendroclimatology pioneers worked with trees growing in semi-arid and

[169] Fritts, *Tree Rings and Climate*, p.238.

low elevation sites in the North of Arizona. They reasoned that trees growing in these conditions grow faster when the monsoon rains come in late summer, and thus, the width of tree-rings is a reflection of rainfall changes.[170] Over the years, dendroclimatologists have concluded that ring-width data are particularly valuable for deriving information about long-term centennial temperature or precipitation trends (what dendroclimatologists call "low-frequency" because the variability of climate is "low").

One of the main advantages of producing ring-width data is its relative cheapness. The inexpensive equipment needed for producing such data (a measuring stage, a microscope and a computer) is likely one of the factors behind the growth in the number of tree-ring chronologies and tree-ring laboratories during the last few decades. Rob's own biography exemplifies the importance of the cheapness of the ring-width parameter to the establishment of professional careers. During the two years that Rob worked as a geologist in Regensburg to support his family, he managed to conduct dendrochronology with his "domestic" and affordable tree-ring laboratory. In collaboration with a German geographer and forester, Rob created a few ring-width chronologies from Norway spruce trees growing in the Bavarian Forest Region that became the basis of Rob's doctoral thesis and his first publication in 2001[171]. Rob explains that the amateur work he conducted in Germany was crucial to his decision to pursue a career as a dendroclimatologist. By the time Rob started his doctorate in 2000 at the University of West Ontario, dendroclimatologists had already identified a few of the limitations of ring-width data. These limitations are related to the fact that the ring-width parameter is only useful when trees produce distinctive tree-rings, which is everywhere except the tropics more or less.

The new density parameter is acknowledged to have been developed by Polge in the 1960s and subsequently developed by Fritz Schweingruber in the 1970s as an alternative when creating climatic estimates from trees growing in the cold, moist and high altitude areas of the Swiss Alps where Schweingruber worked.[172] Unlike trees growing in Arizona, Alpine trees do not produce wide and distinct sequences of tree-rings. The calculation of density is based on the cell wall

[170] Edmund Schulman, Tree-Ring Indices of Rainfall, Temperature, and River Flow, *American Meteorological Society*, 1951; Andrew Douglas, "A Method of Estimating Rainfall by the Growth of Trees", *Bulletin of the American Geographical Society*, 1914, Vol.46 (5), pp.321-335.

[171] Rob Wilson and M Hopfmueller, M., "Dendrochronological investigations of Norway spruce along an elevational transect in the Bavarian Forest, in Germany", *Dendrochronologia*, 2001, Vol.19, (1), pp. 925-936

[172] Schweingruber, *Tree Rings.*

thickness and the percentage of vessels within the ring; these tree-ring properties are known to be related to climate by Schweingruber and tree physiologists more generally. The "latewood" or darker portion of the ring develops during the warmer summer season, whilst the "early wood" or less dense part of the ring grows when it is colder in the spring. In his textbook, originally published in German in 1983, Schweingruber describes two types of density measurements, the "maximum" and "minimum" density. The former (often abbreviated to "MXD"), which measures the thickness of cell walls in the latewood, is the most climatologically useful density parameter for trees growing in temperate regions. Schweingruber and others realised about the value of maximum density by comparing density measurements against temperature data, and finding positive correlations.

The standard methodology developed by Schweingruber to generate density data - known as the "Schweingruber protocol" - requires expensive machinery and specialised skills. As Schweingruber describes in his textbook, dendroclimatologists first need to treat wood samples chemically to remove the resin in the wood cells that are known to bias the measurement of density. Then, dendroclimatologists must cut the samples into laths, or very thin flat strips of wood, which are X-rayed and analysed for their cell density with a densitometer.

Few tree-ring laboratories in the world can afford the machines and the workforce to produce density chronologies. Björn's laboratory (the Swedish member of the Scottish Pine Project) at the University of Stockholm is one of them. The tree-ring laboratory in Western Ontario where Rob carried out his doctorate did not have the facilities to produce density data and sent the samples to two external dendrochronology laboratories known as "Lamont" and "WSL". In his master's dissertation, Rob generated both ring-width and density data from Engelmann spruce samples in the British Columbia. Rob's results published with his supervisor Brian Luckman were seen at the time as a contribution to the development of density. For a long time, dendroclimatologists believed that, contrary to ring-width data, density data was better at providing information about short term yearly weather changes ("high-frequency" because of the "high" climate variability). Rob showed that in the case of samples from living Engelmann spruce trees in the British Columbia, density could provide both good low-frequency and long-term climatic information. [173]

[173] Rob Wilson and Brian Luckman, "Dendroclimatic Reconstruction of Maximum Summer Temperatures from Upper Treeline Sites in Interior British Columbia, Canada." *The Holocene*, 2003, Vol. 13.(6), pp. 851-861; Rob Wilson and Brian H Luckman, "Tree-Ring Reconstruction of Maximum and Minimum

As with density, the production of data from the concentration of isotope is relatively expensive. According to their written methodological accounts[174], dendroclimatologists first need to decompose and combust samples through heating. They then measure the isotopic ratios of the gases with machines called "mass spectrometers". The use of isotopic measurements in dendroclimatology is based on the assumption that variations of carbon, oxygen and hydrogen within tree-rings are related to environmental factors, such as temperature and precipitation. This method for creating estimates of past climates is sustained by a complex body of theories and models of isotope fractionation that explain how plants and trees transform the input from the natural environment into ratios of isotopes. [175]

According to some dendroclimatologists that I spoke to, isotope dendroclimatology is regarded as the most sophisticated tree-ring parameter. When I sit in the "isotope session" in the Melbourne conference, I realise that, unlike in other density or ring-width talks, I do not understand the formulas and acronyms of the isotope researchers. When I share my bafflement with a junior isotope dendroclimatologist sitting next to me, he responds “Oh well, this is why the other dendros [dendrochronologists] call us the ‘isotope gang’”. When I ask Miloš if he has ever done any isotope work, he confirms, “No, isotopes are another world”.

A few dendroclimatologists, including Rob, are openly doubtful about isotope dendroclimatology. He says, “How can you be sure about anything at all when the isotope guys only use a few samples to create a chronology?” For Rob, the fact that there are few isotope chronologies available –because of the high cost of production of its methodology - makes the value of isotope data “yet to be known”. In his review about the “state of art” of dendrochronology's contributions to climatology, Malcolm Hughes asks about isotope measurements from tree-rings: “Do they contain different information to that in the much cheaper measurements of ring widths and wood density? If the answer to either or both of these questions is no, why use them as paleoclimate records?[176] In an ensuing article, experts in isotope dendroclimatology respond directly to Hughes’ question when they write, “Isotope dendroclimatology will not really produce anything that could not be produced more cheaply and

Temperatures and the Diurnal Temperature Range in British Columbia, Canada." *Dendrochronologia*, 2002, Vol. 20 (3), pp. 257-268.

[174] Danny McCarroll, and Neil J. Loader, “Stable Isotopes in Tree-Rings”, *Quaternary Science Reviews*, 2004, Vol. 23(7), pp.771-801.

[175] Danny McCarroll, Danny and Neil J. Loader, *Idem*.

[176] For instance, Malcolm Hughes expresses his scepticism in "Dendrochronology in Climatology – the state of the art.", *Dendrochronologia*, 2002, Vol. 20.(1), pp. 104.

easily by using ring-widths and densities”. [177] Yet, these same co-authors defend isotope data has having the "potential" to provide climate information for trees growing in tropical and mid altitude areas where traditional tree-ring parameters do not work. At these locations, dendroclimatologists have rarely produced estimates of climate from ring-width or density data because trees show “complacent” and homogenous tree-ring patterns (Chapter 2).

Whilst Rob believes that density is the most reliable tree-ring parameter for extracting climate data from trees growing in Scotland, he does not have the necessary resources to adopt density as the main tree-ring parameter in the Scottish Pine Project. Rob employs the density data generated three decades ago by Malcolm Hughes, which are archived in the ITRDB. Also, Rob’s collaboration with Björn is based on the agreement that Björn and his students based in Stockholm will generate new density chronologies from the Scottish samples.

In parallel to the generation of new density datasets, Rob decides to explore an alternative methodology for deriving climate estimates from trees, the “Blue Intensity” or “blue reflectance” methodology (Rob and Miloš initially used these two names interchangeably but eventually decided to use Blue Intensity and this is the term I use). Rob first learnt about Blue Intensity (often abbreviated as BI) in 2004, while he was working with dendroclimatologists from Swansea University (UK) in the same European project where he met Björn. During the 2000s, the Swansea researchers had been experimenting with the use of scanners and digital images of tree rings with the stated purpose of developing an inexpensive alternative methodology to the standard density methodology based on X-rays. They had discovered that the “blue” wavelengths reflected by the scanned images of wood are the form of reflectivity data that varies most closely with summer temperature data (June-July-August) and maximum density data (hence, they called this new parameter “blue reflectance”). They reasoned that reflectivity and density data had similar climatological value, because both parameters result from the amount of lignin in latewood, which is known to be dependent on climate. As the title of one of their published articles indicates,[178] the Swansea dendroclimatologists suggested that blue reflectance could be used as a “surrogate” for latewood density measurements from high-altitude pine trees.

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[177] Mary Gagen et al., "Stable Isotopes in Dendroclimatology: Moving Beyond ‘Potential’." in Malcolm K. Hughes, Thomas W. Swetnam and Henry F. Diaz (eds.), *Dendroclimatology: Progress and Prospects* (Springer, 2010), p. 166.

[178] Danny McCarroll et al., "Blue Reflectance Provides a Surrogate for Latewood Density of High-Latitude Pine Tree Rings.", *Arctic, Antarctic, and Alpine Research,* 2002, pp. 450-453.

Since 2008, Rob has been involved in the development of a cheaper methodology for generating Blue Intensity data. Rob tells me that when he first discovered Blue Intensity "I saw immediately its potential". However, Rob was discouraged by the high cost of the measuring package (called "WinDendro") used by the Swansea researchers.[179] Rob's more affordable methodology involves the use of CooRecorder instead of WinDendro. Rob only pays approximately £45 for CooRecorder and CDendro (the software program used by Miloš for cross-dating tree-ring measurements and developed by Lars), in comparison to the £20,000 cost of WinDendro.

The use of CooRecorder for generating Blue Intensity data more cheaply is the result of different negotiations between Rob, Miloš and Lars. In 2008, Rob discovered the existence of CooRecorder via an email that Lars had sent to the members of the dendrochronology international mailing list. Lars initially designed CooRecorder for the purpose of measuring ring-width from digitised images, but Rob asked him if he could add a function to measure wood reflectance. Rob explains that within days of his request, Lars had sent him the first beta version of CooRecorder with an added Blue Intensity channel. Rob and Miloš are very pleased with their collaboration with Lars. Rob and Miloš not only value the cheapness of the software, but also the "direct" access they have to Lars. Miloš praises Lars for resolving his queries within hours, and incorporating his suggestions into the package. "I am sure that these changes would be more difficult to make if I would be dealing with a bigger corporation like Regents [WinDendro's creator]", Miloš explains.

The epistemological conundrum that Rob and Miloš face at this stage of the development of dendroclimatological knowledge is securing trust from colleagues in a new method of producing dendroclimatological data and generating estimates of climate. By building agreement among fellow dendroclimatologists that Blue Intensity is an effective and reliable tree-ring parameter for deriving proxy estimates of climate, Rob and Miloš also seek to convince colleagues and others that their own Blue Intensity data from Scotland provide valuable insight into past climates. Rob and Miloš negotiate the credibility of Blue Intensity through four social activities: experiments, one workshop, conferences and journal articles.

[179] Rachel Campbell, et al., "Blue Intensity in Pinus Sylvestris Tree Rings: a Manual for a New Paleoclimate Proxy.", *Tree-Ring Research*, 2011, Vol. 67.(2), pp. 127-134.

4. 2 the Development of Blue Intensity

4. 2. 1 Experiments

In one of the conversations taking place before the evening meal during the fieldwork of August 2012, Rob and Björn agree to conduct an inter-laboratory experiment with a set of samples from the Scottish Highlands. Rob and Björn want to know whether maximum density or Blue Intensity correlates more strongly with temperature data, and therefore, to know which parameter is more climatologically valuable in Scotland. Rob and Björn delegate the execution of this experiment to junior members of their respective laboratories. Miloš becomes responsible for generating Blue Intensity measurements with CooRecorder and coordinating the experiment with Emily, Björn's Masters' student in Stockholm who produces the maximum density data.

A year later, during the fieldwork expedition of August 2013, Emily and Miloš generate the relevant samples for the experiment. They decide to generate 20 pairs of replicated cores (40 samples in total) from two living tree sites in the East of Scotland, called "Ballochbuie" and "Ryvoan". In the field, Miloš and Emily extract two cores from the same tree ("A" and "B" cores respectively), each core being of a different thickness. Miloš uses the "standard" corer to extract 20 pieces of wood of 5mm diameter to form the first subset of "A cores". Emily uses a thicker corer, extracting 10mm cores to form a second subset of 20 "B cores".

Emily and Miloš generate samples of different thickness in accordance with the different methodologies for producing maximum density and Blue Intensity data. To produce Blue Intensity data, Miloš submerges the whole core directly into a bottle of acetone to remove the resins. Meanwhile, to produce density data, Emily needs a thicker core so that she can cut it into thin laths. She removes the resins with ethanol using conventional laboratory equipment called "soxhlet apparatus". As part of the experiment, Miloš wants to test whether the resulting Blue Intensity data is influenced by the choice of chemical treatment used to remove the resins from the wood. Thus, Miloš agrees with Emily that she will send offcuts from her subset of ethanol treated cores by post to St Andrews and Miloš will scan and measure the offcuts with CooRecorder. Miloš' plan is to compare the Blue Intensity datasets he has generated from the "Stockholm samples/offcuts" and the "St Andrews samples/offcuts" treated with acetone and ethanol respectively.

The experiment that Miloš coordinates with Emily involves the comparison of parameters (Blue Intensity and maximum density) and chemical treatments (ethanol and acetone) between the St Andrews and Stockholm samples and offcuts. At times, Miloš reports feeling somewhat frustrated by the difficulties of conducting an experiment with a reasonable order. "It is just being difficult to get everything working and organised". One situation that creates considerable confusion and disorder occurs when Miloš finds out via different email exchanges with Emily and Björn that there has been a slight issue with the labelling of the samples and off-cuts. This "mislabelling" has meant that, for a few days, Miloš has wrongly assumed that he was working with identical samples. Miloš wishes that some of the "misunderstandings" that are occurring throughout the experiment had been planned from the outset. At the same time, Miloš thinks that "these confusions are part of what it means to conduct a real experiment, in the sense that we don't really know what to expect from the start and the experiment keeps changing as new ideas are being tested". With Emily committed to parallel research projects in progress it takes seven months for Miloš to receive all the offcuts from Stockholm.

During the time Miloš is waiting to receive the off-cuts from Emily, he conducts a series of tests with the St Andrews samples "to know the limitations of the new Blue Intensity methodology that we are developing". Essentially, Miloš tests the use of the scanner and CooRecorder, which are very novel technologies used for generating tree-ring data. Miloš paints the scanner cover black to ascertain the effect of light contamination. He also changes the position of the cores when he scans them to see if this makes any difference to the results. Miloš also tries different time lengths for submersion of cores into acetone, and measures the physical shrinkage of the wood before and after they are treated chemically. Miloš also tests the different settings of CooRecorder. He does not scrutinise all the aspects of the methodology like the use of calibration cards because he assumes that other experts have already done those tests for him according to "international standards", and he feels that he does not have the sufficient expertise and time to conduct these tests by himself.

At the same time that Miloš is conducting tests with the Blue Intensity methodology, Rob carries out another parallel experiment with the Engelmann spruce samples from British Columbia that he used during his Master's. Rob generated ring-width data and density data from these set of samples and concluded that, for that geographical region, maximum density correlated more strongly against temperature data than ring-width data. This time, Rob wants to compare the old maximum density and ring-width chronologies against a new set of Blue Intensity chronologies generated from the same British Columbia samples.

Essentially, Rob wants to perform an exercise of "replication" and ascertain how well Blue Intensity reproduces the same correlations of density and ring-width data against temperature data that he obtained in his master's dissertation. Rob's experiment is possible because one of his undergraduate students travels to Western Canada in December 2013 and brings back with her the samples that Rob used in his doctorate. Rob also relies on another undergraduate student who produces the Blue Intensity measurements as part of his dissertation project. Once the Blue Intensity data are ready, Rob makes a series of conventional adjustments to the ring-width, maximum density and Blue Intensity chronologies to remove the effect of ageing on tree growth (Chapter 5) and compares each chronology against temperature records from British Columbia to ascertain the climatological value of each parameter.

4.2.2 Workshop

In April 2013, Rob organises a one-day workshop in St Andrews, initially aimed at discussing the preliminary results of the experiments coordinated by Miloš. Rob invites Björn and pays for his travel using the Scottish Pine Project budget. Rob also invites Emily who has generated the maximum density and the Blue Intensity datasets respectively. Perhaps because Rob does not offer to pay for her travel, Emily does not attend the workshop.

As Rob later decides that he would also like to discuss the results of his experiment with the reappraised British Columbia samples, he eventually makes the workshop much more open. He invites two European researchers whom he knows are working on Blue Intensity. Jesper is a Swedish PhD student from Gothenburg who is supervised by Hans and Björn (the Swedish participants in Chapter 2). A Polish researcher and lecturer at the University of Silesia whom Rob has met on a European conference in 2009, and whom I identify as “Andrzej”, also attends the workshop. Rob also invites a professor in wood anatomy at the University of Glasgow and renowned specialist in density and wood properties that I refer as “Barry”. Rob’s undergraduate students who have transported the samples from Canada and generated the Blue Intensity data for Rob’s experiment also attend the seminar. Anne, who is now working as a tree-ring technician for the Scottish Pine Project, also participates. In total, there are 11 attendants including me.

What Rob later refers to as “the Blue Intensity workshop” is structured into five presentations, starting at 9.30 am and finishing around 4pm with a lunch break. The workshop starts with Rob asking us to introduce ourselves, as he is the only one that knows everybody. Rob introduces himself as a “newbie in Blue Intensity”. Björn follows him and jokes about being “the black sheep” of the seminar as “I will be the defender of density today”. The other participants follow standard presentations by giving their names, surnames and professional affiliations. When it is my turn, I describe myself as a "sociologist who studies how Rob and Miloš do their work". By this time, all workshop attendees except Jesper, Barry and Andrzej know me very well because we have worked together on a fieldwork expedition. To comfort the others, Rob says, "Meri knows the science; she has been doing lab work with us and she was in fieldwork with us too". I then ask attendees' permission to record the meeting.

Miloš is the first one to present, and he starts by reporting the results of his experiments with the blue methodology or what he refers to as the "biases of Blue Intensity". While Miloš talks, participants constantly interrupt him asking for clarifications: "What does the number 40 mean in this setting in CooRecorder?"; "What kind of calibration card did you use?", "How did you make sure that the measuring points in CooRecorder do not overlap with each other?"; "Were all the cores scanned on the same day?"; "Where did you get the acetone from?"; "How warm was the room where you air dried the cores?". Miloš looks particularly satisfied when he has anticipated some of his colleagues' questions and he is able to address them in situ: "Yes, I knew you would ask for this issue, let me show you this slide to answer your question". At other times, workshop participants ask questions that Miloš has not contemplated and he cannot answer: "Yes, this is a good point that adds uncertainty to the method and should be explored further". Overall, I count more than 20 exchanges of questions and answers between Miloš and participants during the half hour for which the first part of Miloš' presentation lasts.

Miloš seems to enjoy being interrogated by his peers as he has the opportunity to show the intimate knowledge that he has developed of the laboratory work involved in producing Blue Intensity data. The result of these interactions is to decide collectively the importance of each of the "biases" of the Blue Intensity methodology. On the basis of the results that Miloš presents, participants conclude that the wood shrinkage and the orientation of the scanned core are irrelevant biases. Instead, they acknowledge the effect that the time of submersion of cores into acetone, the colour of the box and the settings in CooRecorder have on the final Blue Intensity data. Furthermore, they agree on a series of "optimal" solutions to the biases they recognise (48 hours for submersion of cores, black box and "160–5–50–15" as CooRecorder parameters).

The second part of Miloš' presentation consists of the results of the multiple comparisons between parameters and chemical treatments. Miloš presents the results of the comparison between Blue Intensity and maximum density produced from the St Andrews and Stockholm samples. Miloš reports that Blue Intensity "performs" better than maximum density, in the sense that it correlates more strongly to temperature data. This result triggers Rob to exclaim "Density sucks!" a comment at which everybody laughs, including Björn who is supposed to be the "defender" of density. Miloš then shows a table with some numbers and Rob says, now with a serious tone, that "it seems that density is more stable than blue". In the concluding slide of his presentation, Miloš includes a carefully worded sentence that reflects the general moderate optimism of participants with regards to the results he has presented: "BI [Blue Intensity] data quality is comparable to MXD [maximum density] (and may possibly even be better in some ways under certain conditions), although some issues still remain to be resolved".

Jesper follows with a presentation about what he calls the "heartwood and sapwood problem". Jesper explains that after treating his Swedish samples with ethanol, he can still identify a clear colour boundary between the heartwood or darker rings of the sample and the sapwood. The "problem", as formulated by Jesper, is that the remnant resins in the darker heartwood could potentially bias the measurements of reflectance and the resulting Blue Intensity data. Jesper presents his solution to the colour-resin bias which he later names as "Delta Blue".[180] Delta Blue involves correcting the Blue Intensity measurements of the heartwood in relation to the sapwood area that is considered non-resin affected. Jesper evaluates the adequacy of these corrections by comparing the similarity between the corrected Blue Intensity data and the maximum density generated from the same sample, which he takes as the standard for correction.

Jesper's Delta Blue method opens up a debate among what participants call the "biases" of density. The origins of this debate is Jesper's observation that the two available densitometer machines he has used to produce density data generated different density measurements from the same image analysis, which made it difficult for him to choose which maximum density data to use as a standard to evaluate the corrected Blue Intensity data. Barry - the tree physiologist and expert on wood density - confirms Jesper's observation and he gives a long exposition about the differences between densitometry techniques.

The third presenter is Andrzej, who in clear reference to Jesper's presentation, starts by saying that "My work shows that you can still get interesting results with Blue Intensity without having to correct the data". Andrzej shows how he has managed to date an archaeological building in Poland using Blue Intensity data alone. Rob claims to be "hopeful" of seeing Andrzej's successful application of Blue Intensity methodology to dendroarcheology. At this time, Rob is struggling to establish the dating of many floating ring-width chronologies from Scotland. Rob is

[180] Jesper Björklund, et al. "Is Blue Intensity Ready to Replace Maximum Latewood Density as a Strong Temperature Proxy? A Tree-Ring Case Study on Scots pine from Northern Sweden.", *Climate of the Past Discussions* 9, (2013), pp. 5227-5261.

considering generating Blue Intensity measurements from subfossil wood as a means to produce an independent verification of ring-width cross-dating. As such, Rob's plan is to accept the cross-dating between floating chronologies only if the dates from both ring-width and Blue Intensity measurements agree.

Rob gives his talk before lunch. He starts by presenting his work as an "example of what Blue Intensity can do if the wood has no discolouration problems" as the Engelmann spruce samples that Rob used in his master's dissertation work do not have any heartwood/sapwood colour distinction. Most of Rob's presentation is focused on what he calls the "unknowns" of the comparison between the three parameters against temperature data. While Rob reports that all parameters produce similar results, he emphasises a figure that he interprets as evidence that Blue Intensity data has less variance than density. Rob says that "Whether this is a good or a bad thing, I really don't know". Rob also points to a number that, he claims, shows that Blue Intensity data offers less low-frequency information or long-term climatic trends than maximum density, which is seen as a potential limitation of Blue Intensity. Pressured by the catering team, who are leaving trays of food onto the tables of the workshop room, Rob jumps onto the last slide of his presentation where there is written a sentence in red that says "Overall, very encouraging results for BI [Blue Intensity] for this species".

Over lunch, workshop participants continue very animated discussions about the results they have just been shown. Some of these discussions continue after lunch, during Barry's presentation. The wood anatomist starts his talk by posing the question, "What wood molecules is BI [Blue Intensity] measuring?" to which he responds "lignin" and "resin", as he says that these are the only two wood components known to absorb visible light. Barry explains that this might be the reason why workshop attendees are finding such similar results between Blue Intensity and maximum density. Barry clarifies that "according to wood anatomy theory, density is also an expression of hemicellulose and cellulose".

Barry admits that his knowledge of Blue Intensity is non-existent as "I had never heard about the Blue Intensity parameter until today", and he reasons by analogy with density when he says "My gut feeling is that Blue Intensity might be also reflecting the presence of hemicellulose and cellulose and that you might be measuring very similar things after all". Workshop attendees and Rob in particular, are interested in Barry's advice on how to decolorate samples and remove resins, which, as Jesper earlier suggested, might bias the resulting Blue Intensity data. Rob uses a metaphor of a white sock that is stained and left in a puddle for a few days to illustrate his interest: "What we need is the equivalent of *Vanish* that allows us to get back to the white sock".

The Blue Intensity workshop finishes with Rob giving a "tour" to attendees around the shared laboratory space in the School of Geography at St Andrews University, and around his office room where he stores his samples. Rob shows a couple of green-looking samples that he says have been "crucial" for cross-dating a number of floating chronologies. Rob suggests finishing the day with a short stroll along the St Andrews West Sands beach, which is famous for the opening scenes of the film "Chariots of Fire", as Rob reminds us. During the walk, we all continue talking about work and participants get to know each other a bit more. Jesper and Andrzej are particularly interested to know more about me and my research. I also take the opportunity to ask Björn if he feels that the development of the Blue Intensity methodology could threaten the status of his lab, which specialises in producing density data. Björn responds "Developing a cheaper parameter is good news for all dendroclimatologists, including our lab". Laughing he adds "But in any case, I still think that density is better than blue".

4.2.3 Conferences

At the international dendrochronology conference in Melbourne in 2014, Rob presents his experiment results with Blue Intensity parameter to the wider community of dendroclimatologists. Although Miloš also attends the Melbourne conference, he does not give a presentation on the methodology of Blue Intensity (instead he gives a talk and presents a poster on other topics). Rob is not alone in presenting the merits and limitations of Blue Intensity. Rob's talk is part of what he calls the "blue session", although this session does not officially exist as such in the conference programme. Rob presents the results of his British Columbia experiment in the same session as the other two participants from the Blue Intensity workshop, Jesper and Andrzej, who also present similar results to their workshop presentations.

Rob is the first presenter of the "blue session" and acts as a "master of ceremonies", introducing the work of his two colleagues at the end of his talk. To represent their collective membership, Rob tells me that he has considered the possibility of agreeing with Jesper and Andrzej to wear a piece of blue attire and to use the colour blue in their presentation slides. All three researchers finish their talks with very enthusiastic appeals to the audience "to go blue". Rob, in particular, emphasises the cheapness of CooRecorder and encourages everybody to "go out there and to measure blue". When I listen to and look at them, I think that Rob and his colleagues look like salesmen trying to sell their best product. At the end of his talk, Rob complains to me that he has not received many questions from the audience. "Conferences are in fact really useless to get good feedback", Rob says regretfully. I tell Rob that I think a few of his colleagues are very interested in Blue Intensity, judging by the fact that the room became packed with people during the talks given by Rob, Jesper and Andrzej.

Being myself part of the audience, I am a witness to the interest that Blue Intensity triggers among certain researchers. Before the start of Rob's talk, I hear a senior dendrochronologist from Argentina whom I had met during the field week in Tasmania the week before the conference and I refer to as "Antonio", saying in Spanish to the members of his lab: "Well, at last, we will learn about a method that poor dendro labs like ours can afford!" Antonio's comment refers to a previous talk given by a researcher about a very expensive machine used to generate density measurements.

Rob is well aware of the fact that the affordability of Blue Intensity is a very attractive aspect for many dendroclimatologists working in small and precariously funded laboratories like Antonio's. Rob and Antonio have known each other since 1996 when they both met as postdoctoral students at the Lamont laboratory and in fact, Rob's first publication included Antonio as a co-author. Rob and Antonio agree to collaborate during the course of a one-day car trip along the Ocean Road near Melbourne, which I had organised with other three PhD students for after the conference. A few weeks later, Rob emails one Argentinian postdoctoral student working in Antonio's lab. The agreement is that Rob will teach the Argentinian researcher how to use CooRecorder and in exchange for this advice, Rob will receive Blue Intensity data from the Argentinian samples.

As Rob makes explicit in the title of one of his talks to members of his department at St Andrews, his ambition is "to paint the world in blue". Rob aims to expand geographically the number of Blue Intensity datasets and to conduct a collaborative experiment that would contribute to the development of Blue Intensity. By mid-2014, Rob has assembled a few Blue Intensity datasets from Scotland, British Columbia, Sweden, Poland, Tasmania, Argentina and South Yukon through different forms of collaboration. In the case of Tasmania, where Rob worked as a technician during his early career, Rob knows the researchers working there very well. After the conference in Melbourne, Rob stays in Tasmania for a month and is allowed to use local samples to generate himself Blue Intensity data. In the case of South Yukon, he receives the Blue Intensity data directly from his supervisor Brian Luckmann. The data from Sweden and Poland has been generated by Jesper and Andrzej respectively. Rob slightly regrets that he has been unable to control the development of these parallel experiments with Blue Intensity. "In my original research design, I wanted to measure Blue Intensity on as many trees species as possible from Australia, New Zealand and South America, and for each species, a minimum of ten trees. Unfortunately, that was not always the case as sometimes I was given less than ten trees".

Rob also appeals to members of more wealthy laboratories who, in theory, are not so intrinsically interested in the affordability of Blue Intensity. In August 2014, Rob gives a talk in the "WSL" laboratory in Switzerland that specialises, among other things, in the production of density data. Fritz Hans Schweingruber, the main creator of the density parameter, is based in WSL. In the past Rob has collaboratorated with the two heads of the laboratory in WSL, and one of them, David Frank is a good friend of Rob and attended Rob's wedding. The title of Rob's talk in WSL is "If I had a blank check, would I use MXD or BI?" Rob's response to this question in the final presentation slide is "I would go for MXD….but tests of BI must continue". This response expresses Rob's belief that, considering all the uncertainties of Blue Intensity, maximum density is still a "safer" parameter. As Rob explains to me in an email after his return from Switzerland, the seminar has been useful for identifying and discussing new uncertainties about Blue Intensity. The colleagues at WSL had come up with more uncertainties about blue intensity as a result of analogical reasoning of known problems with the production of density data.

4.2.4. Peer-Reviewed Journal Articles

One month after the Blue Intensity workshop in St Andrews, Miloš and Rob finish the first drafts of two different journal articles. Miloš is the first author of an article where he describes the tests he has done with the Blue Intensity methodology and the results of the comparisons between parameters and chemical treatments. Miloš insists that this paper is not a "manual" on how to use CooRecorder, but rather a paper about the "methodology" of Blue Intensity. Rob's plan is to publish the two papers jointly as Part I and Part II in a highly-ranked paleoclimatology journal. "The BC [British Columbia] paper", Rob says, "Is an empirical application of the low-cost methodology developed by Miloš, so they fit together very well". In his paper, Rob uses Miloš' article as a "shortcut". Instead of describing his methodology in detail, Rob writes that "We followed the procedures detailed in Rydval et al. (in preparation) to generate the BI data". In turn, Miloš refers to Rob's paper to support his findings ("Similar results were reported in Wilson et al (in review)") and to illustrate that the known uncertainties about Blue Intensity are being addressed by Rob ("The ability of BI to capture longer time-scale information need also be explored further (see Wilson et al., ., in review)").

Rob's original intention to publish the two papers together does not work out. Because of the detailed nature of Miloš' paper, reviewers suggest submitting it to a journal with more specialised audience. Miloš eventually publishes his paper in the main European journal of dendrochronology, *Dendrochronologia,*[181] whilst Rob publishes his article in *Holocene*[182], one of the most reputed paleoclimatology and geophysics journals (as measured by its Impact Factor of 3.794 and positioned 6th out of 46 in the Physical Geography ranking in 2014).

The drafting of Miloš' paper develops alongside the follow-up investigations that he conducts of the potential colour bias in the Blue Intensity dataset resulting from the presence of remnant resins in the heartwood as identified by Jesper in his Swedish samples. Miloš tells me that after hearing Jesper's presentation in the workshop in St Andrews, he suspects that this bias could also exist in his samples from Ballochbuie and Ryvoan. Miloš shows me a few samples that still have a visible colour transition after treating them with acetone. In the first article draft, Miloš writes that "a step trend in Blue Intensity is apparent around the HW-SW [heartwood-sapwood] transition, which becomes reduced but does not disappear following acetone treatment". Rob thinks that Miloš' comment is unjustified as he does not provide any evidence for the way the visually distinctive colour boundaries on the wood result into a "strep trend" in the Blue Intensity data. In one email, Rob writes that "Miloš is creating problems out of nothing".

Even though Miloš initially has no evidence of colour bias in his Blue Intensity dataset for Scotland, he is concerned about how colleagues and outsiders will evaluate his work in the light of Jesper's results.[183] In one meeting with Rob, Miloš says, "I don't want to get into a situation where Jesper, at about the same time as me, publishes a paper which addresses all the colour bias problems quite straight on, and then in my paper I don't make any mention of them as if I was trying to hide them...like in 'the hide the decline". Miloš' later comment ("the hide the decline") is

[181] Miloš Rydval et al., "Blue Intensity for Dendroclimatology: Should We Have the Blues?" Experiments from Scotland." *Dendrochronologia, 2014, Vol. 32, pp. 191–204.*

[182] Rob Wilson et al. "Blue Intensity for Dendroclimatology: The BC blues" A case Study from British Columbia, Canada." *The Holocene*, 2014.

[183] Björklund et al., "Is Blue Intensity Ready to Replace Maximum Latewood Density as a Strong Temperature Proxy?"

a reference to Climategate, when fellow paleoclimatologists were accused of hiding and manipulating data by a few bloggers and commentators (Chapter 1).[184]

In order to ascertain whether his doubts are unfounded, Miloš employs the Delta Blue method that Jasper has created to correct Blue Intensity data. Miloš reasons that if Jesper's method applies any correction to the Scottish dataset this would mean that there is a colour bias in the first place. The results that Miloš obtains with Jesper's method turn out to be inconclusive, mainly due to the fact, Miloš argues, that "the method is still at the early stages of development". However, in preparing the Blue Intensity dataset to apply Jesper's corrective method, Miloš observes a drastic step in the trend of Blue Intensity measurements that correspond to the colour transition of samples, which is later smoothed out by the methods of age correction he uses.

Miloš experiments with different methods to correct for age trends and discovers that the method he has been using in his experiments ("Hugershoff") eliminates the effect of the colour boundaries in Blue Intensity measurements. Miloš interprets this discovery as a victory: "I have been able to demonstrate that the colour bias in Blue Intensity data does exist, and it is masked by specific detrending choices [methods for correcting ageing]". In the final published version, Miloš creates a graph that he employs as evidence of bias existing in Blue Intensity data. In the text, Miloš refers to this graph ("figure 12") as being in agreement with Jesper's conclusions ("Björklund et al. 2014") and in opposition to two previous "misleading" graphs ("figure 7 and 8") that showed good agreement between Blue Intensity and density:

> Figs. 7 and 8 suggested that the Hugershoff detrended MXD and BI chronologies for both locations were very similar. However, Fig. 12c and d, which show both chronologies after using a more conservative linear regression function for detrending, clearly highlight a potential bias in the recent period of the BI [Blue Intensity] data where the recent warming signal, clearly picked up in the MXD data, is not captured in the BI chronologies. This issue was also highlighted in Björklund et al. (2014). [185]

[184] In the process of revising this final version of the draft, Miloš clarified that "this is a bit of an overstatement. More than anything I was curious to find out if there was something to this discolouration issue or not (just had a nagging feeling that it might be something I could miss) – even though I didn't really feel anyone would criticise my work for not addressing or rather discovering this issue (especially if I didn't know about it). I felt it was better to investigate it when I had the chance".

[185] Rydval et al., "Blue Intensity ", p.201.

Drawing inspiration from the work of other colleagues, Rob comes up with a solution that he calls "BI/RW [Blue Intensity and Ring-Width] band-pass approach" to the colour bias now identified by Miloš. This solution consists of creating a single chronology that merges the "best" information provided by both parameters. With regards to ring-width, Rob and Miloš know that its value lies in low-frequency or long-term centennial climate variability. With Blue Intensity, meanwhile, because the darker tree-rings are located in the sapwood or the oldest section of a sample, Miloš and Rob reason that the colour bias distorts the low-frequency information. Therefore, they conclude that Blue Intensity is good for providing high-frequency or yearly and decadal climate variability. The BI/RW band-pass approach mixes the low-frequency from ring-width data and the high-frequency from Blue Intensity to create, what Rob and Miloš call, a "pseudo-parameter".

Rob creates the "BI/RW band pass approach" as an alternative to Jesper's Delta Blue method. Rob disagrees with Jesper about using density as the standard against which to evaluate the corrections of Blue Intensity. "Jesper assumes that blue should be similar to density like most people in the early days of blue, the theory was that blue could be a proxy for density", Rob says. "Instead, what I want to argue is that both Blue Intensity and density are proxies of lignin".

Rob's attempt to reformulate the traditional definition of Blue Intensity from being a surrogate for density to one that sets the two parameters as complementary becomes clearer during the drafting of Miloš' paper. Rob asks Miloš to rephrase a sentence that originally stated "Blue Intensity is strongly correlated to density" to "both Blue Intensity and density are correlated to lignin content". In an email where I ask Rob to clarify the exact molecular difference between the two parameters, he responds that "Blue Intensity is theoretically related to lignin ONLY and wood also is made up of cellulose and hemicellulose which also influence wood density; hence why density and Blue Intensity are not quite the same".

Rob knows from Barry, the wood anatomist present at the Blue Intensity workshop in St Andrews, that the two parameters, in theory, reflect very distinct molecular components. Yet, Rob also knows from Barry that this distinction is not clear-cut. Rob admits, "Being a 'bear of diminished brain' I would bet that cellulose and hemi-cellulose may also reflect in the blue spectrum, so it is very possible that density and BI are in many ways measuring the same thing".

Despite doubts about the exact molecular differences between parameters, Rob is adamant that the different methodologies for producing Blue Intensity and density data make the two parameters different. Rob and Miloš argue further for the differences between parameters by appealing to their different degree of "efficiency". As part of the conventional procedure for testing tree-ring data, Miloš employs a statistic (called "Expressed Population Signal" or EPS) that measures how well chronologies represent the hypothetical overall population of trees. Miloš reports that Blue Intensity scores consistently lower than density. Rob interprets the results by saying that density is a much more "efficient" parameter because it provides better climatological information than Blue Intensity with fewer samples. "Let's not trash density", Rob advises, "because it has its virtues". In the published paper, Miloš reports the EPS results in relation to Rob's paper where similar EPS results are reported with the British Columbia data:

> These results demonstrate that BI data contain a weaker common signal than MXD when replication is equal for both. This observation was also highlighted by Wilson et al. (in review). While at the individual tree level the signal from BI can be improved by averaging 2 or 3 radii, MXD ultimately still has a stronger common signal. However, as the costs and effort associated with generating additional BI data are negligible compared to MXD, this does not present a problem as in most cases more data can easily be obtained by sampling more trees.[186]

In the excerpt above, Miloš tries to "compensate" for the limited efficiency of this new parameter by referring to the cheapness of producing Blue Intensity. In his paper, Rob's insistence on the cheapness of Blue Intensity is seen as improper by one of the reviewers of his papers who comments "I think you are overstating the cheapness of the method". As a result, Rob aims to reformulate the criteria of cost as salient for producing dendroclimatological knowledge. To illustrate his point, Rob refers to a recent controversy in dendroclimatology ("Mann et al". and "Anchukaitis et al" in the quotation below) in which Rob has been involved (Chapter 7). As a response to one of the reviewers who suggests excluding these two references, Rob claims that this controversy is relevant to understand the scientific importance of the affordability of Blue Intensity. In Rob's view, this controversy had demonstrated that the high costs of generating density data have led dendroclimatologists to use ring-width chronologies that are known to be imperfect for reconstructing annual changes in climate resulting, for instance, from volcanic eruptions. In the final published version, Rob adds a paragraph that expresses this same idea:

> This paper is not the place to re-iterate the arguments of Mann et al. (2012) and Anchukaitis et al. (2012). However, this contentious issue did highlight that for

[186] Rydval et al., "Blue Intensity ", p.198.

> robust attribution of climate forcing of the last 1000 years – especially with respect to the influence of volcanic events – more MXD chronologies need to be developed, especially prior to 1500. As MXD data appear problematic for most laboratories to generate, this paper emphasises the potential of a relatively new tree-ring parameter – Blue Intensity (BI) – which could be used as an alternative to MXD to overcome this issue. [187]

Rob is convinced that, even considering all the uncertainties and limitations of Blue Intensity, its affordability will make this parameter a "game-changer" in the field. The basis of Rob's optimism is that while Blue Intensity is admittedly more uncertain and less reliable than density data, the low-cost associated with the generation of Blue Intensity will make it possible for less well-resourced labs to experiment with this parameter and to generate much larger volumes of data, which, when analysed in bulk are seen as capable of yielding equally reliable climate estimates as to density. The positive contribution of Blue Intensity is acknowledged by one of the reviewers of Rob's paper, who writes "The work with its companion paper [Miloš' paper] will be very much sought after in the literature as researchers move forward on technologies and new analyses. Wilson et al. have done a great service to the community here." In the introduction to his paper, Rob states that the aim of his study is to test Blue Intensity "as an alternative, cheaper, proxy archive to MXD for reconstructing past summer temperatures", Rob concludes by refusing to make a superlative comparison between the two parameters:

> Taking into account, uncertainties related to different detrending methodologies and the shortness of the instrumental data, it is not possible from this study to quantify which parameter is best for reconstructing past summer temperatures in this region. Rather, our results indicate that MXD and BI, as they are both measures of lignin content in the latewood, can be used as proxies of past summer temperatures. At this time, we still recommend, if funds allow, that MXD is the parameter of choice as there are still many potential uncertainties with the use of BI data.[188]

Rob concludes his article with an appeal to fellow researchers to continue experimenting with Blue Intensity on different tree species as well as living and subfossil sample material. In February 2015, Rob tells me that Jesper is leading an experiment in collaboration with the density experts in

[187] Wilson et al, "Blue Intensity ", p. 2.
[188] Wilson et al., *Ibid*, 9.

WSL, which Rob thinks “will hopefully contribute to ascertain further the value of the Blue Intensity parameter and to finally address some well-known problems with density”. At the conference in Melbourne, Jesper had received an award for his Delta Blue method and he is being increasingly recognised as a Blue Intensity expert by the community. Jesper is also the main author of a paper where he introduces the method of “Delta Density” - inspired by the Delta Blue method – where he examines the possibility of density data being biased by remnant resins in the samples. In his interlaboratory project, Jesper will explore the density biases and many other issues related to the methodology of Blue Intensity. The description of the experiment that Jesper sends to participants, and which Rob forwards to me, includes the idea of generating Blue Intensity data from a common set of Scots pine samples from Finland. Rob and Ryzshard are participating in Jesper’s comparative project as experts in the low-cost methodology of Blue Intensity. In April 2015, I also learn that Andrzej will be in charge of training students who are participating in a dendrochronology summer course on Blue Intensity in Spain.

4.3 Discussion

Miloš and Rob seek to establish the credibility of Blue Intensity by offering a qualified sceptical assessment of its strengths and weaknesses in relation to existing tree-ring parameters - particularly density - through different examples of civil scepticism during experiments, a workshop, conferences and peer-review processes, which depend on different types of trust relations between Rob and members of the dendroclimatology community.

The experiments with Blue Intensity are a form of civil scepticism that is parasitic on the existence of a widely distributed web of collaborators, colleagues and students whom Rob entrusts to conduct scepticism competently. In the case of his reappraised experiment, Rob entrusts undergraduate students to bring the samples from Canada and to generate Blue Intensity data from them. Crucially, Rob entrusts Miloš with the task of experimenting with CooRecorder and coordinating the experiments and the associated management of trust relations with researchers in Stockholm. The collaboration that Miloš establishes with Emily derives from the primary set of trust relations that Rob first established with Björn.

The discussion of the results of the experiments in the one-day workshop in St Andrews is an example of a collective sceptical display. Through the conventional form of PowerPoint presentations and question and answer sessions, workshop participants seek to demonstrate to each other that they are competent sceptics, and in this way, reinforce trust in their sceptical abilities. The workshop attendees who participated in the fieldwork expeditions (Björn, Miloš and Anne) already constitute an intimate community of trust, whilst the other attendees (Jesper, Andrzej and Barry) have weaker bonds of trust with Rob and other attendees.

Miloš tries to build up and sustain the trust from workshop attendants by demonstrating the extent to which he has been able to anticipate the hypothetical sceptical questions from colleagues and to incorporate these concerns into his own sceptical examination of the Blue Intensity methodology. Likewise, the other workshop attendees engage in other conventional demonstrations of sceptical virtuosity, for instance, when Jesper shows the way he has addressed his concerns about the existence of colour bias in Blue Intensity with the development of the “Delta Blue” method. Other forms of sceptical display are articulated as “scepticism-as-an-account”, such as when Rob focuses on the “unknowns” of his results and when Barry -the wood anatomist- admits the uncertainties about the exact molecular differences between tree-ring parameters.

Through the sharing of mutual and courteous sceptical evaluations of each other’s work, workshop attendees establish an emergent community of collaborators around Blue Intensity. The clearest evidence of the existence of this community is the tripartite presentation from Rob, Jesper and Andrzej in the “blue session” at the conference in Melbourne, and the fact that Jesper and Andrzej entrust Rob with the responsibility of being the “master of ceremonies” for this session. For his conference presentation, Rob mobilises the network of trusted colleagues that have participated in the workshop in St Andrews to address the challenge of exposing his incipient results to the potentially uncivil scepticism of less trusted dendrochronology colleagues. In fact, Rob’s disappointment that he did not receive any questions after his conference talk could be interpreted as an indication that Rob does not believe that conference attendees have practised the same degree of organised scepticism as he expects from trusted colleagues.

In conference talks and posters, Rob invites his colleagues to examine Blue Intensity sceptically by appealing to the relative cheapness of Blue Intensity. Rob is particularly successful at enrolling the Argentinian researcher Antonio, with whom Rob starts collaboration. This strategy also includes density experts in the Swiss laboratory, who, during Rob’s presentation come up with new uncertainties regarding Blue Intensity in analogy with the known problems with density. Rob also hopes that, as a result of the development of Blue Intensity, density experts might start re-examining previously suspended scepticism about the density parameter through Jesper’s interlaboratory experiment and his “Delta Density” method.

Rob's comments about the cost of Blue Intensity are seen by one reviewer as "overstating the cheapness of the method", and being improper behaviour that potentially jeopardises the trust in which Rob is held by colleagues. In his article, Rob aims to convert his emphasis on the cheapness of Blue Intensity from a reason to be sceptical about him and his new parameter to a reason for trusting him. Rob does so by defending the scientific relevance of the affordability of Blue Intensity in relation to a recent controversy.

Rob's ultimate aim to "paint the world blue" and expand the number of Blue Intensity datasets is the result of a progressive establishment of networks of trust. [189] Rob's assemblage of Blue Intensity datasets depends on an existing network of trust with people with whom he worked as a graduate student in Tasmania and Canada, as well as new colleagues like Jesper and Antonio whom Rob has come to trust through the mediation of other trusted people (Jesper is Hans and Björn's student and Rob's collaboration with Antonio resulted from my intermediation as organiser of the Ocean Road trip). Like in a network, the relations of trust that Rob establishes bilaterally with others become the basis upon which new trust relations can emerge without Rob's direct intervention and control, as Rob slightly regrets the fact that his original research design did not evolve as he planned it. As an example of the multiplicity of "nodes of trust", Jesper is trusted by his community as a competent promoter of Blue Intensity when he is awarded a conference prize. Also, the expansion of Blue Intensity in the form of new experiments and training courses is carried out by Jesper and Andrzej respectively; and Rob only participates as a "collaborator".

Miloš and Rob seek to strengthen the trustworthiness of their low-cost Blue Intensity methodology by connecting their respective "methodological" and "empirical" papers, and in this way, consolidating their relationships of trust. In the follow-up investigations that inform the drafting of Miloš' paper, he negotiates how to convey the scepticism he has learnt from Jesper regarding the potential colour bias in the Blue Intensity dataset. Miloš is adamant that he does *not* want to suspend his scepticism, against the advice of his supervisor who thinks that Miloš' display of scepticism is unfounded and "creating problems out of nowhere". Miloš insists in conducting self-critical experiments as a means to pre-empt potential uncivil sceptics who previously accused climate scientists during Climategate of "hiding the data". Miloš conceives the discovery of the

[189] For a similar argument as it applies to metrology read Graeme Gooday, *The Morals of Measurement: Accuracy, Irony, and Trust in late Victorian Electrical Practice* (Cambridge: Cambridge University Press, 2004), p. 16. Gooday argues that the extension or adoption of internationally accepted units of measurement does not depend on "centres of power" (as Bruno Latour suggests) that unilaterally impose a vision. It is rather the result of interconnected relations and "networks of trust".

colour bias in the Scottish dataset as a positive outcome because it allows him to substantiate his sceptical display to others.

In the drafting of the peer-reviewed articles, Rob and Miloš are careful not to delegitimise the other tree-ring parameters. Instead, Rob and Miloš employ the new Blue Intensity parameter to reinforce dendroclimatologists' overall ability to generate reliable climate estimates from trees. Therefore, the credibility between tree-ring parameters is not one of a zero-sum relationship, but of a win-win situation.[190] Three examples occurred during the drafting of the papers illustrate the way that Rob and Miloš reassess and carefully adjust the credibility of density relative to (and in a way that reinforces) Blue Intensity. First, Rob develops the method of the "BI/RW band pass approach" as a means to combine the "best" aspects of ring-width and Blue Intensity in terms of providing low and high frequency information respectively. The second example is Rob's increasing attempt to redefine Blue Intensity *not* as a surrogate for density, but as a distinct and independent means of assessing lignin content. The third example is Rob and Miloš' interpretation of the EPS results, which could have potentially damaged the credibility of Blue Intensity as an inefficient parameter. Yet, by introducing the criterion of "cost", Rob and Miloš readjust the balance of credibility between parameters as Blue Intensity allows cheaper production of samples. Rob and Miloš' practice of complementing data from different tree-ring parameters is part of the tacit dimension of dendroclimatologists' fiduciary framework, as shown by the fact that most tree-ring based reconstructions, including the Scottish one, are created from data from different tree-ring parameters.

When dendroclimatologists like Rob and Miloš employ datasets from different researchers (such as Hughes and Björn's density datasets) they draw upon and reinforce the trust relations that sustain the fiduciary framework.

Rob and Miloš' strategy of sceptically assessing the relative merits of tree-ring parameters is *partly* determined by the features of the natural world. Tree-ring patterns are different across trees growing in warm and semi-arid contexts like the Southern US, in cold environments like the Alps and Scotland, and in tropical areas. If dendroclimatologists employed just one single tree-ring parameter they would restrict themselves to producing knowledge from just one geographical area. Therefore, the exercises of civil scepticism, as exemplified here, are not solely a matter of drawing

[190] The sociologist Michael Lynch describes the phenomenon of the "inversion of credibility" of the forensic methods of fingerprinting and DNA profiling in Truth Machine. In this chapter, I would argue that the relationship between methods for generating climate data from trees leans towards an "equilibrium of credibility" rather than inversion of credibility. *Truth Machine: the Contentious History of DNA Fingerprinting* (Chicago: University of Chicago Press, 2010).

on and building interpersonal trust relations; nor do they merely depend on sharing a common sceptical attitude. Civil scepticism also has an important empirical dimension, in that it involves a set of reference points in the material world, with which the data produced by Blue Intensity and density need to be aligned. Therefore, being seen to exercise competent empirical judgement with regards to the empirical qualities of tree-ring parameters is an important part of the way dendroclimatologists, including Rob and Miloš, secure trust.

5 Standardisation

5.1. The Extraction of the Climate Signal from Noisy Data

Miloš and Rob believe that deriving climatic information from trees growing in Scotland is very difficult. They express their concerns both privately and publicly in conversations and conferences. "If we manage to do dendroclimatology in Scotland", Rob states in front of an audience of 100 people at the conference in Melbourne, "We will make a case for our field". Rob considers the Scottish Pine Project the most challenging dendroclimatological project of his career. He explains that in British Columbia, where he did his PhD, "the behaviour of trees was pretty crystal clear". Miloš sees the experience he will gain working in Scotland as an advantage. "Everything else I find in the future can only be easier!" he jokes.

Rob and Miloš complain that Scotland is "complicated" because it is hard for them to interpret the patterns of tree-ring chronologies. They have put considerable effort and thought into producing carefully dated chronologies of two aspects of tree-ring growth (ring width and blue reflectance). Rob and Miloš seek to merge the ring width and blue intensity data into a single "pseudo-parameter" or RW/BI band-pass chronology (Chapter 4). However, this combined chronology, as it stands, does not yet serve their interests as dendroclimatologists. Rob and Miloš are well aware that other factors besides climate can affect tree-ring growth, and that these confounding factors are reflected in the ring width and blue intensity datasets.

More generally, dendroclimatologists' awareness of the existence of confounding noisy factors within the tree-ring series has grown over time. After repeated observations, pioneers in dendroclimatology working in the Laboratory of Tree-Ring Research in Tucson in the early 20th century concluded that the ring-width data from coniferous species growing in the semi-arid areas of Northern Arizona had a consistent downward trend because tree-rings became increasingly narrower. Andrew Douglass, Waldo Glock and Edmund Schulman reasoned that this declining growth curve was related to the increasing age of trees, as older trees do not have the "energy" to produce rings as wide as when they were young. These dendroclimatologists concluded that by removing the declining ageing effect from the ring width series, they could use the remaining chronologies as an estimation of annual fluctuations of rainfall.[191]

[191] Douglass, "A Method of Estimating Rainfall"; Schulman, "Tree-Ring Indices".

The cleaning of undesirable non-climatic growth trends from tree-ring series, often associated with the ageing of trees, is a process that dendroclimatologists refer to as "standardisation" or "detrending". Standardisation is achieved by dividing each tree-ring measurement by its "expected" value in a mathematical or "standardisation curve". Early dendroclimatologists in Arizona employed a "negative exponential function" to model and to remove the declining tree growth curve they had observed in their ring-width chronologies. The purpose of standardisation, as originally defined by Edmund Schulman, is "to obtain a mean growth curve representing trees of various ages".[192] This "mean chronology" is an average of all the standardised tree-ring series or chronologies from a site. This averaging procedure is used by dendroclimatologists as a strategy to maximise the climate signal that they are certain exists in trees. Averaging strengthens the common (climatic) patterns of tree-ring variability and cancels out the variable effects of ageing that vary from tree to tree.

As dendroclimatologists started doing research in other locations, they realised that ring-width chronologies do not always display the same declining trend as the Arizonan chronologies. Edward Cook addressed this issue explicitly when he was a doctoral student in the Laboratory of Tree-Ring Research in Tucson under the supervision of Harold Fritts. In his thesis, Cook worked with conifers from the North-Eastern US, where trees grow in more humid and dense forests than those in Arizona. Cook found out that the chronologies from these trees did not show negative growth trends. He reasoned that since all trees are similarly affected by ageing, it was noisy factors other than age that affected the growth of trees in the Eastern US. Cook hypothesised that trees in dense forests compete for sunlight and nutrients with neighbouring trees and so their normal growth is more likely to be affected by the clearances that result from logging or the blowdown of trees.

Cook named the non-climatic events that caused the deviations of growth trends from the "classic" declining pattern in the Arizonian chronologies "disturbance".[193] He distinguished between two types of disturbance: one originating from "natural" causes occurring within the forest like a blowdown of trees, and another originating outside the forest often related to "human" causes like fires, pollution or logging. Cook summarised the effect of age (A), climate (C); natural and

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[192] Schulman, "Tree-Rings and Runoff in the South Platte River Basin.", *Tree-Ring Bulletin*, 1945, Vol. 11, (3).

[193] Edward Cook, "A Time Series Analysis Approach to Tree-Ring Standardisation", PhD dissertation, The University of Arizona, 1985, p.4.

human disturbances respectively (D1 and D2), and random “error” variations unique to each tree (E) on tree growth into a conceptual model that he called “the linear aggregate tree growth model”:

$$R_t = A_t + C_t + \delta D1_t + \delta D2_t + E_t$$

Cook’s linear aggregate tree growth model is based on the assumption that the effect of each of the growth factors is distinguishable from each other and that altogether these factors have a cumulative effect on tree growth (hence its name, “linear” and “aggregate”). In his thesis and subsequent papers, Cook explicitly states that the assumptions of linearity and independence in the model are “a necessary oversimplification for the moment”.[194] Yet Cook defends the value of the linear aggregate tree growth model as a conceptual tool for thinking about the distinct signals in tree-ring data, as “the purpose of the linear aggregate tree growth model is not to describe exact relationships between the subseries, but rather it allows for a discussion of certain properties of each component separately from the others as a necessary step in developing a standardisation method that models the nature of the tree-ring series more adequately”.[195]

Cook’s conceptual model expresses, if it does not constitute[196], the contemporary division of labour between dendroclimatologists and dendroecologists. From the perspective of a dendroclimatologist like Cook, the concept of noise is everything that is not relevant to reconstructing climate (hence, age and disturbance). However, for a dendroecologist who is interested in the study of forest dynamics, disturbance is precisely the signal of interest and climate is noise. As Cook explains, what counts as “signal” and “noise” in his model is a matter of perspective, as “one researcher’s signal would frequently be another researcher’s ‘noise’”.[197]

In addition to his theoretical model, Cook also developed a statistical package to perform standardisation calculations as part of his PhD. The “AutoRegressive Standardisation” (ARSTAN) employs a statistical technique called the “AutoRegressive Moving Average model”,

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[194] Cook, *Idem*, p. 24.
[195] Cook, *Idem*, p. 24.
[196] This hypothesis would require a historical study of dendroecology as such, but I think it is important to note that Edward Cook’s thesis was submitted in 1985 and that four years later a foundational paper that spelt out the objectives of dendroecology was published (Fritts and Swetnam, “Dendroecology: A Tool for Evaluating Variations in Past and Present Forest Environments”, *Advances in Ecological Research*, 1989, pp. 19111-19188).
[197] Edward Cook, "The Decomposition of Tree-Ring Series for Environmental Studies." *Tree-Ring Bulletin*, 1987, p.38.

or ARMA, to generate standardised chronologies that exclude the presence of autocorrelated or non-random patterns in tree-ring series that are due to disturbance events, climate or ageing. Cook's linear aggregate tree growth model and the ARSTAN program are the most widely used tools of standardisation in the discipline, to the extent that the model is defined as a "principle" in the most recent dendrochronology textbook.[198] In a conversation with a dendrochronologist in the Tasmanian fieldwork about Cook's role in the field, she claims "I don't know what we would do without Ed!" A few days later, in the conference in Melbourne, Edward Cooks receives a public acknowledgment from his colleagues in the form of the "The Harold C. Fritts Award for Lifetime Achievement in Dendrochronology" for "Cook's significant influence on dendrochronology, emphasizing innovative research that has advanced the field, distinguishing it among our peer sciences."

Crucial to the expansion of the community's use of ARSTAN is the fact that Cook made this software freely available from the beginning and collaborated with many other colleagues to refine it. In 1983, Cook provided the source code to the Laboratory of Tree-Ring Research at the University of Arizona, where Richard Holmes - the creator of COFECHA, the main statistical package for cross-dating (Chapter 3) - updated it to the main programming language of the time (FORTRAN). In 2003, Cook co-developed with Paul Krusič the code for ARSTAN to run on Windows computers, as Cook originally developed it to run on Macintosh. Edward Cook and the co-developers of ARSTAN have never created a user's manual; as Cook tells me, "I have other more important things to do". Instead, Cook has disseminated ARSTAN through personal and face-to-face instructions.

Training courses or "field weeks" like the one I attend in Tasmania are where Cook teaches colleagues and neophytes how to use ARSTAN. Indeed, the main reason that I participate in the field week in Tasmania is because I want to observe how Cook disseminates his methods and how others learn about them. This field week is organised into different subgroups and I sign up for the "statistical dendrochronology" group that is co-led by Edward Cook and a more junior dendroclimatologist whom I will refer as Emma. My group includes four PhD students from Australia, Bolivia, New Zealand and Kathmandu, a postdoctoral researcher and a couple of retired Swedish scientists (the husband is a statistician and the wife is a dendroarcheologist). My colleagues are visibly excited to be taught by Cook personally, and Emma says she feels "honoured" to co-lead this group with Cook.

[198] Speer, *Fundamentals,* p. 17.

During a period of six days, students do all sorts of activities together and get to know Cook quite well; he gets to know us quite well too. Cook comes across as a very generous, patient, good-humoured and knowledgeable teacher and person. As part of the statistics group, Emma and Cook give students the ring width and density datasets that we will use to learn and practice techniques of standardisation and reconstruction. However, on the first day we go together to the field site to see where "our data comes from". Every morning, Cook and Emma teach us one aspect of dendrochronology and the use of statistical methods. Before lunch, we are asked to do an exercise that relates to the daily topic. For instance, on our first day we learn about COFECHA (the main software used for cross-dating; see Chapter 3) and we are asked to resolve whether the ring width and density chronologies we create are properly crossdated. In his lecture, Cook reviews the different settings in COFECHA and shows us "hidden functions"; only "a few people know about them", Cook says. Emma confirms that she did not know about one of the hidden functions that Cook describes. Cook tells us a bit of history of how each of the settings in COFECHA came to be and explains that he disagreed with Richard Holmes over one specific command.

The students in my group feel privileged to hear Cook's insider knowledge about the intricacies of the software programs that he himself helped develop. One colleague tells me, "[W]e would have never known about all these hidden functions because there's nothing published about them". After Cook's instructions, we all feel that we have acquired a very exclusive knowledge that makes us more knowledgeable in many of the programs and concepts used in statistical dendroclimatology. Another colleague in the team, who is a lecturer at his home university in Kathmandu, tells me that he is going to teach these "hidden functions" to his students and in this way disseminate Cook's techniques.

In the field week class about ARSTAN, Cook insists that the most important decision in standardising tree-ring data is to choose a standardisation curve. Dendroclimatologists can choose from a range of standardisation curves, all intended to eliminate the effects of ageing alone. Emma explains that the choice is between "deterministic" or "conservative" curves and "non-deterministic" curves. The first group consists of (negative) linear regression curves like the "classic" Arizonian negative exponential function that entails the assumption that ring-width normally decrease over the life of a tree. The second group consists of data-adaptive techniques such as "smoothing splines" that do not entail any a priori belief about the ageing trend. Cook shows us that if we press "option number 4" in the main Program Menu in ARSTAN we can see the list of all the standardisation curves. Cook emphasises "ARSTAN will not make the choice of deciding what standardisation to use for you".

> The choice of standardisation curve essentially involves a judgement about the proportion of the trend in the dataset that reflects the effect of climate and the proportion that is due to ageing or disturbance. It is often the case that the climatic and ageing trends overlap in time: climate might change slowly over decades and centuries throughout a tree's lifespan. Thus, the main challenge that dendroclimatologists face is that depending on the standardisation curve they choose to remove the effects of ageing, they might inadvertently also remove the long-term growth changes related to climate ("low-frequency").

All dendroclimatologists seem to experience standardisation as the most challenging step in the production of dendroclimatological knowledge. At the international conference in Melbourne, dendroclimatologists consistently end their talks with references to the uncertainties of choosing an appropriate standardisation curve. In one plenary session, the speaker is asked about "the problem of having so many standardisation curves". In one textbook, Cook and another reputed dendroclimatologist, Keith Briffa, acknowledge that the choice of standardisation curve will have consequential effects on the resultant mean chronology and climate reconstructions. However, they refuse to propose any guidelines for choosing a standardisation curve, because "this decision is likely to be completely data and application dependent". They do insist that colleagues "never use any tree-ring standardization method or computer program as a *black box*."[199]

> The epistemological conundrum that dendroclimatologists face at this stage of the production of knowledge is deciding which standardisation methods to choose in order to minimise, as far as possible, the influence of those perturbing influences, and to bring to the fore those changes in tree-ring growth that can be attributed to climate alone. I characterise Rob and Miloš' efforts to clean and standardise the Scottish data as involving three stages: identification, confirmation and removal of noise. The standardisation work performed by Rob and Miloš is an example of *innovation* of scientific practices. As I describe below, and largely as a result of the specificities of the Scottish environment and tree-ring patterns, Rob and Miloš face specific difficulties in applying traditional standardisation techniques. As a result, they have to find new ways of standardising their data, and making these new methods credible to colleagues and outsiders.

[199] Cook and Kairiukstis. *Methods of Dendrochronology, p.*161.

5. 2 Cleaning the Scottish Data

5.2.1. Identifying Noise

In 2007, Rob first notices that "something is off" with the ring-width data from Scotland when a master's student generates data from Glen Affric, a site in the North Western region of the Scottish Highlands. In particular, Rob notices that the ring-width chronologies do not show the declining ageing trend that he expects to find. The ring-width chronologies show irregular peaks of growth around the decades 1820-30AD, which Rob knows are not related to any documented increase or decrease of temperatures in Scotland. Rob does not have any reason to suspect that the fluctuations in the ring-width data are due to any error in the measurements, as he thinks of the undergraduate student who generated the data as an "excellent lab worker". Rob suspects that the abnormal ring-width patterns from Glen Affric could be related to disturbance events, but he does not know exactly what these events are.

From 2008 to 2012, Rob and the rest of the members of the Scottish Pine Project fieldwork team select other sampling sites throughout the Scottish Highlands with the aim of investigating, among other issues, the geographical spread of the disturbance identified in the Glen Affric chronologies. In one report that Rob prepares in 2008 for the landowners of the Rothiemurchus Estates in exchange for being granted easier access to the sites, he explains that "tree-ring data were also utilised from various other sites throughout the Highlands to determine the applicability of the results from Glen Affric to the remainder of the country".[200] Rob soon discovers that many of the new ring-width chronologies, especially from the West of the Scottish Highlands, show similar irregular patterns for the years 1820-30s.

Rob investigates the nature of these disturbances by familiarising himself with the ecological history of Scotland. He reads a couple of books by two Scottish historians that document the occurrence of severe storms and logging events in the Scottish Highlands for the period (1820-1830) where Rob has observed disturbances in the Scottish data.[201] In particular, these historical sources report that pine woodlands in the Scottish Highlands suffered consecutive thinning and clearcutting from the 16th century onwards; the period of greatest

[200] Rob Wilson, *Dendrochronological Investigations of Scots pine from the North-West Cairngorms Region, Scotland*, Unpublished report prepared for the Rothiemurchus Estates 2008. http://www.st-andrews.ac.uk/~rjsw/ScottishPine/PDFs/2008%20Pine%20Report.pdf

[201] Alistair Dawson, *So Foul and Fair a Day: a History of Scotland's Weather and Climate* (Birlinn, 2009); Smout, MacDonald and Watson, *A History of the Native Woodlands.*

activity was the 19th century, due to the Napoleonic Wars (1803-1815), when timber was extracted for economic and war efforts. On this basis, Rob names the disturbances he observes in the ring width chronologies during the early and mid-19th century "Napoleonic Impact Bias".

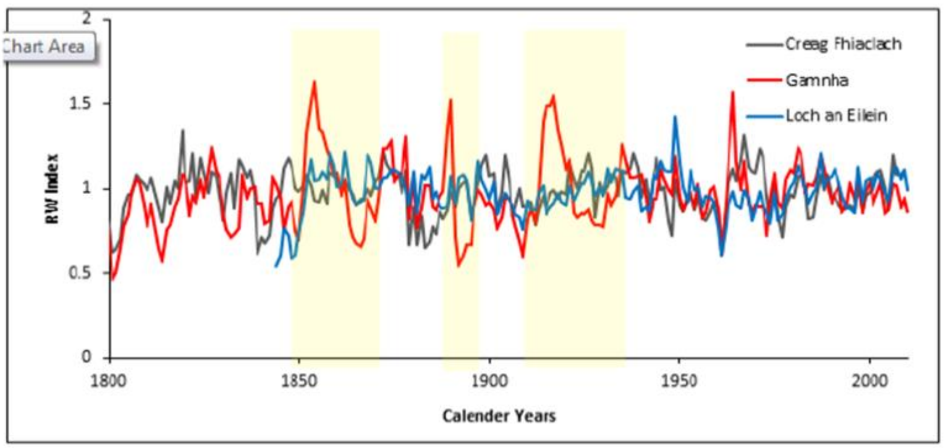
Leah, the dendroarcheologist of the Scottish Pine Project, assists Rob in understanding the nature of this disturbance. Leah provides Rob with information about changes in the timber supply and woodland resources in Scotland through the analysis of historical documents such as diaries and official documents. Like Rob, Leah is interested in locating all the remaining Scots pine-grown pinewoods of Scotland and generating a long tree-ring chronology with material from archaeological buildings. However, their collaboration is established upon a different definition of the signal and noise aspects of the data. Rob often dismisses jokingly as "shite" the effect of forest management on ring width data, which is Leah's interest. She is well aware that the divergence of interests with Rob suits both parties. "I am happy to work with Rob's 'leftovers' and what he calls 'disturbance'", Leah says.

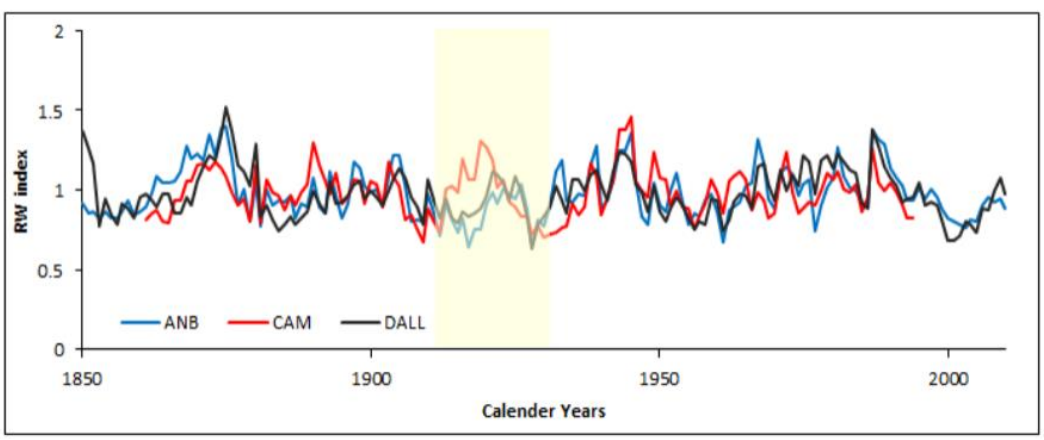
In 2012, Rob seeks his own independent confirmation of the presence and nature of human disturbance in the Scottish data. If disturbance indeed exists, Rob hypotheses, he will observe how the effects of logging and forestry management translated into wider ring-width patterns. This is because the removal of trees in a forest decreases competition between trees for light and nutrients and allows the remaining trees to grow faster. Rob delegates the execution of this experiment to an undergraduate student, Chloe, who conducts this work as part of her dissertation. The experiment consists of comparing "control" ring-width chronologies from relatively lightly managed woodland sites with chronologies from more highly impacted woodlands in the East of Scotland. With Leah's help, Chloe has been able to create a list of "periods of disturbance" from historical records.

Chloe reports that the chronology from trees near Loch Gamhna deviate from the disturbance-free chronologies (Loch an Eilein and Creag Fhiaich) during three periods, which coincide with intense forest management events she and Leah have identified in the Rothiemurchus Estate (image 23a). The comparison of three chronologies from Rannoch - a more southern site in the West of Scotland - shows that chronologies deviate from each other in the first half of the 20th century (image 23 b), coinciding with the Forestry Commission takeover of the forest in 1947. For Rob's dendroclimatological purposes, the interest is not only to identify disturbance, but to quantify it. Therefore, in the same study, Chloe compares the disturbance-affected and disturbance-free chronologies against thermometer records using linear regression analysis. She reports that the disturbance-free chronologies show better correlation results against temperature records. On this basis, Chloe concludes in her dissertation that "the hypothesis that woodland management has an impact on tree growth which may mask the response of trees to climate can therefore be accepted".[202]

Image 23. As part of her undergraduate dissertation, Rob's student generated these two graphs that Rob employs as evidence of the presence of confounding noise in two datasets from the Cairngorms. The highlighted areas where the red line deviates are interpreted by Rob and Chloe as evidence of human disturbance and the effect of logging and forest management on ring-width data.

(a)





(b)

Source[203]

[202] Referencing this source would disclose the identity of my research subject.

[203] Referencing these two graphs would disclose the identity of my informant.

2.2.2 Confirming Noise

After identifying the presence of human disturbance at two sites in the Cairngorms, Rob decides to employ modelling as a method to confirm the existence of disturbance within the wider dataset from the Scottish Highlands. In particular, Rob would like to employ a model of tree-ring-width formation called "VS-Lite" to generate a hypothetical picture of what a ring-width chronology would look like if only the effects of climate in Scotland affected tree growth. Rob's aim is to provide another kind of standard against which the Scottish data can be compared, so that he can confirm the effects of disturbance. "Any differences between the modelled and observed ring-width data", Rob reasons, "can only be attributed to disturbance".

VS-Lite, and tree-growth modelling in general, is a relatively recent method used in dendroclimatology to identify the climate signal.[204] The conventional "empirical-statistical approach" involves the use of linear regression to model the patterns of variation between ring-width and climate, in the same way that Chloe does in her dissertation. However, after decades of research, dendroclimatologists and other scholars have concluded that the effect of temperature and precipitation on tree growth is not linear and that linear regression analysis is a limited technique for modelling the relationship between the two. The "process-modelling approach" represented by VS-Lite is used by dendroclimatologists to simulate the non-linear physiological processes by which trees respond to climate. The VS-Lite is a simplified version of another tree-growth model developed by two dendroclimatologists whose surnames are Vaganov and Shashkin, hence the initials of the model. [205]

Rob delegates to Miloš the work of producing the modelled ring width chronologies with VS-Lite and comparing them against the observed ring width chronologies from Scotland. In order to learn how to use the model, Rob arranges a visit for Miloš to the Lamont laboratory in New York where one of the main creators of the VS-Lite, Kevin Anchukaitis, works at the time. Rob defines Kevin as a "young rising star in the field"; they have collaborated together a few times, including on their response to a recent controversy in dendroclimatology (Chapter 7). Lamont is the laboratory that Cook helped establish in 1975 after graduating from Arizona, and has become a "centre of pilgrimage", where many dendroclimatologists want to work at least

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[204] Hughes, "Dendroclimatology in High-Resolution Paleoclimatology.".
[205] Tolwinski-Ward, et al. "An Efficient Forward Model of the Climate Controls on Interannual Variation in Tree-Ring-width.", *Climate Dynamics, 2011, Vol.* 36.(11-12), pp. 2419-2439.

once in their careers. Rob himself was a postdoctoral researcher at Lamont, and maintains his affiliation with the laboratory as an adjunct scientist.

During his laboratory visit to Lamont in August 2012, Miloš learns from Kevin Anchukaitis how to interpret and manipulate the source code and the parameters of the VSLite model. In particular, VS-Lite employs a simulation technique called “Monte Carlo” that produces random runs of ring-width data by incrementally varying the input monthly temperature and precipitation data in terms of a set of 12 adjustable parameters. These parameters set the conditions by which the model simulates the limiting effect of temperature, and soil moisture availability on ring-width during an established period of time. In the article where they present VS-Lite, its creators explain that these parameterisations are a “simple implementation of the principle of limiting factors”[206] , the same principle that inspires the practice of site selection (Chapter 2). For instance, the temperature parameters simulate the limits of temperature on growth on the assumption that trees do not grow below freezing temperatures and above 20°C. Likewise, the “window” parameter that Miloš employs establishes that tree growth is limited to a period of 15 months (from the previous year’s September through the current year’s December for each simulated year). The creators also explain that they have borrowed the soil moisture parameters from a model of hydrology (the “Leaky Bucket Model”) developed by other scientists.

In order to use VS-Lite, Miloš adjusts the existing parameters because he thinks that they do not accurately model the peculiarly wet conditions of trees growing in the West of Scotland. In particular, he includes an upper soil moisture threshold to simulate the way high amounts of water in the soil become limiting to growth. When I ask Miloš how he has come up with the idea of including a new parameter in the model, he refers to his intimate knowledge of the growing conditions of trees in the West of Scotland that he acquired during fieldwork. “I just imagined that the wetness we saw in the field had to have an effect on how trees grow in Scotland”, Miloš explains.

Miloš insists that a competent use of VSLite involves adjusting the parameters in a way that makes sense in terms of what the researcher knows about the specific ecology of an area and the general physiology of trees. “I use the parameters as a starting point. You can experiment, but you have to have a justification for the values you use. I mean, I can’t set this upper limit [temperature threshold] to 50 degrees or this lower limit to minus 22 degrees, because it does not make any sense

[206] Tolwinski-Ward, et al. "An Efficient Forward Model.”, p.2420.

in terms of how the tree grows". Miloš also insists that his decision to adjust the model is in line with previous uses of the model, as "this new parameter isn't just any random addition because it was actually included in the full VS model".

Miloš tests the reliability of his adjusted model against observed data. He creates two modelled ring-width chronologies for the West and the East of Scotland respectively and discovers discrepancies against the observed chronologies. Miloš reports that the correlation between the modelled and observed chronologies from the East is lower than the one in the West. Miloš infers that lower correlation and worse agreement implies more disturbance, which he sees as a confirmation of the original hypothesis that disturbance was more present in the Western sites like Glen Affric due to high levels of precipitation and the effect of logging. Miloš employs these results as an indication of the need to adopt corrective method that eliminates disturbance.

5.2.3. Eliminating Noise

Rob and Miloš use a method called "Combined Step and Trend intervention approach", later refined into "Combined Curve and Trend intervention approach" ("CCT" hereafter) to remove the effect of human disturbance on the Scottish dataset. CST was created by Daniel Druckenbrod ("Dan"), a US dendroecologist whose objective is opposite to that of Rob and Miloš, as disturbance is the signal in which he is most interested.

Essentially, CCT estimates the effect of disturbance on ring-growth and reconstructs the history of disturbance events for a forest. To develop CCT, Dan drew on Cook's ARSTAN standardisation methodology based on AutoRegressive Modelling (ARMA). This means that CCT estimates the auto-correlation effect or the non-random patterns in data and identifies periods of unusual auto-correlation or growth, which are predefined as disturbance events. The program defines "unusual" growth in terms of growth falling above or below a certain minimum and maximum statistical threshold. On the basis of this threshold, CCT corrects the ring-width measurements by using an iterative curve-fitting mechanism that flattens out the trend in the chronology. The result is a new standardised tree-ring chronology that represents a disturbance index.

Rob learns about CST through Cook, who recommends it to him in a workshop that they both attend in June 2012. During the meeting, Rob explains the problem of disturbance he and Miloš have with the Scottish dataset, and Rob remembers Cook mentioning that he was co-authoring a paper about a method that Cook describes as "magic". In this paper, which Cook has co-authored with Dan and another dendroclimatologist from Lamont (Neil Pederson), they present CST as a “tool for reconciliation” between dendroecologists and dendroclimatologists. They state that “as a final objective, we propose that a time series approach [as employed in CST] has the potential to bridge a divide between dendroclimatology and dendroecology that would enable more complementary analyses of tree-ring series”.[207] Dan’s objective is to demonstrate how a statistical technique (“time-series analysis” technique), originally devised by Edward Cook to detect the climate signal, could be used by dendroecologists to detect the disturbance signal. The irony is that CCT is eventually used by Rob and Miloš for removing, rather than isolating, the disturbance signal.

Rob and Miloš’ successful appropriation of CST for the Scottish Pine Project is the result of the mutual adjustment of interests between all parties. These negotiations first take place when Miloš visits the Lamont laboratory in August 2012 to learn about VS-Lite, and meets Dan fortuitously. Dan is also visiting the Lamont laboratory to draft the article about CST with Cook and Pederson. Miloš explains to Dan about the disturbance problem in Scotland and they agree to collaborate. When I ask Miloš by email how important this meeting was, he responds, “I don't know how important the meeting itself really was, but the good thing about it was that I had a chance to talk to Dan face-to-face which is always helpful if you want to start working with someone”. Miloš later explains that, at the time, Dan had not published any articles about CST, and so Miloš could learn first-hand how to use it. Miloš tells me that Dan is pleased that, thanks to his collaboration with Rob and Miloš, he has discovered a “new potential application” for CST. Also, with Miloš’ help, Dan is using the Scottish data to develop the more “refined” CCT version originally developed from CST. This improvement entails more work for Miloš, who has to redo all the CST analysis of the Scottish data with the new CCT version. However, Miloš conceives this extra work as part and parcel of collaborating with Dan.

The adoption of CST for dendroclimatological purposes is also conditional on approval from the wider community of dendroecologists and dendroclimatologists who have come to think of Miloš’ work very highly. In the international dendrochronology conference in Australia in January 2014, Miloš receives a prize for the best poster, on which he presents his results on the use of CCT with the Scottish data. A couple of conference attendees I talk to praise the fact that the CCT method “bridges the gap” between the disciplines of dendroecology and

[207] Daniel Druckenbrod et al., "A Comparison of Times Series Approaches for Dendroecological Reconstructions of Past Canopy Disturbance Events." *Forest Ecology and Management*, 2012, Vol. 302, p. 24.

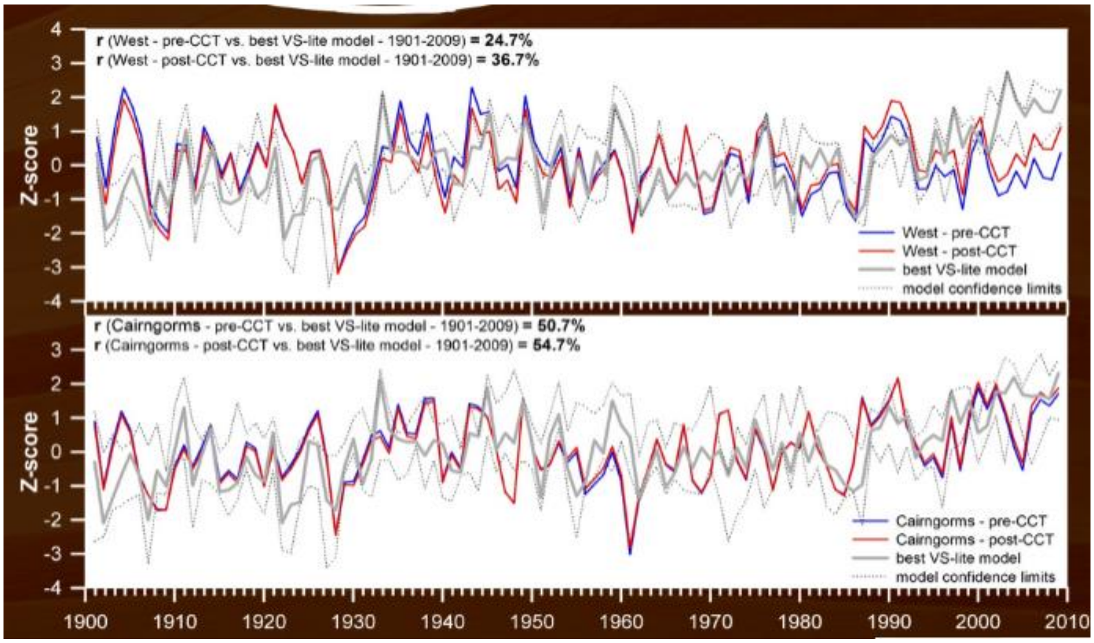
dendroclimatology.

Rob thinks that the CCT methodology is, in fact, the most “innovative” element of Miloš’ PhD thesis and speculates that the paper that Miloš is starting to write with Dan will be highly cited. Rob thinks of CCT as a potentially revolutionary method as “for a very long time, dendroclimatologists assumed that our chronologies were disturbance-free, but this methodology might reveal that we’ve been wrong all the way”. Rob imagines a situation in which archived ring-width chronologies could be re-analysed with CCT so that dendroclimatologists can discover whether chronologies contain disturbance.

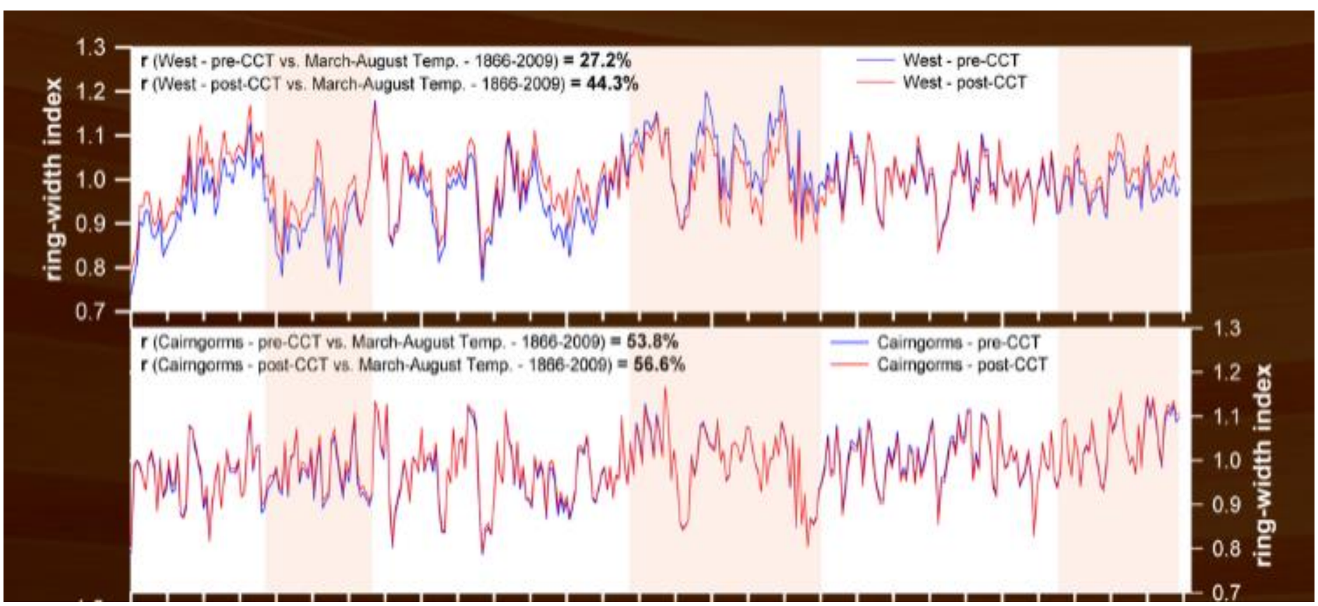
Miloš designs different tests to ascertain the reliability of his corrective method. Essentially, he compares the corrected and uncorrected chronologies with CCT against the simulated chronologies that he has produced by VS-Lite and thermometer records. Miloš reports that the chronologies from the West become more similar to both modelled chronologies and temperature records after being corrected with CCT than the chronologies from the East (image 24). He interprets these results as confirmation that the Western chronology is more extensively affected, and thus corrected, by disturbance.

When I ask Miloš if he expected the correlations to be higher, he responds that “The reason I did not expect the post-CCT [corrected chronologies] results to be better than they were is because the method is not perfect and actually any improvement at all is a good sign”. In fact, Miloš suspects that CCT is “over-correcting” the chronologies and removing part of the climatic signal. He has discovered that, in some cases, the correlations between corrected and temperature records have worsened rather than improved, which he sees as evidence of the limitations of the method. Miloš is planning to investigate this issue further with Dan, but he says, “For the purpose of cleaning the chronologies from disturbance, CCT is good enough for me now”.

Image 24. Miloš includes these two sets of graphs in his prize-winning conference poster to justify the use of VS-Lite and CCT to confirm and remove noise respectively. The first two graphs (a) compare the uncorrected (pre-CCT)) and corrected (post-CCT) chronologies from the West (top graph) and the East (Cairngorms - bottom graph) of the Scottish Highlands against modelled chronologies. The next two graphs (b) compare the same uncorrected and corrected chronologies from the West and the East against temperature data. The correlation coefficients included are meant to show the "improvement" in the degree of similarity of corrected chronologies (post-CCT) against modelled and temperature data.



(a)



(b)

Source: Rydval et al., "Detection and Removal of Disturbance Trends in Tree-rings for Dendroclimatic Purposes", Poster Session, January 2014.

Having identified and partially eliminated the effect of disturbance on the ring-width data, Rob and Miloš still face the difficulty of deciding which standardisation curve to use to eliminate the ageing effect in both ring-width and Blue Intensity chronologies. Rob explains that after years of research he has concluded that “there is no right or wrong way to detrend the data”. Rob tells me that in all his previous work, he has always used more than one standardisation curve to produce standardised chronologies. Rob remembers how his colleague Jan Esper “used to make fun of me that I could not decide on which was the best version, but I always argued that they all had strengths and weaknesses”.

As a means of keeping some flexibility in the face of the indeterminacy of standardisation curves, Rob uses a method that he calls “the ensemble approach”. Essentially, the ensemble approach consists of generating variations of standardised chronologies with different standardisation curves (what Rob calls “flavours”) and evaluating their accuracy in terms of their coherence to temperature data. When I ask Rob whether this approach is common practice among dendroclimatologists, he refers to a European paleoclimatology project “where we discussed a lot about uncertainty”. Rob points to a paper published by his friend and head of the dendroclimatology laboratory in the WSL laboratory in Switzerland, David Frank, as “the ultimate extension of this concept”. Rob clarifies that the ensemble approach is “not yet common practice, but it kinda is for Jan, me, Dave, Ulf etc.”, referring to some of his colleagues (Jan Esper, David Frank and Ulf Büntgen).

Miloš experiments with four standardisation curves as part of the ensemble approach. To detrend the blue intensity chronologies, he employs the “classic” Arizonian negative exponential curve and another deterministic curve (the “Hugershoff” curve) that assumes a negative linear trend in the data. To detrend the ring width chronologies (after correcting them with CST), Miloš experiments with two detrending methods that dendroclimatologists have developed to resolve some of the limitations of the negative exponential curve (“Regional Curve Standardisation”[208] and “Signal-Free”[209]).

The fact that the standardised chronologies resulting from the use of the Signal Free

[208] Keith Briffa and Thomas Melvin, "A Closer Look at Regional Curve Standardization of Tree-Ring Records: Justification of the Need, a Warning of Some Pitfalls, and Suggested Improvements in its Application." in M. K. Hughes, H. F. Diaz and T. W. Swetnam, (eds.), *Dendroclimatology: Progress and Prospects* (Springer Verlag, 2011), pp. 113-145.

[209] Thomas Melvin and Keith Briffa, "A ‘Signal-Free’ Approach to Dendroclimatic Standardisation”, *Dendrochronologia*, 2008, Vol. 26 (2), pp. 71-86.

curve offer better results (because they are more strongly correlated against thermometer records) poses a problem of interpretation for Rob and Miloš. They admit that they do not know how and why Signal Free works better than the other standardisation curves. Rob says, "The mantra is that it is a better method, but few people, including myself, fully understand why it works better". Signal Free has a reputation as a highly intricate standardisation method. During the field week in Tasmania, Cook gives a lecture about Signal Free, introducing it as "one of the most original and intriguing PhD dissertations ever done in the history of dendroclimatology". Rob and Cook explain the nature of Signal Free in relation to the character of its creator, the dendroclimatologist Tom Melvin, whom they regard as a "genius". Because of the unintelligibility of Signal Free, Cook decides to co-develop with Melvin software that "translates" this method to others. The "lite" version of Signal Free has become the most commonly used version by many researchers, including Rob and Miloš.

Rob and Miloš are concerned about the way outsiders to the community of dendroclimatology will interpret their standardisation choices and results. In particular, they worry about criticism from people whom they call "sceptics", who use blogs to scrutinise the work of dendroclimatologists (Chapter 1). Rob distinguishes between different types of "sceptics". In February 2013, on my request, he gives a guest lecture titled "Interacting with sceptics. Is it worth the effort?" to a group of undergraduate students from the University of Edinburgh, where he distinguishes between three types of "sceptics" and forms of interaction. Rob refers to the first group as "non-believers", whom he sees as refusing to accept any scientific evidence and "not worth the effort, as they do not listen and do not want to". Rob labels the second group "cautious sceptical (non-believers)" because he thinks that "these individuals have some faith issues with regards to the science but they are generally willing to learn, and their minds are not closed". Rob specifically refers to a man called Andrew Montford, who has a blog called *Bishop Hill*, as a "cautious sceptical". Finally, Rob presents "the validating sceptic", whom he defines as "a few rare individuals who try to spend their time to work through studies"; he specifically refers to Steven McIntyre, who has a blog called *Climate Audit*. Rob defends engagement with this latter group as "vital, as unfortunately, mistakes and problems are found, and dialogue is needed to ensure clarity for the science".

Over time, Rob has developed a record of interactions with the "cautious" and "validating sceptics" Andrew Montford and Steve McIntyre respectively. In particular, Rob has interacted more with Andrew Montford who lives very near to St Andrews (Scotland). Rob often writes blog entries and comments on Montford's *Bishop Hill* blog. Montford and Rob have also participated together as speakers at a few talks and panels. On one occasion, Rob's collaboration with Montford got him into trouble with colleagues at the university. In May 2013, when a few of his Geography students approached Rob with the idea of organising a debate called "Grilling the environmentalist", Rob suggested inviting Montford as a panellist. A few of Rob's colleagues in the department openly opposed this idea, on the grounds that Rob was promoting "climate scepticism".[210] On another occasion, Rob asked Montford to collaborate in organising an activity with his undergraduate students. Rob asked students to conduct an experiment in class and to address the criticism by the readers of Montford's blog of a blog entry that described this experiment. Rob justifies this activity as a means "to get students thinking about how to deal with sceptics". Equally, Rob encourages Miloš to think about how "sceptics" will react to his doctoral results, particularly to his standardisation choices. In a conversation between Rob and Miloš, they discuss their concerns about how Signal Free works; the differences of credibility between Melvin and Cook's versions, and how to justify their standardisation choices to sceptics like Montford:

1. Miloš: You see, these results are really interesting because for some reason, with standard negative exponential detrending there is not much of an improvement in the chronologies, but if you do it with Signal Free there is an improvement after cleaning them with CST. But if you use the raw data [without CST correction] and you apply Signal Free, then the results are much worse. I am not quite sure why and how to interpret this…
2. Rob: Mm, I don't quite know Signal Free either. I've just toyed with it.
3. Miloš: So, you know, I am a little cautious about these results.
4. Rob: Yeah, yeah. No one really knows how it [Signal Free] works. I mean people are black boxing it.
5. Miloš: Well, I guess we will need to experiment with it then?
6. Rob: Yes, I don't think I truly understand how it works. I know how it works conceptually, but then you have to compare Tom and Ed's approach. I somehow trust Ed's version more, because Tom is just crazy! [Rob laughs]
7. Miloš: Hahaha [Miloš laughs]
8. Rob: Yes, Signal Free is certainly going to be an issue. Again, the ensemble approach is crucial here. You should do one version with Ed's version and

[210] Andrew Montford wrote a blog entry about this event: "St Andrews Green Week", *Bishop Hill*, 14 March 2013, accessed 15 July 2015, http://www.bishop-hill.net/blog/2013/3/14/st-andrews-green- week.html

another one with Tom's version and ideally, they should agree. We can make subjective and objective decisions with regards to standardisation; I am not too concerned about this. But we've just got to rationalise every step.

9. Miloš: Yes, yes
10. Rob: I am just thinking about the sceptics. We are actually in an interesting position because I know that Montford from Bishop Hill is very interested to see what comes out of Scotland. So we have to be clear about everything we do. I have actually agreed to write a blog post, but he will also keep an eye on our papers.
11. Miloš: Oh, yes [nervous laughing]
12. Meritxell: No pressure!
13. Miloš: Yeah [laugh], sure, no pressure at all!

5. 3. Discussion

Rob and Miloš seek to create standardised tree-ring chronologies that will be accepted by dendroclimatologists and outsiders as a historical record of climatic conditions in Scotland by making the various ways they standardise the data visible, and by offering those data for sceptical public evaluation. The "ensemble approach" devised by Rob represents the clearest example of the "cleaning and showing" strategy that Rob and Miloš employ in order to secure trust from both insiders and outsiders of the community of dendroclimatology.

The "cleaning" strategy whereby Miloš uses multiple standardisation curves to detrend and clean the data is a result of Rob's critique of colleagues who, in his opinion, are too trustful of a single curve and standardisation method. Rob's criticism is an expression of the organised scepticism that Cook and Briffa advocate in a textbook article when they advise against using "any tree-ring standardization method or computer program as a *black box".* Rob explicitly complains about the fact that colleagues, and even himself, are "black boxing" Signal Free and regards this lack of organised scepticism by the community as a problem because it opens the door to potentially uncivil forms of scepticism from certain "sceptics". Rob believes that doing competent standardisation work involves acknowledging the uncertainties of standardisation and not picking a single method. He also seeks to assert that being a trustworthy dendroclimatologist involves a willingness to be transparent about the uncertainty of standardisation methods.

The "showing" strategy represented by Rob and Miloš' intention to show the standardisation variants is an example of sceptical display. The showing strategy underlies the production of the graphs that Miloš produces with VS-Lite and CCT that are part of his prize-winning poster at the conference in Melbourne, whereby Miloš shows the improvement of the dataset before and after removing noise. The showing strategy is also inspiring the production of the variants of reconstructed maps that I include in the next chapter whereby Miloš shows the progressive improvement in the correlation coefficients as he adds more tree-ring parameters and associated standardisation techniques into the analysis.

These forms of sceptical display are aimed at reinforcing the trust relations that Rob maintains with colleagues and certain trustworthy "validating and cautious sceptics" like Montford. Rob's expectation is that trusted sceptics will perform civil scepticism and treat his results seriously, fairly and at face value. Unlike his colleagues in the Geography department, Rob distinguishes between those sceptics he trusts to engage in constructive scepticism and those "non-believers" who he does not trust to listen. The fact that Rob's colleagues do not have trust relations with Montford explains why they regard Rob's attitude as a reason for mistrusting him.

Despite all their efforts, Rob and Miloš are nervous about the risk of uncivil scepticism from untrusted others. They are keen to exert as much control over the interpretation of their "transparent" data as possible. As Rob says to Miloš, "we've just got to rationalise every step". To perform the kind of scepticism and "showing your workings" strategy that Rob and Miloš hope will secure the trust of their community and outsiders in their standardisation methods and data, they rely on existing trust relations.

Rob and Miloš' work of standardisation is substantiated on a fiduciary framework and the trust that many dendroclimatologists have placed on Cook's conceptual model of tree growth and programmes. The increasing social robustness of the trust relations underlying this model is expressed in the fact that the model of aggregate tree-growth has become included as a "principle" in the most recent textbook written by Jim Speer. Edward Cook as an individual is trusted and regarded very highly (and hence awarded in the WorldDendro conference in Melbourne) by his dendroclimatology peers for his various contributions to the community. The trust that dendroclimatologists place in Cook can also be seen in the fact that people like Rob and Miloš borrow Cook's language of "disturbance" to refer to the anomalies in the Scottish dataset.

The fact that Cook explicitly says that the assumption that growth factors (climate, age, and disturbance) have a linear and aggregate effect on trees is "a necessary oversimplification" suggests that dendroclimatologists use the “model of aggregate tree-growth” as a useful fiction. By collectively suspending their disbelief and trusting the model “As-If” it accurately described the growth of trees, dendroclimatologists are able to achieve standardisation. The fiction of the model of linear aggregate tree growth also serves to establish trust relations and collaborations between dendroclimatologists and dendroecologists like Rob and Leah on the basis of a distinct definition of “signal”.

Rob and Miloš have relied on Cook’s expertise and brokering role in order to standardise the Scottish data. Cook first recommended CCT and Dan’s work to Rob. Cook also created the Signal Free “lite” version that Rob, Miloš and other dendroclimatologists trust more to generate cleaner chronologies. The trust relations between Edward Cook, Rob and Miloš have been constituted by interactions in the context of workshops, conferences and Miloš’ laboratory visit to Lamont. Field weeks like the one in Tasmania are also crucial training spaces where neophytes more generally are able to appreciate Cook’s insider knowledge about the “hidden functions” of the standardisation computer software and to trust Cook as an exceptionally expert dendroclimatologist.

Rob liaises between existing trusted collaborators whom he trusts will standardise the Scottish data with care and a critical attitude. He liaises between the Scottish historians, Leah and Chloe to conduct an independent sceptical examination of the effects of forest management and to identify the “Napoleonic Impact bias” in two ring width datasets from the Scottish Highlands. The trust relation between Rob and Leah is based on a well institutionalised cognitive division of labour between dendroclimatologists and dendroarcheologists in the use of the disturbance noise/signal. To confirm the existence of noise in the wider Scottish dataset, Rob liaises between Miloš and Kevin. As a result of his laboratory visit, both Rob and Kevin entrust Miloš as sufficiently competent to use VS-Lite. [211] More generally, the features of the VS-Lite model itself are the result of various anonymous and more familiar trust relations between the co-creators of the VS-Lite model and others, including the creators of the full VS model and the hydrology scientists from whom the VS-Lite authors borrow one of their parameters (“Leaky Bucket Model”).

To establish new trust relations that will allow him to present his standardisation methods and data to further public scrutiny, Miloš employs his expert knowledge of the Scottish sites and

[211] For other examples of how laboratory visits engender trust relations, which in turn facilitate replication see Collins,"Tacit knowledge, Trust and the Q of sapphire”.

dataset. On the one hand, he appeals to his experience of the wetness of the Scottish site to justify his modification of the standard VS-Lite model, and presumably, as a reason for Kevin to entrust him to perform a "realistic" and competent adjustment of the VS-Lite model. On the other hand, Dan entrusts Miloš with developing CST because Miloš knows the history of management interventions in the Scottish Highlands and is able to infer such human disturbance from the patterns in the tree-ring chronology. As one of the conference attendees acknowledges in a conversation with me, one of the merits of Rob and Miloš' appropriation of the CST method - and presumably one of the reasons why Miloš was awarded the poster prize - is that it "bridges the gap" and builds new relationships of trust between dendroecologists and dendroclimatologists. On the basis of the new trust relations that Miloš establishes with Kevin and Dan, he is able to use VS-Lite and CCT to confirm and eliminate noise respectively.

To complete the work of standardisation and remove the effect of disturbance, Miloš needs to suspend his scepticism about the CST method temporarily. Miloš knows that the resulting chronologies might not be totally clean of disturbance and might be "over-corrected". Yet, as Miloš says, "CST is good enough for me now", and he believes that this method could be improved. Indeed, Miloš' trust relation with Dan is the basis upon which they are planning to sceptically examine the CST method in the future. Miloš' temporary suspension of scepticism about the quality of the corrected chronologies allows him to progress to the last stage in the production of dendroclimatological knowledge.

6 Reconstruction

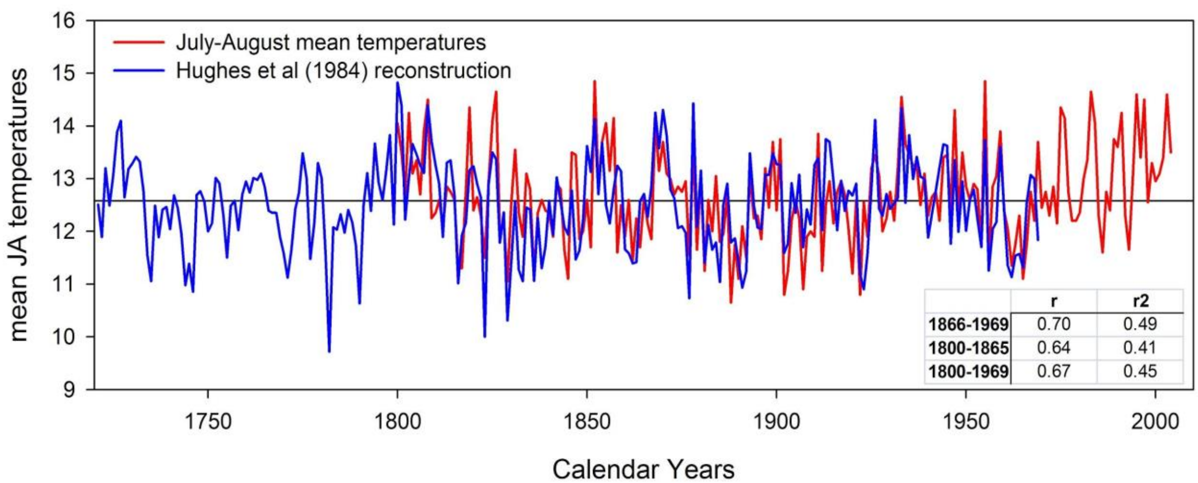
6.1 The Establishment of Extrapolations Back in Time

After having sampled trees, counted and dated tree-rings, measured the width and reflectance of tree-rings and eliminated disturbance noise from the data, Rob and Miloš are finally able to reconstruct the climate of Scotland for periods before temperature records. Their motivation for creating a temperature reconstruction is twofold.

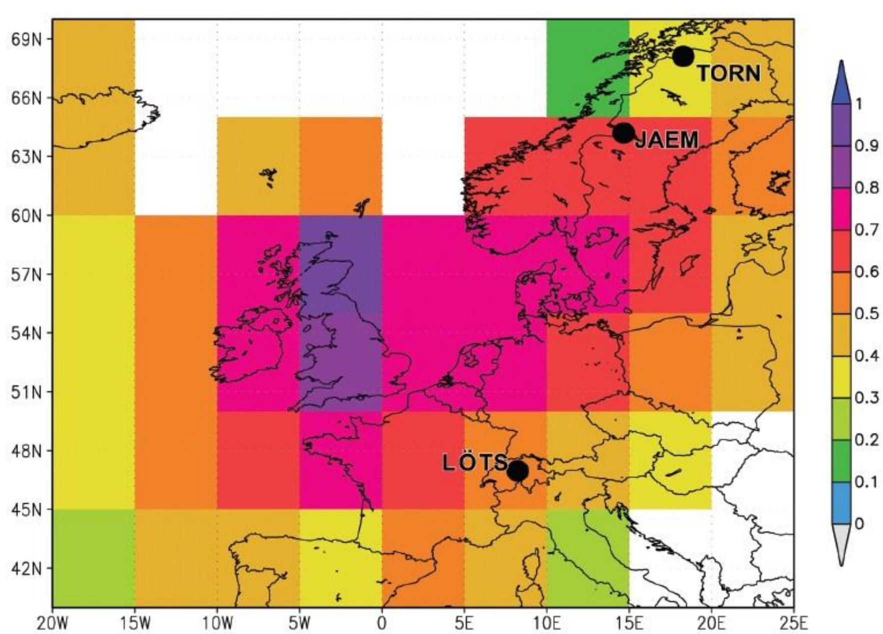
First, Rob wants to extend backwards and update to the present the Scottish reconstruction that Malcolm Hughes published in 1984 that went back to 1721 AD. On the website of the Scottish Pine Project, Rob presents the promise of a Scottish reconstruction using a graph with Hughes' reconstruction and the following comment (image 25a): "The original Hughes reconstruction is shown below showing the excellent calibration potential of this species [Scots pine] in the Scottish Highlands. Although some success has already beenen made in finding older living sites, the truly exciting work will be related to extension of the living material with either historical or sub-fossil material." Rob and Miloš are employing the 800-year-long chronology that they have created from sub-fossil samples from the Cairngorms (Chapter 3) to extend Hughes' reconstruction back to 1200 AD.

Rob and Miloš' second goal is to reconstruct the temperature of Scotland through space. By using the chronologies from the West and East of the Scottish Highlands, they want to show how climate has changed over time across sub-regions in Scotland. Instead of a single graph - as is the case with the extended reconstruction from the Cairngorms - the result of a spatial reconstruction is a succession of maps of reconstructed temperatures across Scotland. On the website of the Scottish Pine Project, Rob justifies the spatial reconstruction in terms of its usefulness for providing information about large scale spatial climate patterns and its complementarity to existing reconstructions. Rob includes a graph (image 25b) and the following comment: "The spatial correlation below (...) clearly shows the potential importance of such a summer temperature reconstruction for providing information of past climate for the NW European sector. Such a reconstruction will complement similar tree-ring based summer temperature reconstructions from Scandinavia, the Alps and the Pyrenees"

Image 25. On the website of the Scottish Pine Project and other publications, Rob uses these two graphs below to justify the purpose of the Scottish reconstruction. The graph at the top (a) is Malcolm Hughes' reconstruction (in blue) and temperature data (in red) and the correlation coefficients shown in the table bottom right, which Rob interprets as a promise of an extended long reconstruction. The graph (b) is a map of Northern Europe and the potential for a temperature reconstruction of the Scottish region (expressed in high correlation coefficients and associated purple colours) that would complement the other existing European reconstructions shown in dots.



(a)



(b)

Source: "Motivation", Scottish Pine Project, https://www.st-andrews.ac.uk/~rjsw/ScottishPine/motivation.html

The work of creating quantitative estimates of past climate is described in all dendroclimatology textbooks and Rob's undergraduate classes as involving the stages of "calibration" and "verification". Dendroclimatologists first use half of the meteorological data to establish a calibration or a relationship between observed temperature/ precipitation data and tree-ring data. Afterwards, they verify the reconstructed climate data by comparing it against the other half of meteorological data withheld from calibration. Often, dendroclimatologists invert the data used for calibration and verification to check if the results remain the same for the two periods of data.

Dendroclimatologists employ a statistical technique called "linear regression analysis" to reconstruct past temperature or precipitation values. With the use of software programs, they create a "response function" that models how the tree "responds" to temperature/precipitation data during the calibration period. The computer predicts past temperature/precipitation data with linear regression analysis by inverting the calibration equation and using tree-ring data as the predictor and instrumental records as the predicted. The resulting equation is referred by dendroclimatologists as the "transfer function" as the tree-ring data are "transferred" into reconstructions of climate.[212] Ultimately, the regression of climate data from tree-ring data involves the assumption that the variations between the climate and tree growth in the calibration and verification period will extend backwards to the past. Dendroclimatologists use correlation coefficients and other statistics to test the "skill" or the reliability of their extrapolations.

Dendroclimatologists, as in all other paleoclimatic disciplines, base their extrapolations on the "principle of uniformitarianism". The definition and attributed authorship of the principle of uniformitarianism have long been a source of dispute among geologists.[213] In their textbooks, dendroclimatologists ignore these debates and offer a standard definition of uniformitarianism with the sentence "the present is the key to the past" attributed to the 18th century Scottish geologist James Hutton. As defined by Harold Fritts in his 1976 textbook, the uniformitarian principle "implies that the physical and biological processes which link today's environment with today's variations in tree growth must have been in operation in the past". Fritts insists that uniformitarianism does not mean that the past climate is the same as the climate in the present, but,

[212] Fritts, *Tree Rings and Climate*, p.318.

[213] I am not aware of any sociological history of these disagreements. The most representative publications representing this disagreement are: J. S. Gould, "Is Uniformitarianism Necessary?" *American Journal of Science*, 1965, Vol. 263 (3), pp. 223-228; and James Shea, "Twelve Fallacies of Uniformitarianism", *Geology*, 1982, Vol. 10 (9), p. 455.

"it does imply that the same *kinds* of limiting [climatic] conditions affected the same *kinds* of [tree physiological] processes in the same ways in the past as in the present".[214]

Over the years, dendroclimatologists have realised that the uniformitarian assumption does not always hold true. The constitution of the "divergence problem" as a research topic in dendroclimatology is an example of this acknowledgement.[215] The divergence problem refers to dendroclimatologists' observation that, in some sites in the Northern Hemisphere, ring width data and temperature trends appear to have diverged in recent decades. Since the 1960s, temperature has been recorded as steadily rising but tree-ring data shows it has been declining or not increasing so much. Because dendroclimatologists attribute superior credibility to thermometers as recorders of climate than trees, they have concluded that the observed divergence is related to limitations of tree-ring data. The identification of divergence has led outsiders to the community of dendroclimatology to question uniformitarianism and dendroclimatology as a result. Outsiders reason that if there is a discrepancy between a few tree-ring datasets and warmer temperature records in modern times, such divergences could also occur in the past and render the assumption that the relationship between tree growth and climate is stable over time false.[216]

Dendroclimatologists - Rob included - have proposed numerous theories that explain the phenomenon of divergence. Divergence is still a topic of ongoing concern for dendroclimatologists and they have not reached any consensus with regards to its causes. At the international dendrochronology conference I attend in Melbourne in January 2014, Rob chairs a "divergence session" where dendroclimatologists discuss how methodological practices could generate what they call "spurious" divergence. Rob himself is actively involved in researching divergence and his most cited paper is a review of the research on the subject.[217] Overall, dendroclimatologists do not regard divergence as a refutation of uniformitarianism as a whole;

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[214] Fritts, *Tree Rings and Climate*, p.14-15.

[215] The constitution of the "divergence problem" as a research problem would require a sociological history in itself. Jacoby and D'Arrigo were the first to publicly report this phenomenon, identified with the tree-ring chronologies from Alaska in the article "Tree Ring Width and Density Evidence of Climatic and Potential Forest Change in Alaska" published in 1995. Keith Briffa and others published "Reduced Sensitivity of Recent Tree-Growth to Temperature at Northern High Latitudes"in 1998 in the widely read journal *Nature*, which made the phenomenon known to wider audiences.

[216] Read McIntyre's blog entry and the comments on "Mike's Nature trick". *Climate Audit*, 9 November 2009. http://climateaudit.org/2009/11/20/mike%E2%80%99s-nature-trick/

[217] Rosanne D'Arrigo et al., "On the Divergence Problem in Northern Forests: a Review of the Tree-Ring Evidence and Possible Causes." *Global and Planetary Change*, 2008, Vol. 60 (3), pp. 289-305.

because they have been able to conclude that it is restricted to certain anomalous chronologies (they have observed divergence in a few ring-width and density chronologies from high latitudes trees).

Dendroclimatologists employ uniformitarianism as a working assumption with the acknowledgement that their extrapolations back in time are uncertain. As Jim Speer explains in his textbook "We know that our assumptions that present processes have not changed through time is not always correct, but uniformitarianism is a *productive starting point* in the analysis of past climates and environmental variability". [218] All the dendroclimatologists I witness presenting their climate reconstructions at the conference in Melbourne end their talks by saying something like "more data are needed to draw more definitive conclusions".

Rob and Miloš are also aware of the limited and temporary nature of their extrapolations about past climate in Scotland, and for this reason, their priority is to assemble more and more samples and to generate more and more data. Until the very last year of his PhD, Miloš never stops generating new standardised chronologies from the recently sampled sites in the Scottish Highlands. In fact, one of the reasons for the delay of the submission of his thesis is that Miloš decides to wait to see if Rob is able to cross-date the subfossil and living based chronologies from the Cairngorms to create an 800-year long chronology (Chapter 3). Rob and Miloš see the constant flow of "more data coming in" as a requirement for building up a cleaner climate signal and a mean chronology that offers good calibration and verification statistics against temperature records. The better the statistics, the more certain they can be of the reliability of the reconstructed temperature values outside the calibration and verification periods.

In this chapter, I use the notion of "finitism" to describe the work that Rob and Miloš carry out to create climate reconstructions and to extrapolate past climates from tree-ring data. Essentially, finitism is a theory of the way people classify and attribute meaning to items, but it has also been used to account for the way people behave and follow rules. Finitism has multiple philosophical origins, and the version I employ here has been developed by the sociologists Barry Barnes and David Bloor.[219] They explain that the "[finitism's] core assertion is that proper usage [of a term] is developed step by step, in processes involving successions of on-the-spot judgements".[220] The open-endedness of the meaning of terms derives from the fact that terms have only been employed a finite number of times. When individuals encounter a new item, they

[218] Speer, *Fundamentals,* 11. My emphasis.
[219] Barnes, Bloor and Henry. *Scientific knowledge*.
[220] Barnes, *T.S. Kuhn and Social Science,* 30.

have to decide whether this item is sufficiently similar to the previous items that they have classified using that term. Consider the concept of "murder", and how the existing laws and finite cases defined so far as "murders" do not suffice for all possible applications of the term in the present. This is why there is debate about whether "murder" includes the killing of enemy soldiers, human foetuses, animals (for scientific research or food) or terminally ill people who have expressed a wish to be helped to die.[221] Finitism suggests that present and future applications of terms such as "murder" depend on the agreement and often redefinition of other terms carried out by individuals at particular places and times. That is, the meaning of words is a function of social order.

Analogously, the epistemological conundrum that Rob and Miloš face at this final stage of the production of knowledge is, as they assemble more chronologies, to decide whether the finite evidence they have for the relationship between climate and tree growth during the calibration and verification periods holds for the temperature reconstructed period. The work of reconstructing climate involves an interpretation of the meaning of statistics and a decision on whether the resulting reconstructions can be classified as "true" or "false" representations of past climates.[222] Climate reconstructions are finitist insofar as they are open to revision and re-interpretation as dendroclimatologists generate new tree-ring data. In their work of reconstructing climate, I suggest that Rob and Miloš temporarily resolve the open endedness of establishing extrapolations back in time on the basis of a double strategy: "Trained Variation and Natural Selection" and "Complementarity".

6. 2. Finitist Climate Reconstructions

6. 2.1 Trained Variation and Natural Selection

Rob and Miloš create reconstructions by *enabling* "nature" to select the final reconstruction. After limiting the choices of nature's selection on the basis of their expertise as dendroclimatologists,

[221] I take this example from Hatherly, David; Leung, David and MacKenzie, Donald, "The Finitist Accountant: Classifications, Rules and the Construction of Profits" in Trevor Pinch and Richard Swedberg (eds.), *Living in a Material World: Economic Sociology meets Science and Technology Studies* (MIT Press, 2008), pp. 131-160.

[222] In the revised version, Miloš says that "I wouldn't see this as black and white type situation thought – we are talking about degrees of confidence".

Rob and Miloš delegate to nature the responsibility of selecting the most reliable reconstruction from different versions.

The sociologist Karin Knorr Cetina observed that the molecular biologists of her study employed a similar experimental strategy to resolve situations of uncertainty. Knorr Cetina characterised this behaviour as "blind variation" and "natural selection" in analogy with evolutionary biology. Knorr Cetina explains "They [molecular biologists] vary the procedure that produced the problem, and let something like its fitness -its success in yielding effective results - decide the fate of the experimental reaction".[223] Later on Knorr Cetina clarifies that rather than "blind variation" these molecular biologists deploy their expert knowledge to enable "trained variation" and the preselection of variations that are most likely to be selected by nature. Knorr Cetina writes that "variation in molecular biology, however, is by no means as sightless and undiscerning as the random genetic mutations from which the term *blind variation* is borrowed. For example, the experienced body of the scientist, when it operates, naturally brings its experience to bear on the variations it concocts for selection by success".[224]

In the case of dendroclimatology, natural selection and the fitness of a reconstruction is related to the ability of tree-ring chronologies to resemble the meteorological records. In fact, Rob and Miloš talk about the "skill" of reconstructions and whether reconstructions are a good "fit", meaning whether the reconstruction provides good calibration and verification statistics against temperature data. In particular, the way Rob and Miloš evaluate this similarity is through the use of correlation coefficients. They interpret a higher correlation coefficient between tree-ring data and temperature data as a good fit and an indication that the reconstruction is an accurate representation of historical changes in climate.

The work of reconstructing temperature, and the roles of natural selection and trained variation in it, starts with Rob and Miloš identifying the particular months of the year or the "target season" in which tree-ring data are most closely correlated to temperature data. This is the first step in the creation of the "response function" in the calibration stage. Rob and Miloš talk about "maximising" the climate signal of the reconstruction as the selection of the best correlated months of the temperature data will also provide the best calibration and verification statistics.

Generally, dendroclimatologists often employ the data of either temperature or rainfall and only aim to reconstruct one of the two climatic variables. This is because they work on the

[223] Knorr Cetina, *Epistemic Cultures*, p.91.
[224] Knorr Cetina, *Idem*, p.109.

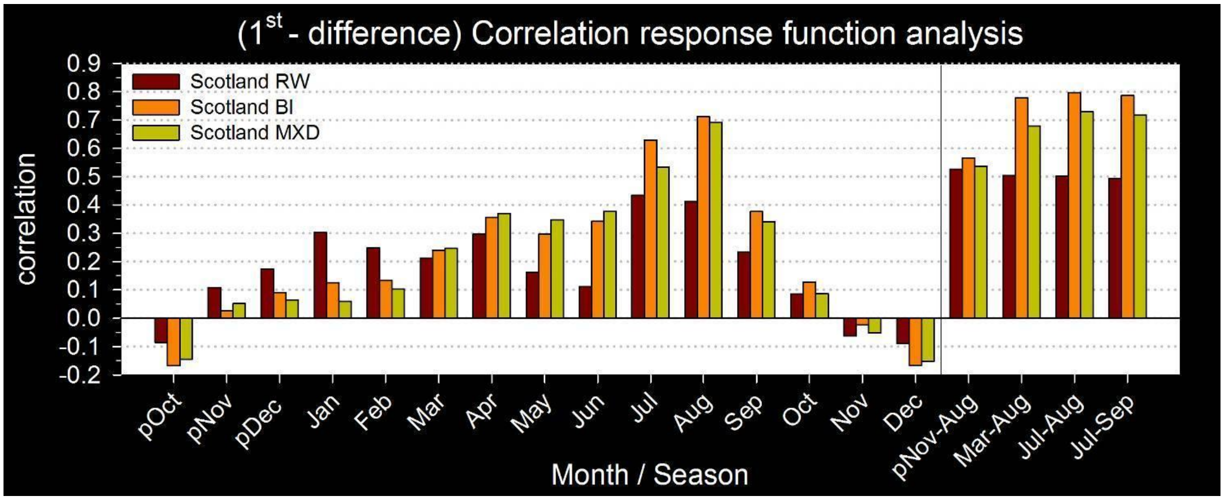
assumption of the "principle of limiting factors", which guides the production of dendroclimatological knowledge from work in the field site and suggests that only the scarcest climatic variable "limits" the growth of trees. Dendroclimatologists have discovered that some tree species growing in certain locations are not suitable for climate reconstructions because they have a "mixed signal" and it is difficult to distinguish if their growth is dependent on either temperature or precipitation. A few decades ago, dendroclimatologists thought it was impossible to carry out dendroclimatology in the British Isles because trees, particularly oak trees, showed a mixed climate signal.[225] This vision changed when Malcolm Hughes published the first Scottish reconstruction, demonstrating that the growth of Scots pine in Scotland at high elevations is dependent on summer temperatures and that this climate signal could be used for reconstructing past climates.[226] Hughes' reconstruction was the result of calibrating tree-ring data against July and August temperature data.

As part of their attempt to update and extend Hughes' reconstruction, Miloš and Rob also employ temperature as the climate variable for reconstruction in Scotland, but struggle to understand the underdetermined selection of nature regarding the months of the reconstruction. They face a difficulty in that each tree-ring parameter (ring width, blue intensity and density) "responds" differently to temperature. To illustrate this point in a conference presentation, Miloš creates a graph that shows the different "response" of each tree-ring parameter to temperature data (image 26).Miloš points to the disparity between parameters: ring width data correlate more or less uniformly throughout the year, whilst blue intensity and density correlations are distinctly higher in July and August. Miloš rationalises this result in terms of the different physiological basis of tree-ring parameters. He tells me that ring width data are based on the cell growth of trees that can be triggered by favourable conditions throughout the year, whereas blue intensity and density data are an expression of cell thickness and lignin content that are particularly related to warm summer temperatures. To choose from nature's selection, Rob recommends that Miloš employ the same months that Hughes employed in his reconstruction. Rob says, "For the sake of coherence with Hughes' work I think it's better if we go with July-August".

[225] Keith Briffa, *Tree–Climate Relationships and Dendroclimatological Reconstruction in the British Isles*, PhD thesis, 1984; Pilcher and Baillie, "Six modern oak chronologies from Ireland" and "Eight modern oak chronologies from England and Scotland", *Tree-Ring Bulletin*, 1980, 40.

[226] Hughes et al., "July–August temperature at Edinburgh".

Image 26. Miloš creates this graph to show the diverse response of trees (each parameter in different colours) to monthly temperature data and the under determination of nature's selection.



Source: Rydval, et al. "Spatiotemporal Reconstruction of Scottish Summer Temperatures". May 2014, TRACE conference.

Once Rob and Miloš have "helped" nature to make a conclusive selection of the months of the reconstruction, natural selection also plays a crucial role in reducing the number of tree-ring datasets needed for the reconstruction. The method of linear regression that Rob and Miloš employ to generate the reconstructed temperature requires working with *averaged* series of data. The response and transfer functions compare *one* monthly temperature series against *one* tree-ring series or chronology.

With regards to temperature data, Miloš and Rob rely on averaged monthly data recorded and curated by scientists working at the United Kingdom's national weather service (Met Office) and one of the main research centres on climate in the UK (the Climatic Research Unit). Rob personally knows Phil Jones, one of the main scientists in charge of these datasets.[227] Jones and a few other scientists have curated an extraordinary 214-year long record of average monthly temperature data series for Scotland. Rob and Miloš are very pleased to have the second longest series of monthly temperature data in the UK, starting in 1700, at their disposal. The longer the

[227] Phil Jones is also the scientist from the Climatic Research Unit whose emails were stolen during Climategate.

instrumental data, the more evidence Rob and Miloš have for the similarity between temperature and tree growth, and the more certain they are of their extrapolations for the period outside the calibration and verification periods.

Jones and colleagues are also in charge of a "gridded" monthly temperature series that Rob and Miloš employ for the spatial reconstruction. A grid is a two-dimensional measurement of the Earth's surface expressed in longitude and latitude. The gridded temperature datasets consists of a series of monthly temperature data for each of the grids of the Earth's surface, including Scotland. Most of these grids do not have direct data from meteorological stations; instead, Jones and colleagues use a technique called "interpolation" to infer data for the "empty grids" between (hence "inter") two grids with observed data.[228] The gridded data for Scotland comes from temperature data recorded at six locations in mainland Scotland and the islands (Stornoway, Edinburgh, Kirkwall, Braemar, Dumfries and Paisley).

Given that interpolation is based on distant station data, Jones and colleagues acknowledge in their publications the potential problems with "representativeness" of the interpolated data, and have developed "diagnostics" associated with each gridded value.[229] Before Miloš attempts to create the spatial reconstruction, I ask him one of my breaching questions via email: "[H]ow confident are you that the interpolated temperature values for the empty grids in Scotland are the 'real' unknown temperature data?" Miloš admits that there could be some problems with the gridded data, especially for the grids in the mainland part of the West of the Scottish Highlands, where the nearest station is at Stornoway in the Western Isles. He finishes his email and the conversation by saying, "I won't go into more detail as there is plenty of literature that goes into the limitations of interpolation".

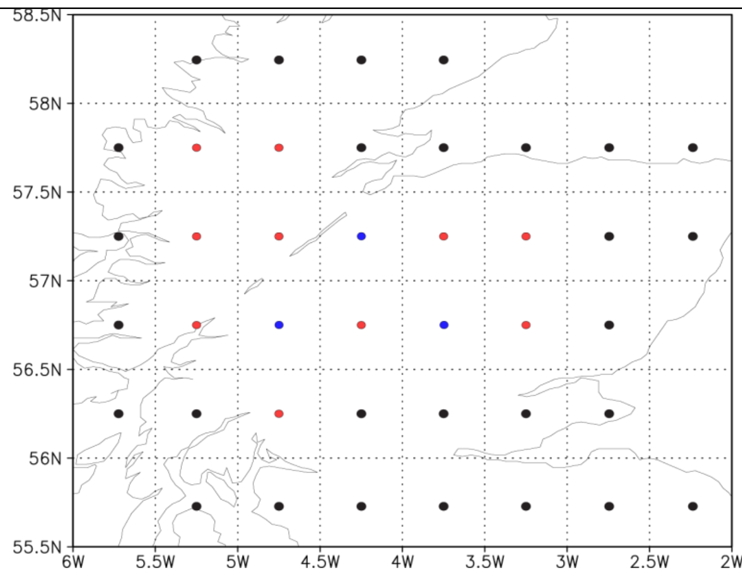
Whilst Rob and Miloš do not worry about creating averaged temperature datasets because other experts do that for them, they are concerned about how to reduce the tree-ring dataset and, again, delegate the responsibility of "screening" tree-ring data to nature. Rob and Miloš employ a standard method of data reduction called "Principal Component Analysis", which produces a sub-set of chronologies or "principal components" that correlate most strongly against all the other

[228] Historically, meteorologists have used interpolation to create global gridded datasets of climatic data. For a sociological account of the creation of these datasets read Paul Edwards, *A Vast Machine: Computer Models, Climate Data, and the Politics of Global Warming*, (Cambridge, Mass.; London : MIT Press;2010).
[229] I. Harris et al., "Updated high-resolution grids of monthly climatic observations – the CRU TS3.10 Dataset", *International Journal of Climatology*, 2014, Vol. 34 (3), pp. 623–642.

chronologies.[230] The result of this analysis is a tree-ring chronology that Rob and Miloš assume contains the strongest climate signal out of all the chronologies, and therefore, should correlate most strongly against the temperature data.

The procedure that Miloš follows to reduce the number of chronologies for the spatial reconstruction is particularly cumbersome; the Principal Component Analysis is done 76 times, for each of the grids into which Miloš has divided the map of Scotland (see image 27). To reconstruct climate spatially, Miloš divides a map of the territory of Scotland into 0.5X 0.5 grids (which in Central Scotland equates to an approximate distance of 30 km North/West); he has to make sure that each grid contains tree-ring and meteorological data. The temporal succession of maps of reconstructed temperature results from the linear regressions between temperature and tree-ring data performed at the level of each individual grid. Miloš employs the gridded temperature dataset that Jones and colleagues have created for Scotland. However, the patchy distribution of Scots pine woodlands in Scotland means that most of the 48 grids have no tree-ring data.

Image 27. Miloš creates this gridded map of mainland Scotland to show the geographical distribution of tree-ring chronologies (red dots) and empty grids (black and blue dots) that he fills in with "local" chronologies selected by nature.



Source: Rydval, "Dendroclimatic reconstruction of late Holocene summer temperatures in the Scottish Highlands", Presentation at the postgraduate Conference St Andrews University, Geography Department, May 2013.

[230] My own understanding of how PCA works is very superficial. PCA essentially ranks the tree-ring chronologies in terms of their individual ability to account for the variance among all the chronologies. The key aspect of this method is the transformation of correlated variables (different tree-ring chronologies) into uncorrelated variables called "eigenvectors".

To fill in the "empty" grids with tree-ring data in the spatial reconstruction, Miloš employs computer software that identifies the nearby tree-ring chronologies with the strongest correlation against gridded temperature data. This procedure of natural selection is very similar to Principal Component Analysis. The software that Miloš uses to fill in the empty grids is called "Point-by-Point Regression", developed by Edward Cook to reconstruct past precipitation and drought patterns in the United States and elsewhere.[231] Miloš learnt how to use Point-by-Point Regression from Cook when he visited the Lamont Laboratory in August 2012. Since then, Miloš has been in contact by email with Paul Krusič , one of the persons who helped Cook to develop he software.

In his paper, Cook explains that he developed the Point-by-Point Regression method as an alternative to another method of natural selection of tree-ring chronologies. This other method, called "Canonical or Orthogonal Spatial Regression Technique" and created by the dendroclimatologist Keith Briffa, selects the tree chronologies that have the highest correlation against the climate grid regardless of the distance between the two grids. Cook's method instead includes a "search radius" that identifies "local" chronologies near the empty grids. When I ask Rob via email to clarify why he and Miloš use Cook's software rather than the alternative method, Rob criticises the latter because "The rationale is that the statistical relationship (even if using a precipitation variable) reflects some sort of real climate teleconnection. I don't believe it for a second."

Rob and Miloš prefer Cook's method because it allows them to constrain the limits of nature's selection on the basis of their judgement of what constitutes a "local" tree-ring chronology. After a few tests, Miloš and Rob agree on the main conditions they will need to accept a correlation coefficient between a chronology and gridded temperature data as adequate (based on a minimum of chronologies found within a maximum distance of 30 km as a search radius).

Once Rob and Miloš have reduced the number of tree-ring chronologies, they must decide whether and how to generate a single reconstruction. They face one specific difficulty associated with the ensemble approach they decided to employ at the stage of standardisation (chapter 5). The ensemble approach consists of using multiple standardised methods in order to generate multiple chronology variants. After Rob and Miloš have reduced the number of standardised chronologies for each variant, they have a total of seven versions of the same chronology.[232] For a long time,

[231] Cook, Edward R.; Meko, David M ; Stahle, David W.; Cleaveland, Malcolm K.; Cook, "Drought Reconstructions for the Continental United States", *Journal of Climate*, 1999, Vol.12 (4), pp.1145-1163.

[232] The standardisation variants are: blue intensity chronologies (the measurements are first inverted because the original blue intensity measurements cannot adopt positive values) detrended with negative exponential

Rob and Miloš ponder whether they should generate seven different versions of a temperature reconstruction or if they should generate a single one. In the case of the spatial reconstructions, this would mean generating seven reconstructions for each of the 48 grids, and ultimately, seven series of maps of the evolution of climate in Scotland.

For the spatial reconstruction, Miloš and Rob finally agree to test different combinations of tree-ring parameters and their associated standardisation nature and let nature decide which one of these combinations is more successful at replicating temperature data. Miloš starts with ring-width chronologies originally detrended with the classic negative curve and later with Signal Free. Then he adds Blue Intensity chronologies alone, and then combines them with ring-width data into band-pass chronologies. Miloš presents this strategy in a postgraduate conference in Aviemore that Rob organises in May 2014. In this talk, Miloš shows a succession of maps in which the calibration and verification statistics for each of the grids progressively improve as more tree-ring parameters and their associated age detrending techniques are added (see image 27). This improvement is expressed through a numerical scale of colours. Miloš and Rob uphold a clear criterion for a "failure" of a reconstruction. If some grids of the reconstruction fall below zero or are in colour grey in the reconstruction map, they interpret that the reconstructed temperature value cannot be accepted as accurate.

On the basis of these criteria, Miloš reports that the first succession of maps, generated with chronologies that have been standardised with the "classic" negative curve, fails, as indicated by the light and dark blue colours (image 28a). Miloš expected this result beforehand because he had not removed the effect of human disturbance and logging from these chronologies. He reports that the "best" spatial reconstruction is achieved with standardised chronologies that are the result of the band-pass approach (combination of ring width and blue intensity), CST and Signal Free. Miloš is happy to find out that the best calibration and verification statistics are obtained in the Cairngorms region, from where they are developing the extended reconstruction. Miloš regards this result as "encouraging" because it is a complementary source of evidence for the value of the long reconstruction (see the next section). Miloš also reports that in this "best" reconstruction, the calibration and verification statistics in some grids in the North-West of Scotland are lower than zero (image 28d). Miloš interprets this

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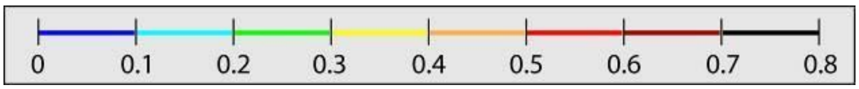
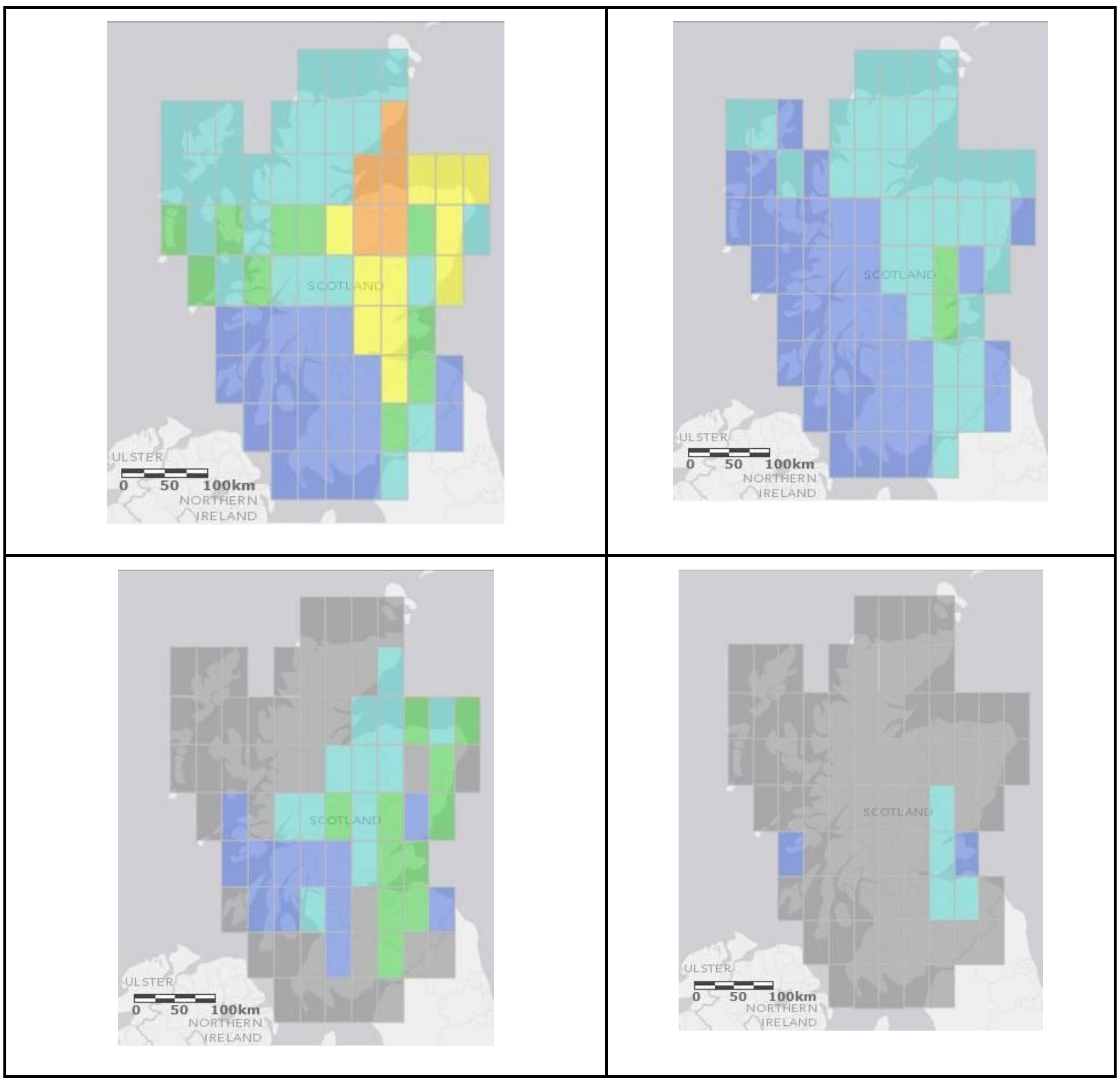
and Hugershoff curves separately; long ring width chronologies from the Cairngorms detrended with RCS and the rest of the ring width chronologies from the West of Scotland detrended with Signal Free; ring width chronologies before and after being corrected with CCT; and the combination of blue intensity and corrected ring width chronologies into the "band-pass chronologies".

result as an indication that the reconstructed values for this area cannot be accepted as reliable and truthful.

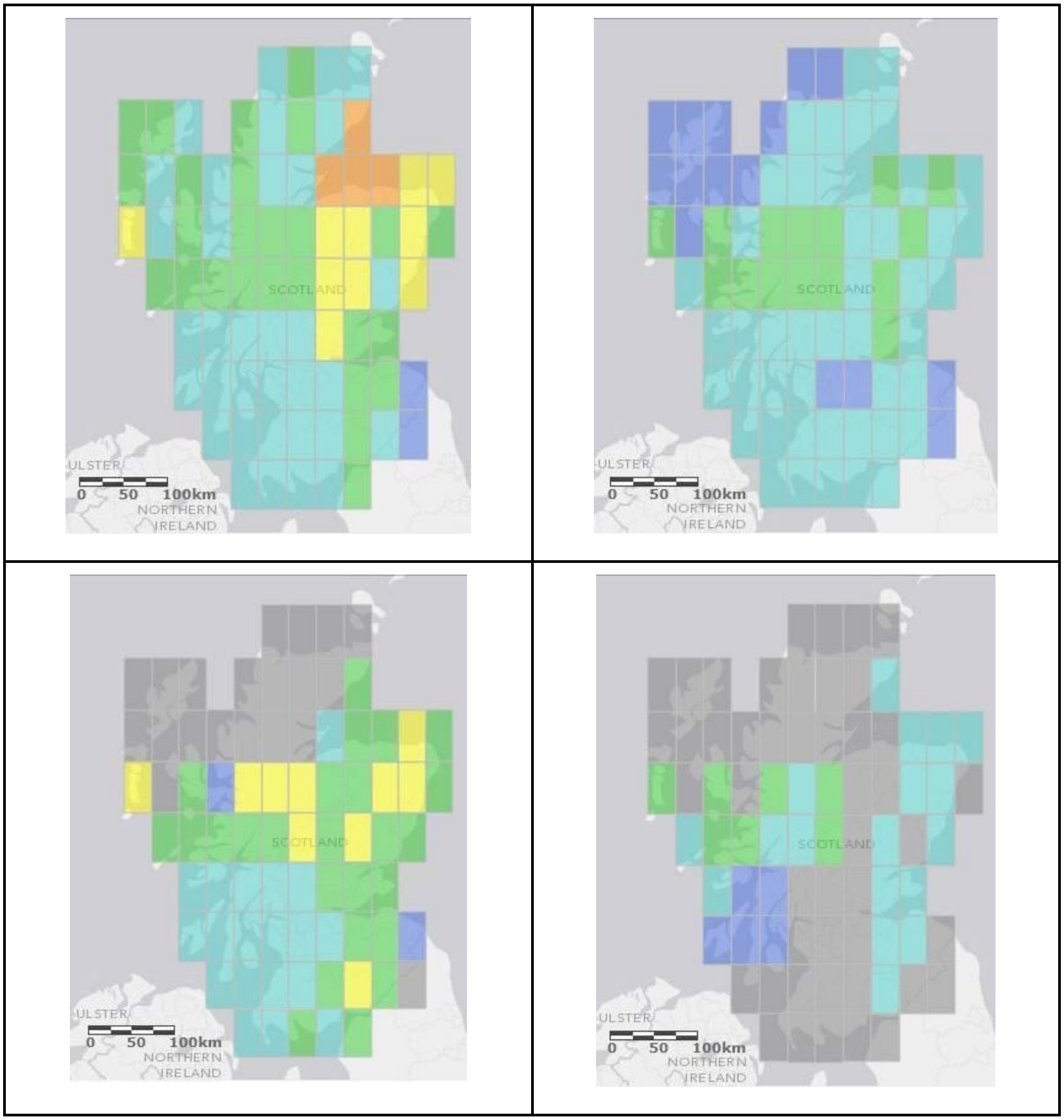
Image 28. In his presentation at the TRACE conference as well as in his doctoral thesis, Miloš presents the succession of four series of maps of the Scottish Highlands (a, b, c and d) to convince conference attendees and readers of the thesis of the need to combine tree-ring parameters and associated standardisation methods to achieve an increasingly better natural selection. The resemblance between tree-ring data and gridded temperature is represented by a coloured scale of correlation coefficients (image at the top). Each group of maps represents different examples of trained variation and variants of standardised chronologies.



a) Ring-width data standardised with negative exponential and uncorrected with CCT.



b) Ring-width data standardised with Signal Free and corrected with CCT.



c) An ensemble of ring-width data (corrected with CCT), Blue Intensity and maximum density data detrended with Signal Free.

d) Combination of Ring-width (corrected with CCT) and Blue Intensity data into a single band-pass chronology that has been detrended with Signal Free.

Miloš' discovery of the "failed" results in the West of Scotland renders visible the finite nature of reconstructions and the possibility of redefining existing extrapolations about past climates. This process of revision starts with the identification of the source of error of existing reconstructions, and the potential for a regress regarding the exact reason for the failure.[233] As Rob succinctly describes the dilemma, "If there is disagreement between a reconstruction and instrumental data - which should we blame?" In his textbook, the dendroclimatologist Harold Fritts offers a method to answer this question with a series of flowcharts that guide the dendroclimatologists to former stages in the reconstruction.[234] These charts offer a set of plausible explanations and solutions for situations in which dendroclimatologists find that the calibration and verification statistics have failed. These explanations are: tree-ring chronologies might have not been standardised properly; instrumental records might be biased; the calibration equation might be imperfect, other climatic variables and months of reconstruction might limit the growth of trees. In line with the range of plausible explanations outlined by Fritts, Rob and Miloš explore a series of factors that could explain why the reconstruction failed in the West.

The reasons that Rob and Miloš give to rationalise the anomaly in the West of Scotland, and to foreclose the interpretation of finite reconstructions, relate to the uncertainties that they have previously considered in preceding stages. First, they speculate that the standardised ring width chronologies are not completely clean of disturbance. To resolve this issue, Rob, and Miloš in particular, plan to continue experimenting with CCT in collaboration with Dan. Rob has also agreed with Björn from Sweden to employ the Scottish samples to generate new density chronologies that will reinforce the climate signal from existing ring width and blue intensity chronologies. Second, Rob and Miloš initially explore the possibility that the interpolated gridded data for the North-West of Scotland might be "unrepresentative", in line with known problems with the interpolation method. Miloš conducts some tests and employs an alternative gridded dataset; he concludes that the problem lies with the tree-ring chronologies, rather than the instrumental record. Finally, Rob and Miloš also contemplate that climate variables other than temperature (such as strong winds and higher rates of precipitation) could have an effect on the way trees grow in the Western Highlands. Rob performs some correlation analysis between the tree-ring data and precipitation data, but does not report any significant correlation. Rob and Miloš also suspect that differences in site elevation/latitude could explain the differences observed among regional

[233] Harry Collins calls this phenomenon "the experimenter's regress" in *Changing Order*.
[234] Fritts, *Tree Rings and Climate*, p.315.

reconstructions, but after some experimentation Miloš decides to leave aside this explanation because "a detailed examination of such effects is beyond the scope of my study".

Overall, Miloš and Rob are very pleased to have discovered the anomalies in the West. They regard this discovery as a vindication of their belief that, in order to interpret nature's selection competently, it is necessary to take into account the local growing conditions of trees and potential confounding growth factors such as disturbance. Miloš writes in his thesis chapter about the spatial reconstruction, "On the whole, this exercise highlights the importance of developing an awareness and appreciation of tree growth response within the interplay of often confounding climatic, environmental and ecological factors in order to appropriately assess the suitability of tree-ring data for climate reconstruction. Such intricacies have often been ignored by dendroclimatologists in the past."[235]

Miloš and Rob also see the identification of anomalies as an opportunity for further research by the members of the Scottish Pine Project and others. Rob explains that even "If the West does not work for a climate reconstruction, at least, we still have the East, and for those interested in disturbance and ecology we've proved that there's plenty of work to do in the West". Both Miloš and Rob agree that "making the West work" surpasses the main aim of the Scottish Pine Project and Miloš' PhD, which is essentially about updating and extending Hughes' temperature reconstruction.

Over time, Rob refines a method for creating the extended reconstruction for the Cairngorms that captures the combined strategy of trained variation and natural selection. He calls this method the "Combo Approach", and he presents it for the first time in his Blue Intensity paper (Chapter 4). Essentially, the combo approach consists of creating a single reconstruction from a weighted combination of all the temperature reconstructions that derive from the ensemble approach. More specifically, the procedure starts by ranking each chronology variant in terms of its similarity to temperature data. Rob and Miloš evaluate this similarity with a statistical metric called "root mean square error". The chronology with the smallest error is weighted most heavily in the average of all the reconstructions.

Effectively, the weighting procedure in the combo approach spares Rob and Miloš the decision of which reconstruction variant to accept as more truthful. Instead, the decision on the "best" representation of climate is left to nature. The anonymous reviewer of the Blue Intensity

[235] Miloš Rydval, *Dendroclimatic Reconstruction of Late Holocene Summer Temperatures in the Scottish Highlands*, unpublished thesis, The University of St Andrews, p. 101.

article where Rob presents the combo approach for the first time recognises this feature from the combo approach when he/she says, "For this reviewer this is a new and important concept that encourages experimentation and thinking about a range of reconstruction techniques and not having to pick the 'best' reconstruction".

Rob's decision to develop the combo approach is an attempt to achieve "objective" reconstructions and a response to alternative approaches in the community. Rob explains to me in an email, "In my COMBO games, I derive multiple versions of the reconstructions and an objective way (hopefully) to combine them". Rob is critical of an alternative approach used by his colleague and friend Jan Esper for choosing the "best" reconstruction. When I ask Rob via email to clarify his disagreement with Jan, Rob tells me that in one of his papers, Jan also used the ensemble approach and created multiple reconstructions from multiple standardisation methods. Rob tells me that Esper decided to pick one reconstruction on the basis of his "expert judgement". Rob criticises this stating "There was no statistical reason to choose this over other versions as far as I can tell. Jan feels he knows a 'best' option which I feel we cannot do".

Rob criticises Jan Esper for appealing to his "expert judgement" to select a reconstruction variant, which Rob believes is undetermined and should be left to nature. The first time I hear this criticism is in Rob's talk at the international dendrochronology conference in Australia in January 2014 when he refers to "Esperism". In his talk, Rob admits there are uncertainties in deciding the "best" reconstruction and presents his solution, including the combo approach and the band-pass approach that he and Miloš use to create the spatial reconstruction of Scotland:

> The strategy that I am slowly starting to put together about how to go about this is…[2 second of silence] Well, every record, whether it is a single site record or a regional composite, we need to be very careful in our local calibration and screening. We need to come up with the most robust calibration and verification statistics for a particular region. Divergence we can easily identify empirically. We look carefully at residual analysis and through stringent verification we can say how robust a particular series is. We must use the "best" chronology variant for a particular region. There are several approaches to this. We can use what I call "Esperism" or the "Esper approach" and his terminology of "expert judgement". Jan would say "this is my best record" or whatever he would say. [giggles from the audience]. I would much rather go for the statistical approach. Maybe we can use some sort of fusion using regression-based methods where

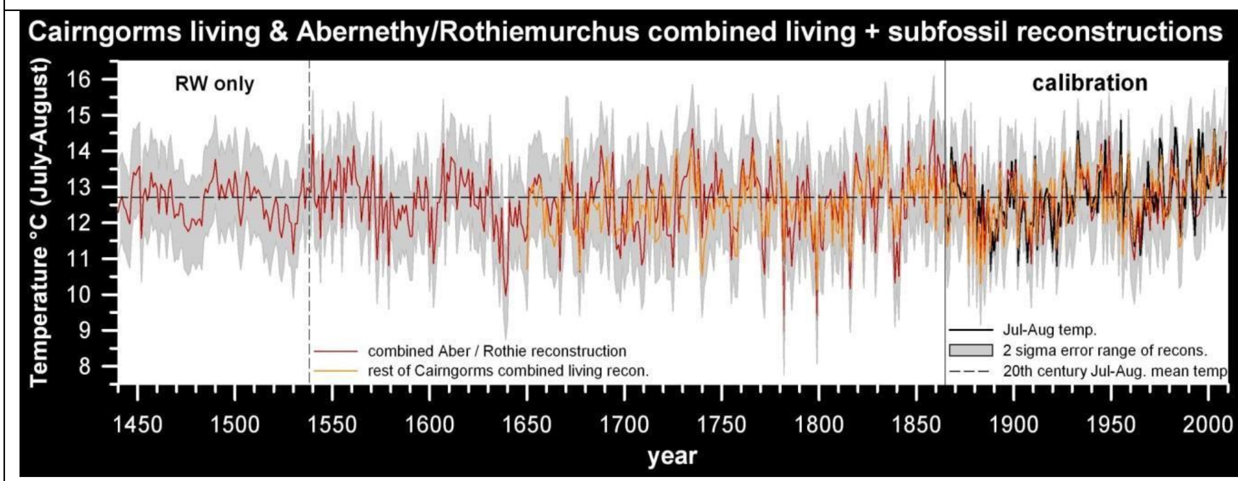
> you can combine ring width and density. We can maybe weight all the different variants as a function of their R2 against the target season, even weakly correlated chronologies can be still included, but they will be weighted very weakly [combo approach]. There are a few groups that are playing around with the band-pass approach; you might want to use the high frequency of density or blue intensity and the low frequency from ring width. [3 seconds of silence] These are all valid methods. Whatever you choose, you want to come up with the best variance that can be rationalised and is well defendable. I always put my Steve McIntyre's cap when I do any analysis: "Can I defend this in a public venue?" "Is this a good choice what we did and do we have good reasons for doing it?"

6. 2. 2. Complementarity

Throughout their reconstruction work, Rob and Miloš search for complementary evidence that demonstrates the consistency of nature's selection. At the conference in Melbourne in January 2014, Miloš refers to this strategy of complementarity when he presents what he calls a "very provisional" reconstruction. At this point, the Cairngorms reconstruction only goes back to 1450 AD. Months later, after cross-dating a few other subfossil and living chronologies, Rob and Miloš are able to extend the reconstruction back to 1200 AD (Chapter 3). The 500-year temperature reconstruction that Miloš presents at the conference in Melbourne is based on the RW/BI band-pass approach that combines blue intensity and ring width data. The graph that Miloš shows in front of an audience of 100 people includes two graph lines or reconstructions (image 29). One is the "Abernethy/Rothiemurchus" reconstruction that Miloš has generated from all the subfossil data and living trees located in one specific area of the Cairngorms (Abernethy and Rothiemurchus). The other reconstruction includes data from all the sites in the Cairngorms except Abernethy and Rothiemurchus.

With the exclusion of the Abernethy and Rothiemurchus subset from the more general Cairngorms dataset, Miloš and Rob seek to create an "independent" reconstruction against which to evaluate the other one. They expect that the two separate reconstructions will agree considerably well with each other (as they are both from the same area in the East of Scotland) and against monthly temperature data for July and August. At the conference, Miloš presents the results of this comparison as evidence that the reconstruction is “a first step towards developing an extended robust summer temperature reconstruction for Scotland.”

Image 29. Miloš presents the graph and the table below at a conference as an independent confirmation of the reliability of the long temperature reconstruction for Scotland. The graph uses the long subfossil chronology of the Abernethy/Rothiemurchus area (red line) as “internal control” for the “Rest of the Cairngorms” reconstruction (orange line). The black line is the temperature dataset against which these two reconstructions are calibrated. The table below quantifies the similarity between reconstructions and against temperature data with correlation coefficients (r2) and an associated statistic (Durbin-Watson). The graph includes error bars or estimates of the error of the reconstruction in grey.



Abernethy-Rothiemurchus vs. Jul-Aug temp.	**r^2**	**DW**
1866-2009	51.9%	1.485
Rest of Cairngorms vs. Jul-Aug temp.		
1866-2009	59.1%	1.407
Aber.-Rothie. vs. Rest of Cairngorms		
1866-2009	75.7%	
1650-2009	50.9%	

Source: Rydval et al. “A Preliminary 600-Year Summer temperature Reconstruction for the Scottish Highlands”, Conference presentation, Melbourne January 2014.

In the question and answer (Q&A) session following his presentation, Miloš receives a question from Malcolm Hughes, the author of the first Scottish reconstruction. Rob has already warned Miloš of this possibility: “I am sure that Malcolm will ask you about his baby”. The conversation that Hughes initiates with Miloš eventually involves other members of the audience. Hughes’ question revolves around what he calls “the problem of multiplicity”, which essentially refers to the risk of “spurious correlations” and the possibility that natural selection does not in fact reflect any real relationship in the natural world. The conversation goes as follows:

> Hughes: A comment as Hughes et al. [referring to Hughes’ paper where the first Scottish reconstruction was published].
>
> Miloš: [laughs]
>
> Hughes: I have a general question that it is not aimed exclusively at your presentation. It’s something we all get involved in. We try all these different variables; we change this step of the process and that step, and so on. And it’s probably the kind of thing that we kindly don’t mention to our neighbours statisticians.
>
> Miloš: [laughs]
>
> Hughes: … because it will be bad for their blood pressure.
>
> [General laughs]
>
> Hughes: Have you got any thoughts about how to deal with that problem of [2 -3 seconds silence] potential self-delusion associated with multiplicity?
>
> Miloš: [laughs nervously].
>
> [2 seconds silence]
>
> Miloš: Um, if I understand your question correctly I would say that really [2 seconds silence] including many variants and combinations is necessary. It’s not possible to derive a single result, an ultimate definitive answer. In that sense, it might be difficult to choose a different approach to develop a reconstruction and so on. [3 seconds silence]. I am not sure if I’ve answered your question…
>
> Hughes: Can I come back and say that you have come the nearest to answer it by actually bringing on independent datasets and seeing the same pattern...
>
> Miloš: Yes
>
> Hughes: It seems to me that to the extent that they are really independent datasets, that’s the way to cut through these issues

Miloš: I think so. That's definitely true. You know… Here I have showed that using two different datasets you can achieve more or less the same results or very similar. Yeah, I agree that this is one way, one way, to solve this problem.

[5 seconds silence]

Moderator: any other questions?

[A third person in the audience stands up and says]

Attendee: Well, if it was me, if I was talking to a statistician, I'd tell him to go and have a look at the tree physiology library, and to read through all the hundreds and hundreds of studies that relate climate to tree growth. You might get the question [from statisticians] of "how many observations do you have?" You might answer "I don't know". But, you know… It gets a little bit harder when, you know, you get six hundred observations to actually kind of go out of the line.

Miloš: Well, yes, what I would say here is that if you want to be sure that the data are showing something real, a real representation of what the climate is showing, it is necessary also to compare it to other regions or areas. In this case, I don't think it is so much of a problem because the instrumental record in the UK or Scotland is quite long and it has been looked at in quite a lot of detail in terms of the quality and so. There are different approaches towards how you can validate the data that you've generated and the reconstructions that are coming out of this.

Moderator: Thanks very much. Hopefully, this is the sort of conversation that we can continue over a beer in just one more session.

As Miloš explicitly says above, the strategy that he and Rob have adopted in Scotland to prevent the potential for "self-delusion" in assuming that the reconstruction is a real representation of past climates is to search for complementary sources of evidence.

One of the sources of evidence that Rob and Miloš employ is qualitative. They use historical evidence for the Scottish climate. In his book *So Foul and Fair a Day: a History of Scotland's Weather and Climate*[236], the Scottish geographer Alistair Dawson provides a list of the five coldest and warmest years in Scotland on the basis of historical documents. In a talk that Rob is preparing to give in a meeting with other Scottish geographers, he notes a correlation

[236] Dawson, *So Foul and Fair a Day*.

between the "extreme years" identified by Dawson and some of the temperature peaks estimated in the reconstruction. Rob communicates by email this finding to Miloš, who responds, "I've also noticed that there's pretty good agreement between some of the reconstructed temperature periods and Ali Dawson's book - very encouraging".

The other source of complementary evidence that Rob and Miloš employ is quantitative, and it includes multiple temperature datasets and reconstructions (images 30 and 31). One crucial complementary source is the long 214-year record of temperature records for Scotland that Phil Jones and colleagues have curated. In the calibration and validation comparison, Rob and Miloš discover an anomalous divergence between tree-ring data and temperature data in the early 19th century. Rob had already some suspicions that the temperature records were not reliable during the first part of the 19th century and he later on discovers that Jones had written in one of his papers that the temperature measurements from 1800 to 1866 might be inflated because the weather stations of the time were not properly insulated from the sun.[237]

In an email to Jones, Rob explains how the identified warming bias in the temperature dataset for Scotland allows him to explain the observed divergence. Rob writes "Your paper is perfect as we actually have found a misfit between the TR [ring-width] based reconstructions (too cold) and the instrumental data prior to about 1857 and your paper says that there are homogeneity problems prior to 1866. This is great as it was a real worry for us". Rob reports to Miloš the discovery of the warming bias in the temperature series as "great news for us". Rob sees the anomaly in the temperature dataset as "good news" because "it suggests that the divergent trends we get in the calibration period are not due to problems with the tree-ring data".

Other quantitative forms of evidence against which Miloš compares the Scottish reconstruction in his thesis include the longest temperature record in the world, which corresponds to Central England (CET); the first Scottish reconstruction from Hughes et al 1984; the first ever semi-quantitative climate reconstruction of Central England created by Hubert Lamb in the 1960s; a Central European reconstruction (Luterbacher); an Alpine reconstruction (Büntgen), a Northern Scandinavia reconstruction (Esper); a Central Scandinavia reconstruction (Jämtland) and a Pyrenees reconstruction (Dorado Linan).

[237] Phil Jones and Lister, "The development of monthly temperature series for Scotland and Northern Ireland", *International Journal of Climatology*, 2004, Vol. 24, (5), pp.569-590

Image 30. In his thesis, Miloš uses the graphs below to compare the Scottish Cairngorms reconstruction against other forms of quantitative evidence such as observed temperatures in England and reconstructed temperatures for the UK (represented by a red line graph).

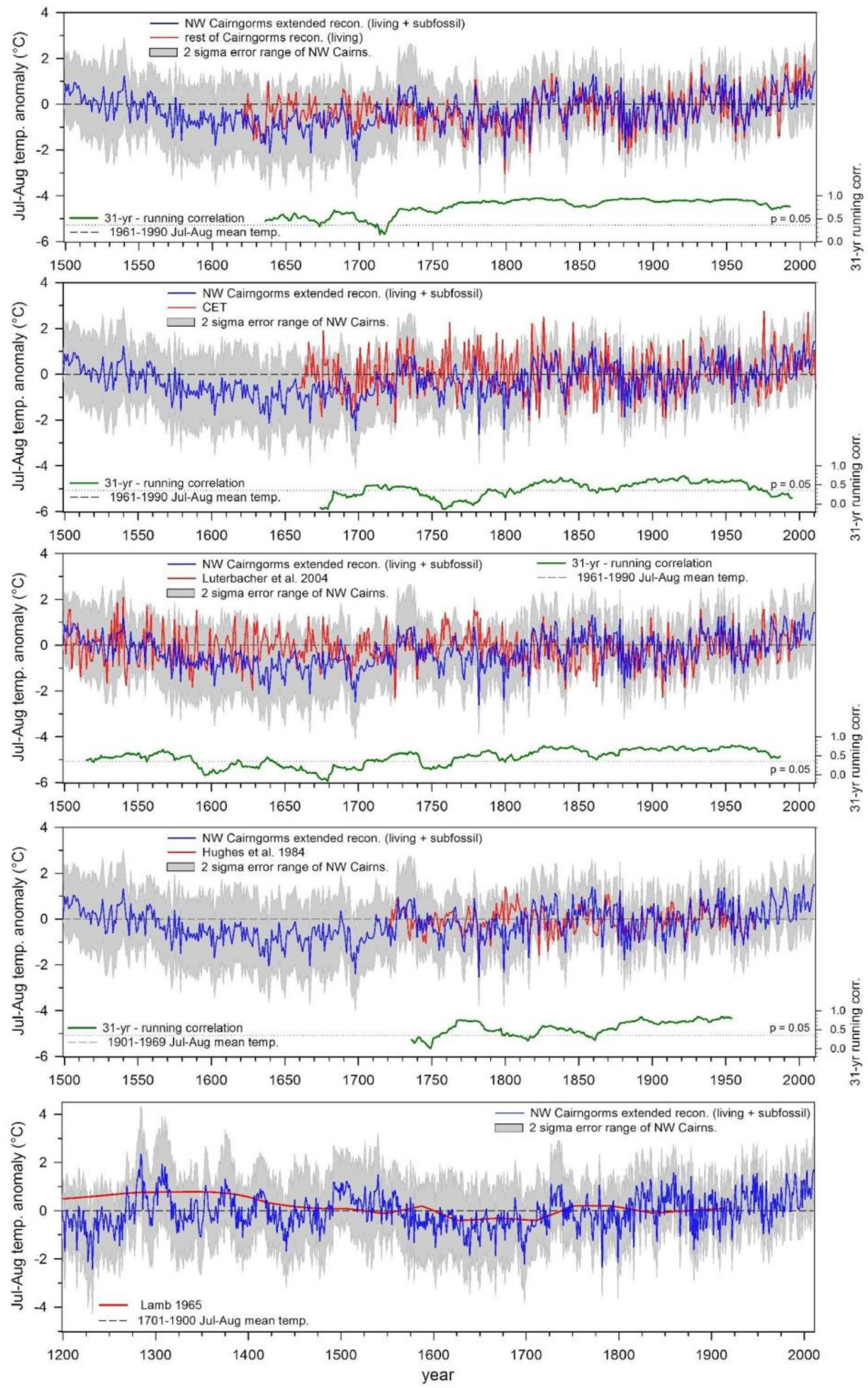
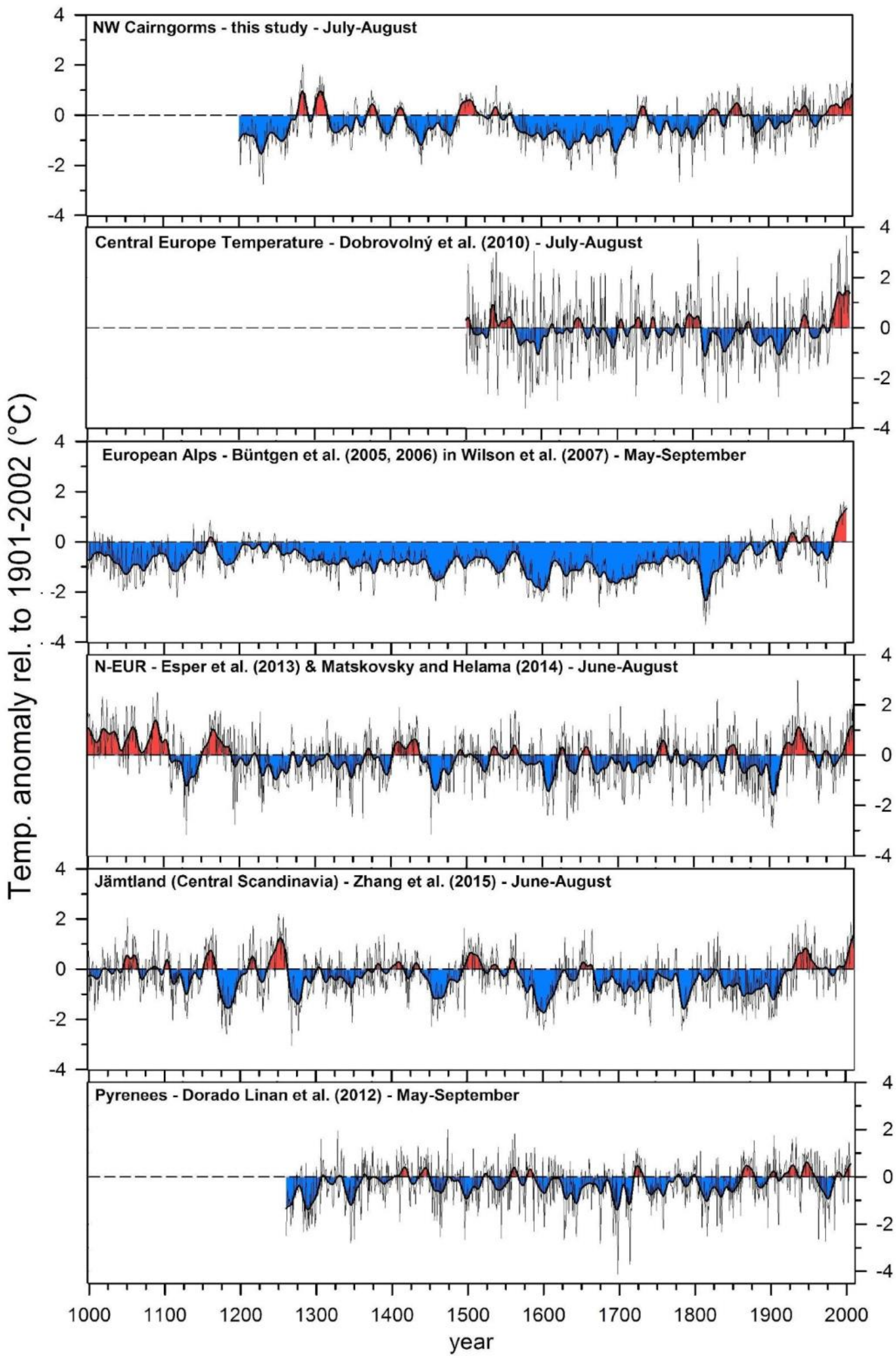


Image 31. In his thesis, Miloš includes the graphs below to compare the Scottish Cairngorms reconstruction (at the top) against the reconstructed temperature anomalies (relative cooling in blue and relative warming in red estimated on the basis of a reference period) to other European reconstructions.



The last source of quantitative evidence that Rob and Miloš employ is a “default” complement to the reconstruction. The “error bars” or confidence intervals measure the uncertainty associated with each reconstructed temperature value. The graphical representation of estimates of error with grey bars has become standard in climate reconstructions after the publication of the famous “hockey-stick” temperature reconstruction in 1998 (Chapter 7)[238]. Rob and Miloš think that the estimates of error for the Scottish reconstruction are conservative and under-estimate the likely error. In one email that Rob sends me, he lists some of the uncertainties, or what he calls “known unknowns”, that the error bars do not capture (they include a regression error associated with the use of linear regression; a sampling error related to the weakening of the climatic signal as a chronology extends back in time; and the remaining effect of disturbance onto some chronologies in Scotland).

When I ask Rob why he uses error bars if he knows that they are not completely accurate, Rob says that error bars are a “guide” and that he is exploring alternative methods. In particular, Rob refers to “Bayesian statistics” as a method to achieve “the ‘true’ uncertainty”. Rob specifically mentions the name of one scientist, Martin Tingley, whom Rob had just met in a workshop in the US “as the first Bayesian statistician who balances theory with reality”. However, Rob explains that “As very few people are playing with such approaches, we must work with non-ideal but still useful estimate of uncertainties”.

In the last months before Miloš submits his thesis, Rob and Miloš employ the “non-ideal” error estimates to create a more accurate reconstruction. As part of their attempt to provide a double quality check, Rob and Miloš have agreed that they will replicate each other’s reconstruction. The result of this replication is that Rob’s reconstruction has smaller error bars than that of Miloš, which they interpret as evidence that Rob’s reconstruction offers a more accurate representation of past climates. They both agree that Miloš needs to “constrain” the error bars of his reconstruction and redo some of the analysis in order to create a reconstruction that resembles Rob’s.

6. 3. Discussion

The two strategies of “trained variation and natural selection” and “complementarity” that Rob and Miloš employ to resolve the potential uncertainty of extrapolations about past climates are essentially examples of civil and organised scepticism.

[238] Mann, Bradley and Hughes. "Northern hemisphere temperatures during the past millennium”.

The "combo approach" and the strategy of letting nature perform the selection of the final reconstruction involves an explicit scepticism towards an alternative approach in the community that Rob calls "Esperism", which involves the use of expert judgement in deciding between the different versions of the reconstruction. Likewise, the strategy of employing complementary evidence against which to compare the Scottish reconstruction represents sceptical tests about the reliability of nature's selection. Once nature has made the selection of the best reconstruction, Rob employs his expert knowledge and that of other scientists to examine this selection sceptically. Overall, the sceptical strategies of trained variation and natural selection and complementarity are parasitic upon specific forms of trust relationships. These networks of trust are ultimately responsible for the closure of the extrapolations and the interpretation of climate reconstructions.

Regarding the strategy of trained variation and natural selection, trust relations are involved in the process of constraining the choices of nature in many different ways. Miloš' acceptance of Rob's recommendation to select July and August as the months of the reconstruction "for the sake of coherency" is not only an act of trust in his supervisor but also of trust in Hughes' work. The trust that Rob and Miloš place on generations of anonymous scientists who have developed the Principal Component Analysis method as a fairly established method of data-reduction is crucial for the work of reconstruction. Similarly, Rob and Miloš' trust of Edward Cook and his method of spatial reconstruction is the basis upon which they are able to practice a specific form of natural selection, which is based on the screening of local chronologies rather than purely statistically-based selections as represented by Briffa's method that Rob distrusts

Regarding the strategy of complementarity, Rob and Miloš trust the evidence (both qualitative and quantitative) put forward by the Scottish geographer and historian about the years with the most extreme weather and by generations of dendroclimatologists and paleoclimatologists who have been involved in the work of creating the longest temperature dataset in the world and the UK and European reconstructions. Rob also has the expectation that the Bayesian statistician he met in the US could improve the ability of "realistic" error bars as another form of complementary evidence. Rob and Miloš use the "non-ideal but still useful estimate of uncertainties" and suspend their scepticism about the "known unknowns" that are not captured in the error bars. The act of suspending scepticism about error bars allows Rob and Miloš' mutual sceptical replications of the reconstructions, and to produce a more accurate reconstruction with a more "constrained" error range.

More generally, collective suspensions of scepticism, which are themselves dependent on existing trust relations, have had a crucial role in foreclosing the reinterpretation of finitist reconstructions. The rationalisations that Rob and Miloš employ to explain the anomalies of the reconstruction are instances of scepticism that they have temporarily suspended at previous stages of the reconstruction. This is the case with the CCT chronologies, which Rob and Miloš already suspected were not properly cleaned representations of climate (Chapter 5). Rob and Miloš hope to be able to use their already existing trust relations with Dan to explore and resolve the limitations of CCT in the future. This is also the case with both temperature datasets. The trust that Rob and Miloš place on Phil Jones and experts of temperature datasets is crucial for their interpretation of the anomalies in the reconstruction. Through Jones' published articles, Rob and Miloš know of the warming bias in the early part of the 214-year long temperature record for Scotland and the limitations of interpolations in the West of Scotland. Rob and Miloš believe that these datasets, whilst being limited in many aspects, offer the best approximation of climate and trust that the relevant experts will continue doing sceptical work with them.

The second instance in this thesis where collective suspensions of disbelief and fictions play a role relates to uniformitarianism. All dendroclimatologists and paleoclimatologists involved in the sceptical work of reconstructing past climates share the supposition that the relationship between climate and tree growth is stable over time. The existence of uniformitarianism as a "principle" is evidence of the existence of a fiduciary framework that all dendroclimatologists employ to develop their reconstructions. For dendroclimatologists, the discovery of consistent cases of divergence between tree-ring data and temperature data in the present period has shown a few limitations of the fiduciary framework, but in most cases, uniformitarianism still applies. By using uniformitarianism "As-If" it was true, dendroclimatologists are able to start developing their extrapolations (or in Jim Speer's words in his textbook, the assumption of uniformitarianism is a "productive starting point" for dendroclimatology). The constitution of the "divergence problem" as a distinct research problem indicates that, whilst Rob suspends his disbelief about uniformitarianism in the immediate work of reconstructing climate about Scotland, he and his community are collectively addressing the limitations of uniformitarianism through different exercises of civil scepticism as in the "divergence session" led by Rob at the Melbourne conference.

As an attempt to pre-empt outsiders' likely future criticisms about the accuracy of tree-ring based climate reconstructions, Rob and Hughes display their scepticism in front of colleagues at the conference in Melbourne. By showing awareness and offering solutions to resolve the potential limitations of existing reconstructions outlined by outsiders, Rob and Hughes also demonstrate their competence to colleagues. On the one hand, Rob emphasises the under determinacy of reconstructions and offers trained variation and natural selection as solutions. On the other hand, Hughes formulates the potential disillusion of accepting natural selection as accurate and offers complementarity as a solution. Both Rob and Hughes display their scepticism rhetorically, as a form of scepticism-as-an-account, by invoking outsiders (McIntyre and statisticians). Rob says he is wearing "the McIntyre cap" and Hughes asks a devil's advocate type of question as if it had been formulated by statisticians. These two cases of sceptical display are aimed at reinforcing trust from dendroclimatology colleagues as well as pre-empting the scepticism hypothetically expressed by outsiders about the work of the community of dendroclimatology. The next chapter about a "controversy" details a case in which dendroclimatologists dealt with what they perceived to be uncivil scepticism from an individual who prior to the controversy was considered to be more or less a member of their community.

7 Controversy

7.1 The Sceptical Challenge to Dendroclimatology

Rob and Miloš are producing their temperature reconstructions for Scotland against the background of a controversy in dendroclimatology. This scientific discussion originates when Michael E. Mann, Jose D. Fuentes and Scott D. Rutherford ("Mann et al. (2012)" hereafter) publish an article in February 2012 presenting their "missing-ring hypothesis".[239] Throughout the whole period of my research, Rob and Miloš refer implicitly to this controversy by naming, often sarcastically, the lead author. For instance, while sampling a lake during my first fieldwork expedition, Rob tells me, "You see this is the difference between people like Mann who sit at a desk and use archived tree-ring data to formulate a stupid hypothesis about missing rings, and those like us who create the data at the site". When Rob makes this comment, I am in the early stages of my research and do not know who Michael Mann is. I ask Rob if Mann is a dendroclimatologist, and Rob responds, "Even though Mann poses with tree samples in his university website profile, I doubt Mann has ever cored a tree. He is not a dendroclimatologist; he is more of a statistical paleo climatologist".

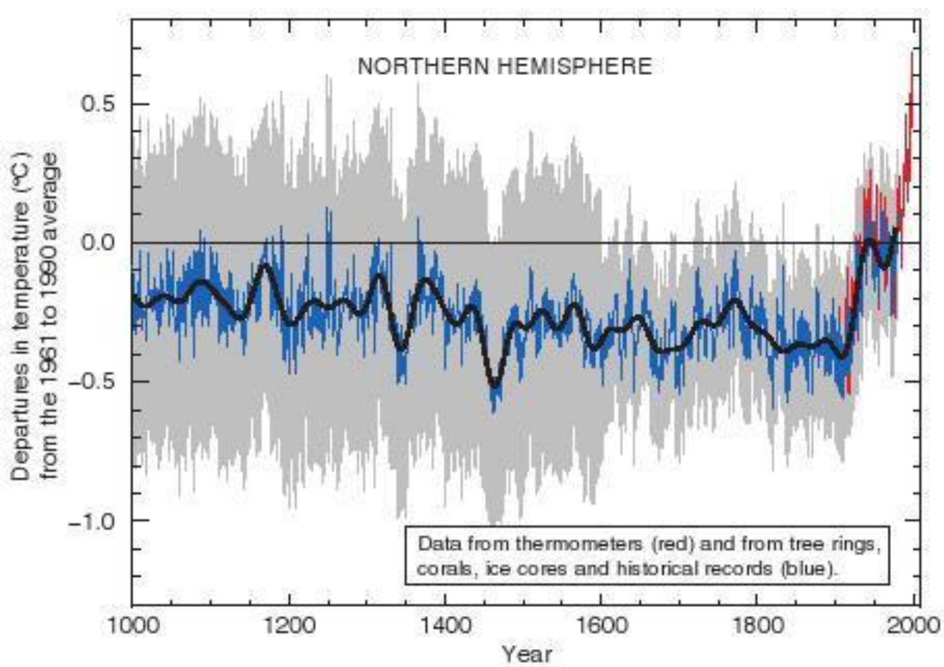
Michael Mann is well-known in dendroclimatology and elsewhere for being an expert in statistical methods applied to paleoclimatology and co-authoring a series of Northern Hemisphere temperature reconstructions in the late 1990s with Raymond Bradley and the dendroclimatologist Malcolm Hughes (who created the first Scottish reconstruction). The basis of their collaboration was to apply one of Mann's statistical techniques to a paleoclimatic dataset developed by Bradley and Hughes, among others. The result of the collaboration between Mann, Bradley and Hughes later became known as the "hockey-stick" reconstruction[240]. With this

[239] Michael Mann, Jose D. Fuentes and Scott Rutherford, "Underestimation of Volcanic Cooling in Tree-Ring Based Reconstructions of Hemispheric Temperatures." *Nature Geoscience*, 2012, Vol. 5 (3), pp. 202-205.

[240] Unfortunately, there are no sociological histories that explain the evolution of the "hockey stick graph" from the "MBH98 reconstruction" (Michael Mann, Raymond Bradley and Malcolm Hughes, "Global-scale temperature patterns and climate forcing over the past six centuries", *Nature*, 1998, Vol. 392 (6678), pp. 779–787) through the "MBH99 reconstruction" (Michael Mann, Raymond S. Bradley and Malcolm K. Hughes, "Northern Hemisphere Temperatures During the Past Millennium: Inferences, Uncertainties,

graph, the co-authors reported that the Earth's temperature decreases until 1900, after which it increases sharply, like the upturned blade of a hockey stick. In 2001, the hockey-stick graph achieved prominence after the authors of the Intergovernmental Panel on Climate Change's (IPCC) Third Assessment Report - Mann being one of them - included it in the Summary for Policymakers as evidence of 20th century global warming (image 32).

Image 32. The "hockey stick" graph or temperature reconstruction for the Northern Hemisphere (as featured in the IPCC report) created by Mann, Bradley and Hughes triggered political and scientific debates.



Source: IPCC Third Assessment Report, *Climate Change 2001, p.3.*
http://www.grida.no/climate/ipcc_tar/vol4/english/pdf/wg1spm.pdf (accessed 8th April 2008).

The statistical procedure that Mann employed to create the hockey stick graph became disputed by scientists and politicians alike - particularly in the US where the political culture is described

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and Limitations." *Geophysical Research Letters*, 1999, Vol. 26 (6), pp. 759-762) that eventually featured in the IPCC report, and the corrigendum that Mann, Bradley and Hughes published in 2004 ("Corrigendum: Global-scale temperature patterns and climate forcing over the past six centuries", *Nature*, 2004, Vol. 430 (6995), p. 105) after the bloggers Steven McIntyre and Ross McKitrick pointed out errors in the MBH98 reconstruction.

as distinctly confrontational[241]- in an episode that Mann and others have dubbed the "Climate Wars".[242] To counter-attack criticism of the hockey stick graph from people like Steven McIntyre who was by that time publishing his blog *Climate Audit*, Mann co-founded the blog *RealClimate* in 2012. Authors and participants in *RealClimate* use this online medium, among other things, to publicise and discuss existing peer-reviewed articles like Mann et al. (2012). On 6 February 2012, one day after the article by Mann et al. (2012) is published online in the journal *Nature Geosciences*, Mann writes a blog entry on *RealClimate*. There, he summarises his article and concludes, "Our study, in this regard, once again only puts forward a hypothesis. It will be up to other researchers, in further work, to assess the validity and potential implications of this hypothesis."[243]

Essentially, the missing-ring hypothesis is an explanation of the supposed biases that Mann et al. (2012) have identified in a Northern Hemisphere temperature reconstruction that Rob co-authored in 2006 with two other dendroclimatologists, Rosanne D'Arrigo and Gordon Jacoby (paleoclimatologists use the nomenclature "DWJ2006" to refer to this reconstruction and so will I). DWJ2006 was the result of Rob's postdoctoral corroboration and start of official adjunct status at the Lamont laboratory where D'Arrigo and Jacoby worked. Until the controversy occurred, DWJ2006 was the only large-scale millennium reconstruction that Rob had published.[244]

The importance of DWJ2006 is that, up until the controversy occurs, it is the latest Northern Hemisphere reconstruction entirely based on tree-ring data, unlike for instance, the hockey stick graph, which uses multiple sources of proxy data including data from trees, ice cores, subfossil pollen and corals. To create DWJ2006, Rob employed 19 ring-width chronologies and one density chronology from high elevation sites in North America and Eurasia

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[241] Hampel, *Climate Reconstruction and the Making of Authoritative Scientific Knowledge*, Unpublished Ph.D. thesis,April 2015, King's College London.

[242] Mann has recently articulated his vision of these 'wars' in his book *The Hockey Stick and the Climate Wars: Dispatches from the Front Lines*, (New York City: Columbia University Press, 2013).

[243] Michael Mann, "Global Temperatures, Volcanic Eruptions, and Trees that Didn't Bark", *RealClimate*, 6 February 2012. Accessed 15 July 2015, http://www.realclimate.org/index.php/archives/2012/02/global-temperatures-volcanic-eruptions-and-trees-that-didnt-bark/#sthash.GsxX0Hid.dpuf

[244] Rosanne D'Arrigo, Rob Wilson, and Gordon Jacoby. "On the long- term context for late twentieth century warming.", *Journal of Geophysical Research: Atmospheres (1984–2012)*, 2006, Vol. 111.D03103.

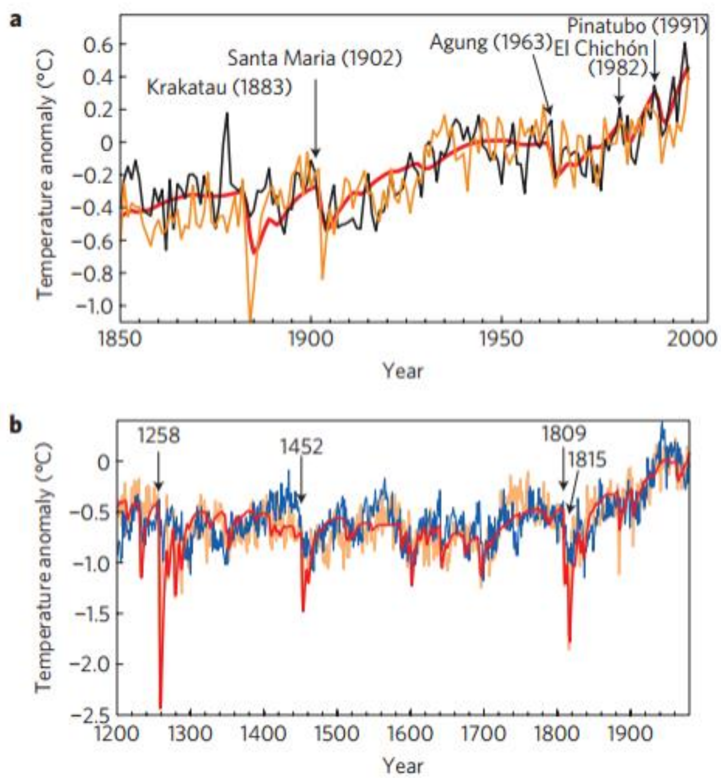
that D'Arrigo and Jacoby had mostly generated and archived in the International Tree-Ring Data Bank (ITRDB). Since then, the data have been used by other dendroclimatologists and paleoclimatologists, including Michael Mann.

Mann et al. (2012) begin their article by reporting a discrepancy following three volcanic eruptions that occurred in 1258, 1452 and 1816, between DWJ2006 and the simulated temperatures they have generated with two climate models. In particular, Mann et al. (2012) report that both climate models predict a drop of 2ºC, whilst DWJ2006 shows a decrease of only 0.6ºC several years after the eruptions (image 33b). Mann et al. (2012) interpret this discrepancy (and resolve the potential of an "experimenter's regress"[245]) by attributing the mismatch to a deficiency in DWJ2006 rather than in the climate models. They justify their claim by comparing the simulated temperatures against observed temperature records over the last century, which they take as the standard. On the assumption of uniformitarianism, Mann et al. (2012) extrapolate backwards in time the similarity they observe between the simulated and observed temperature for the present time (image 33a). They conclude that "given their success in reproducing volcanic cooling events of the historical era, we might expect the models' predictions for previous centuries to be similarly reliable".[246]

[245] The potential of circular reasoning or the "experimenter's regress" in this case would be: Mann et al. (2012) want to know about past climate and use tree-ring reconstructions and climate models to find this out. How do they know if trees and climate models give a truthful reconstruction of past climates? To find this out, they would need to know what past climate was like, but indeed, this is the point of the experiment...*ad infinitum.* As Harry Collins suggests in *Changing Order*, the experimenter's regress is often broken because scientists share commonly agreed criteria of experiment quality. In this case, Mann et al. (2012) share the belief that climate models are a superior source of knowledge of past climates to trees.

[246] Mann et al. (2012), p. 202.

Image 33. Mann et al. (2012) use the graphs below to demonstrate the alleged undercooling bias of Rob's temperature reconstruction (DWJ2006). Graph (b) is said to suggest that the simulated temperature anomalies from two climate models (red and orange lines) are much lower than the reconstructed temperature in DWJ2006 (blue line) for four years of volcanic eruptions. With graph (a), Mann et al (2012) discard the possibility of the mismatch identified in graph (b) being due to deficiencies in climate models by arguing that simulated data (red and orange lines) and instrumental records (black line) are synchronous during the last century.

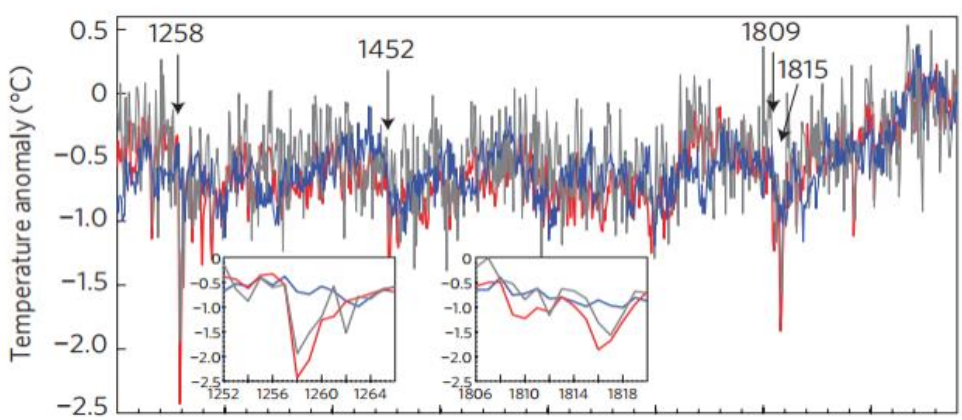


Source: Mann et al. (2012), p.202.

Mann, Fuentes and Rutherford explain that they have explored different hypotheses with regards to why DWJ2006 allegedly underestimates volcanic cooling, the most plausible being that the tree-ring chronologies employed in the reconstruction do not account for widespread missing tree-rings. They base their hypothesis on the "principle of limiting factors", a theory of the way environmental conditions limit the growth of trees. The reasoning that Mann et al. (2012) put forward to support their hypothesis goes as follows: the aerosol particles released into the air by volcanic eruptions block some direct sunlight causing cooling and a more indirect, diffuse light at the surface. With the temperature dropping a couple of degrees after a volcanic eruption, Mann et al. (2012) suggest that many trees at Northern sites like the ones included in the DWJ2006 might stop growing and produce missing rings for the years of the volcanic eruptions. According to the co-authors, the widespread existence of missing rings across trees would make it very difficult for dendrochronologists to detect asynchronies between chronologies with the technique of cross-dating. As a result, most tree-ring chronologies in the Northern Hemisphere would contain misdating errors for the volcanic years, which might explain why DWJ2006 allegedly shows a delayed cooling effect.

Mann et al. (2012) test their supposition that the tree-ring chronologies used in DWJ2006 have multiple missing rings by using the same tree-growth model (VS-Lite) that Rob and Miloš used to confirm the existence of disturbance in the Scottish dataset. In this case, Mann et al (2012) use the VS-Lite model to generate simulated ring-width chronologies as hypothetical representations of the effect of volcanic cooling on trees growing at Northern latitudes as in DWJ2006. Mann et al (2012) use these simulated tree-ring chronologies as standard against which to infer the presence of missing rings in DWJ2006 (image 34).

Image 34. Mann et al. (2012) create this graph in order to demonstrate the hypothetical existence of missing rings in DWJ2006. The large graph compares past temperature values as reconstructed by DWJ2006 (blue line), as simulated by climate models (red line) and as modelled by the VSLite (grey line). Mann et al. (2012) infer the presence of missing rings in DWJ2006 by reporting that DWJ2006 is asynchronous to simulated temperature and simulated tree-ring chronologies around two volcanic eruptions (inset smaller graphs).



Source: Mann et al. (2012), p.203.

Mann et al. (2012) explain that they have made a few adjustments to the original parameters of the VS-Lite model, and justify these changes with a reference to published sources. In terms of the temperature threshold and growing season, Mann et al. (2012) establish the parameter in 50-60 days of a minimum of 10ºC temperature before a ring starts forming. However, they conclude "Our findings are insensitive to the precise details of the growth model".[247] They also establish that any tree-ring that VSLite produces for shorter periods of growth below 26 days (as a result of sudden volcanic cooling) could be interpreted as a missing ring.

On the basis of the adjusted parameters in the VS-Lite and the 26-day threshold definition of missing rings, Mann et al. (2012) predict that more than half of the tree-ring chronologies employed in DWJ2006 would contain missing rings. In particular, they report that 90% of the modelled chronologies produced with VS-Lite might have a missing ring after the 1258/1259 AD eruption, and 55% of chronologies might contain missing rings following the 1815 eruption. As a result, Mann et al. (2012) hypothesise that the dating of the observed tree-ring chronologies in DWJ2006 would be offset many years, as "for each missing ring, an error of one year is introduced in the age model. For example, if there were no growth during 1816, then the 1815 growth ring would instead masquerade for the '1816' ring. Through this process, chronological errors will accumulate back in time as missing rings are encountered".[248]

Mann et al. (2012) suggest that the accumulation of misdated tree-rings would explain the observed delayed cooling in DWJ2006. They write "There is consequently increased temporal smearing back in time in the hemispheric composite. This smearing leads to a predicted delay of 1-2 years in the peak cooling for the 1815 eruption and an even larger delay of 4-5 years for the ad 1258/1259 eruption." Mann et al (2012) conclude their paper by spreading the suspicion of missing tree-rings to all Northern Hemisphere reconstructions "as the potential biases identified in our study necessarily impact all existing hemispheric-scale estimates of the interannual cooling response to volcanic forcing in past centuries". [249]

The hypothesis of missing-rings refers to a few of the epistemological conundrums that Rob and Miloš faced in their work of reconstructing the climate of Scotland, and that I have documented in preceding chapters. The hypothesis raises the possibility that purposive sampling

[247] Mann et al. (2012), p.203.
[248] Mann et al. (2012), p.204.
[249] Mann et al. (2012), p.204.

of trees at Northern Hemisphere sites creates biased samples that systematically underestimate volcanic/climatic cooling. It also refers to the risk that dendrochronologists cannot detect missing tree-rings with cross-dating, and hence cannot produce properly dated tree-ring chronologies. The hypothesis also touches upon the fact that the ring-width parameter might be limited in providing estimates of volcanic cooling and high frequency climate changes more generally. Finally, Mann et al (2012) raise questions about the reliability of extrapolations of past climates given trees do not reflect post-volcanic cooling as recorded by temperature records during the period of calibration and verification. As Rob and many other dendroclimatologists see it, the hypothesis of missing rings put forward by Mann et al. (2012) is a sceptical challenge not just to Rob's climate reconstruction but to the reliability of all Northern Hemisphere reconstructions and the entire practice of dendrochronology.

The epistemological conundrum that dendroclimatologists, and Rob in particular, face at this stage, is knowing how to respond when someone internal (more or less) to the dendroclimatological community poses sceptical questions about knowledge claims and practices that have become fundamental to the production of knowledge within the community. In this instance, the conundrum is heightened by the fact that those questions are posed in a medium (the journal *Nature Geosciences*) that is clearly visible to people external to that community. This chapter explains how Rob, in particular, and other dendroclimatologists deal with this sceptical challenge and try to secure acceptance, within the wider dendrochronological and scientific community, of the fact that tree-ring based climate reconstructions - including the Scottish ones- are a reasonable representation of the way climate has changed over the past millennium.

7. 2 the "Community Response"

As Mann chooses to focus on the supposed inadequacies of Rob's earlier reconstruction, Rob becomes particularly implicated in Mann's critique and in pioneering the defence of dendroclimatology. Two days after Mann has written his post on *RealClimate* announcing the publication of Mann et al (2012), Rob writes a blog comment: "Dear Mike, your paper has certainly generated a lot of discussion over the last few days between some dendroclimatologists. You would appreciate that we are somewhat sceptical of your hypothesis and analyses and are drafting an appropriate measured response to your work".[250] Rob signs with a list of 12 forenames of dendroclimatologists, giving the impression that Mann knows them all personally.

After a few email exchanges, Rob eventually galvanises a total of 23 dendroclimatologists to write what he calls a "community response" to Mann et al. (2012). Rob knows all the signatories in the community response; a few of them are involved in different aspects of the Scottish reconstructions (Malcolm Hughes, Rosanne d'Arrigo, Björn Günnarson, Edward Cook, Kevin Anchukaitis) or have previously worked with Rob as a doctoral supervisor and co-author (Brian Luckman and Jan Esper respectively). After a peer review process that Rob qualifies as "unduly long", the community response (hereafter "Anchukaitis et al. (2012).") is published online on the 25 November 2012 in *Nature Geosciences*. Rob explains that "the analysis and drafting of this response was one of the more interesting periods in my research career. I think we all learned a lot". The 23 dendroclimatologists that eventually sign the letter of response to Mann et al. (2012) dispute the hypothesis of missing tree-rings on three grounds.

First, Anchukaitis et al. (2012) accuse Mann et al. (2012) of selecting "arbitrary" and "unrealistic" parameters in the VS-Lite model. As Miloš points out to me, this criticism is invested with extraordinary authority as the creators of the full and lite versions of the VS model (Alexander V. Shashkin, Eugene A. Vaganov and Kevin J. Anchukaitis) are among the signatories of the letter and "they know what they are talking about". In particular, Anchukaitis et al. (2012) criticise Mann et al. (2012) for employing a 26 days-threshold to define missing tree-rings; for ignoring relevant parameters of the VS-Lite model, and for misusing the equation to generate modelled tree-ring data. The signatories conclude that "these assumptions all bias Mann and colleagues' tree-growth model results towards erroneously producing missing tree rings".[251] To illustrate the consequences of selecting realistic parameters, Anchukaitis et al.

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[250] Rob Wilson, comment on *RealClimate*, "Global Temperatures, Volcanic Eruptions, and Trees that Didn't Bark", 8 February 2012, accessed 15 July 2015, http://www.realclimate.org/index.php/archives/2012/02/global-temperatures-volcanic-eruptions-and-trees-that-didnt-bark/

[251] Kevin J. Anchukaitis; Petra Breitenmoser; Keith R. Briffa; Agata Buchwal; Ulf Büntgen; Edward R. Cook; Rosanne D. D'Arrigo; Jan Esper; Michael N. Evans; David Frank; Håkan Grudd; Björn E. Gunnarson; Malcolm K. Hughes; Alexander V. Kirdyanov; Christian Körner; Paul J. Krusic; Brian Luckman; Thomas M. Melvin; Matthew W. Salzer; Alexander V. Shashkin; Claudia Timmreck; Eugene A. Vaganov and Rob J. S. Wilson; "Tree Rings and Volcanic Cooling.", *Nature Geoscience, 2012, Vol.* 5.(12), p. 836.

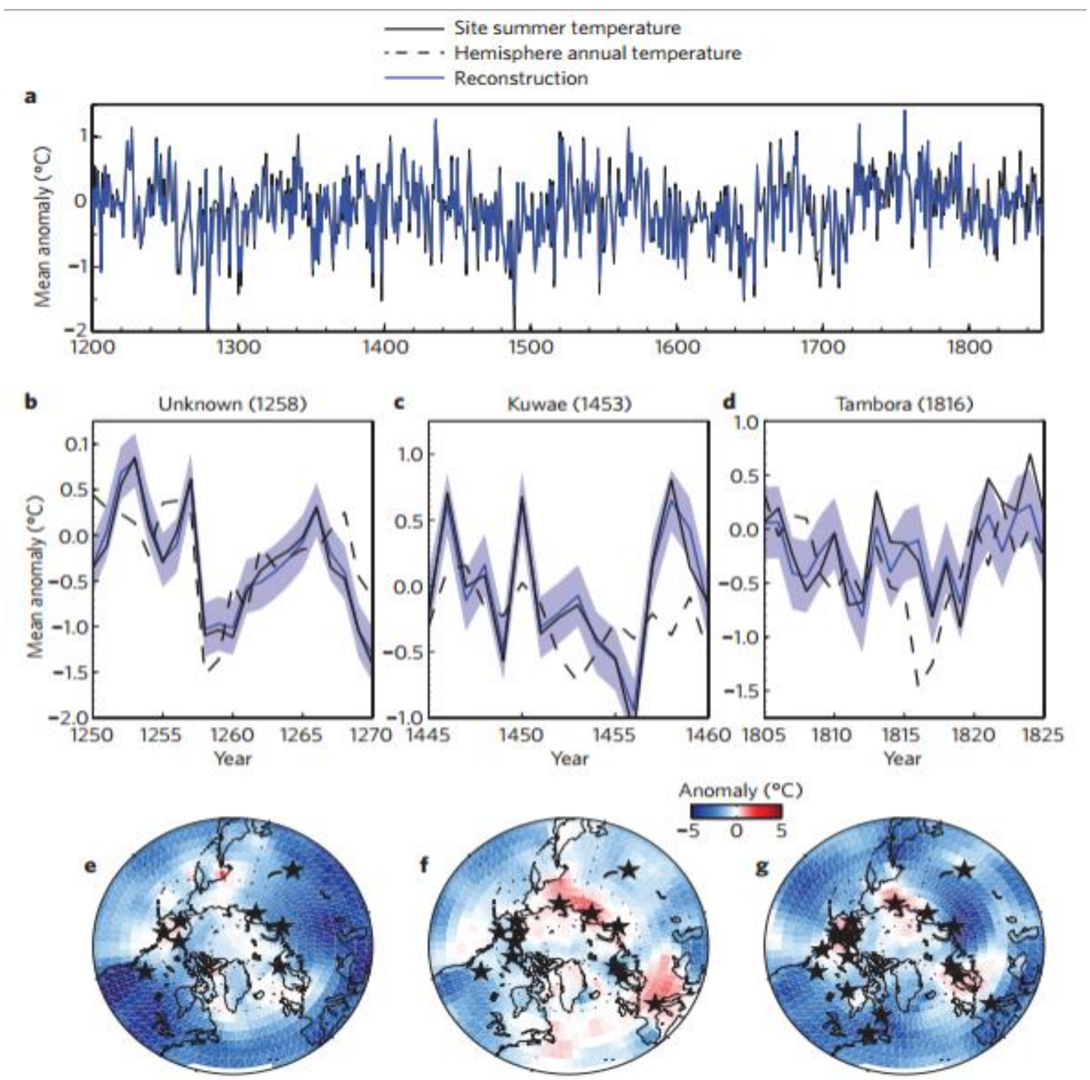
(2012) generate a series of graphs that show how “realistic” VS-Lite chronologies are in good agreement with both DWJ2006 and simulated temperature (image 35).

Second, Anchukaitis et al. (2012) criticise Mann et al. (2012) for attributing more credibility to climate models than to trees or nature. They argue that Mann et al (2012) have not taken into account in their simulations the fact that the network of tree-ring chronologies employed in DWJ2006 is spatially distributed and that the resulting simulated temperature must vary accordingly. They include a graph to illustrate this later criticism (image 35 e, f, g). Anchukaitis et al (2012) also enumerate what they think are more general faults of climate models and conclude that “an alternative hypothesis of an overestimation of volcanically induced cooling in the simulations cannot be ruled out”.[252]

Finally, Anchukaitis et al. (2012) criticise Mann et al (2012) for not providing “empirical evidence” of misdating errors in tree-ring chronologies. As a response, Anchukaitis et al. (2012) include a supplementary figure that shows the synchronicity of DWJ2006 with an independent density-based Northern Hemisphere temperature reconstruction. They conclude that whilst DWJ2006 shows a “muted cooling coincident with volcanic eruptions”, both reconstructions “show precise correspondence with the timing of explosive volcanic eruptions”.[253]. The signatories refer to different sources of literature to clarify that the diminished cooling expressed by the ring-width based reconstruction is partly due to the well-known fact in the community that the ring-width parameter (because of its physiological basis) responds slower to short term changes in climate. As the density and ring-width data of the supplementary figure are generated from the same samples, signatories also argue that the synchronicity between density and ring-width chronologies in DWJ2006 is evidence that the latter is correctly dated.

[252] Anchukaitis et al. (2012), p. 837.
[253] Anchukaitis et al. (2012), p. 837.

Image 35. Rob and colleagues illustrate their community response to Mann et al. (2012) with a series of graphs that they include as one single figure. The upper and middle graphs are meant to refute the allegation that DWJ2006 (blue line) contains missing rings and underestimates volcanic cooling by showing its synchrony with a "realistic" set of new VS-Lite modelled tree-ring chronologies (black line) and a simulated temperature series created with an alternative climate model (dash line) over a long period from 1200 until the late 19th (a) century as well as for three specific volcanic periods (b,c,d). The last three images of the Northern Hemisphere (e, f, g are intended as a criticism to how Mann et al. (2012) have used climate models. Anchukaitis et al. (2012) argue that simulated temperature must vary (as expressed in a scale of colours) in relation to the location of the chronologies as indicated by stars.



Source: Anchukaitis et al. (2012), p. 836.

On the same day that Anchukaitis et al. (2012) is published online, Rob sends a message to the members of the international dendrochronology mailing list. Rob presents the implications of the missing tree-rings hypothesis as a matter of concern not only to dendroclimatologists, but to the entire community of dendrochronologists as "this [hypothesis of missing tree-rings] implies Dendrochronology's inability to detect missing rings".[254] In response to Rob's message, a forester and member of the forum writes "'A temporary cessation of tree growth' resulting in no rings for all trees? Now this is a hypothesis that I am willing to bet good money has no empirical support since studies of trees began 200 years or so ago. Speculation this bald could give dendrochronologists a bad name." Malcolm Hughes - Mann's collaborator on the hockey-stick graph and one of the signatories of the community response - responds to the previous comment that "No dendrochronologists were involved in the offending Mann et al 2012 paper. What Rob described was the response of a number of us to some of the multiple flaws in the original paper".[255]

As the dendrochronology internet forum is publicly accessible, a few bloggers report on the publication of Anchukaitis et al. (2012) and on the shifting alliances between Mann and a few other dendroclimatologists that this controversy represents. Anthony Watts writes a blog entry entitled "Dendros stick it to the Mann" where he notes that "what is most interesting is that Hughes and Briffa are co-authors of the response to Mann".[256] Similarly, the Scottish blogger Andrew Montford also writes a post on his blog "Bishop Hill" entitled "Lonely Old Mann", where he comments "The list of authors of the new paper is very long. Almost looks like they are ganging up on him. ;-)". [257]

[254] Rob Wilson, "[ITRDBFOR] Comment to Mann et al. (2012) at Nature Geoscience", 25 November 2012, 20:43, not available online. The last time I accessed these messages was in August 2014. The dendrochronologist Henri Grissino-Mayer informs me that, sadly, "the archived messages of the ITRDB Internet forum are no longer available largely because the University of Arizona no longer hosts the ITRDB dendro forum on its servers" (personal communication). I have a copy of the messages but I cannot provide a link to them.

[255] Malcolm Hughes, "[ITRDBFOR] Comment to Mann et al. (2012) at Nature Geoscience", 26 November, 2012 16:42.

[256] *Wattsupwiththat*, "Dendros Stick it to the Mann", 25 November 2012 http://wattsupwiththat.com/2012/11/25/dendros-stick-it-to-the-mann/ (accessed 8 April 2015)

[257] Andrew Monford - Bishop Hill blog - Lonely old Mann." 2012. 24 November 2012 <http://www.bishop-hill.net/blog/2012/11/26/lonely-old-mann.html> (accessed 8th of April 2015)

As Rob follows the blog Bishop Hill regularly, he comments on Montford's entry to clarify the nature of his disagreement with Mann. Rob complains that Mann does not share the dendroclimatologists' understanding that "nature" and trees provide the truly real evidence for climate signal rather than climate models. Rob explains "Mann's major flaw was to see something in his model which did not agree with "nature" and assumed that there must be something wrong with nature. Alas, if he had taken the trouble either (1) to speak to some of his dendrochronological colleagues or (2) look at some real tree-ring data to learn what "cross-dating" is, he would have quickly realised that his hypothesis was wrong and would not have wasted a lot of time for many people."[258] In a follow-up comment on Bishop Hill, Rob also expresses his wish to continue the discussion face-to-face with Mann very soon. Rob hopes he will be able to meet Michael Mann at a conference of American geophysicists in San Francisco (US) a month later, in December 2012, where Mann will give four plenary talks. Rob comes back from the conference disappointed that he has not been able to talk to Mann. To date, Rob and Mann have only met once face-to-face and that was before the controversy occurred. Since then, they have developed a fairly hostile relationship through interactions in private electronic emails and online social media.

7. 3 the Amplification of the Controversy in the Online World

In October 2013, Rob becomes part of an online discussion on Montford's Bishop Hill about the missing tree-rings hypothesis. This discussion eventually involves outsiders to the community of dendroclimatology who spur the controversy in different ways. The online discussion starts when Rob invites Andrew Montford to attend one of his undergraduate lectures on millennial temperature reconstructions. A few days later, Montford writes a blog entry about Rob's lecture where he describes some of Rob's criticisms of Mann's statistical methods for the hockey stick as a "gentle beginning". Montford follows by saying, "The real fireworks came when Mann's latest papers, which hypothesise that tree ring proxies have large numbers of missing rings after

[258] Rob Wilson, Comment on "Lonely old Mann", *Bishop Hill blog* , 26 November 2012, 5:28pm.

major volcanic eruptions, were described as "a crock of xxxx".[259] In a matter of hours, Montford's blog post triggers tens of comments from blog readers who appeal to Rob for clarification.

Rob responds immediately to Montford's blog entry by posting a comment where he clarifies that "My 2 hour lecture was, I hope, a critical look at all of the northern hemispheric reconstructions of past temperature to date. It was not focussed entirely on Michael Mann's work." He adds that "My criticism of Mann's work is all published in the literature" and lists a few links with pdf articles attached. Rob finishes by saying "Lastly, the "crock of xxxx" statement was focussed entirely on recent work by Michael Mann w.r.t. hypothesised missing rings in tree-ring records (a whole bunch of papers listed below). Although a rather flippant statement, I stand by it and Mann is well aware of my criticisms (privately and through the peer reviewed literature) of his recent work".

In another follow-up comment, Rob defends the method of cross-dating and interprets Mann's hypothesis in terms of his inexperience with fieldwork and laboratory work. "To be less flippant and putting aside criticisms of tree-ring series as proxies of past climate, the method of cross-dating is robust and easily verifiable by different groups. I would be surprised if Mann has ever sampled a tree, looked at the resultant samples and even tried to crossdate them. He has utterly failed to understand the fundamental foundation of dendrochronology".

I learn about the episode in Bishop Hill because Rob sends an email to both Miloš and me expressing his concern that this episode might antagonise Mann. Rob writes "It looks like Michael Mann has seen this [a link to Bishop Hill] and, to say the least, is not very happy. Oh well - maybe this is a good lesson on how to wreck one's career. Or, maybe this highlights how difficult it really is to communicate with sceptics as the whole discussion very quickly gets personal without individuals really looking at the facts".

Miloš confirms that Mann has expressed on Twitter his dissatisfaction with Rob's comments. Specifically, Mann has tweeted "Rob Wilson not a climate change denier but has played a contrarian role in debate…[260]. Before Rob reaffirms his "crock-of-xxxx" statement on

[259] Andrew Montford, "Wilson on Millennial Temperature Reconstructions", *Bishop Hill blog*, 21st of October 2013, http://bishophill.squarespace.com/blog/2013/10/21/wilson-on-millennial-temperature-reconstructions.html (accessed 11 April 2015).

[260] Michael Mann, Twitter post, 21 October 2013, 07:11, https://twitter.com/michaelemann/status/392292189176619008 (accessed 11 April 2015).

Bishop Hill, Mann writes another tweet that says “Awful blog piece (bishophill. net/blog/2013/10/2…) may well have misrepresented Rob Wilson's views. I suspend judgment, pending his disavowal of it…” [261]

Mann’s comments trigger more reactions from other Tweeter users, either criticising or defending Rob. Mann’s co-editor on the blog *RealClimate*, Gavin Schmitt, implicitly refers to the episode when he writes that “science is not linear. Interesting ideas can be proposed & challenged (w/o anyone's work being a 'crock'). Leads to deeper understanding”.[262] Among those who tweet in support of Rob is the dendrochronologist Scott St. George, who comments, “Rob Wilson (U St. Andrew's) is a fine dendrochronologist and paleoclimatologist, a thoughtful scientist, and 100% not a 'climate denier'.”[263] Similarly, the computer modeller Tamsin Edwards comments to Mann, “[Y]ou are seriously calling Rob a denier for criticising your work, M? That's pretty strong to call a prof climate colleague”[264]

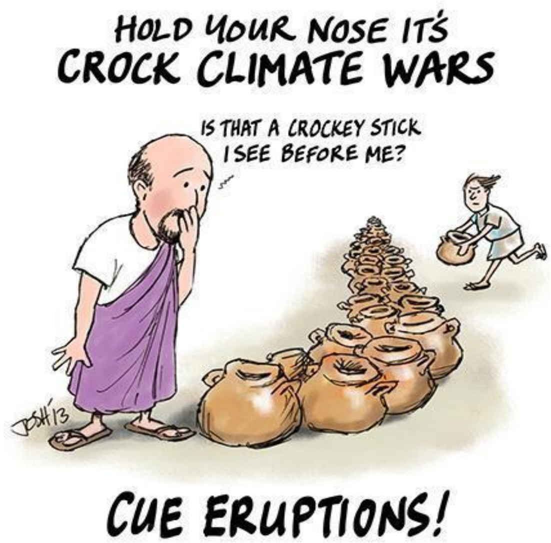
After a few private emails with Mann, Rob tells me that he feels “somewhat anxious” that Mann could ostracise him from the community of climate scientists. “Mann is a very influential scientist”, he tells me. Rob feels relieved when he starts receiving calls and emails of support from colleagues and friends. “Jan called me yesterday to tell me that he found the whole episode very amusing”. Rob also tells me that the last email he receives from Mann says that Mann considers their professional and personal relationship finished.

A few weeks after these public and private online exchanges take place, a cartoonist, who publishes on Montford’s blog and is the creator of the “Climate Skeptics Calendar”[265], illustrates the episode in a cartoon (image 36). Essentially, this drawing uses Rob’s words to criticise Mann. The cartoon draws on the etymology of the expression “crock of shit” - which allegedly relates to the ancient Roman tradition of judging the quality of philosophers by the

[261] Michael Mann, Twitter post, 21 October 2013, 08:27, https://twitter.com/michaelemann/status/392311221371678720 (accessed 11 April 2015).
[262] Gavin Schmidt, Twitter post, 21 October 2013, 13:39, https://twitter.com/climateofgavin/status/392384557523025920 (accessed 11 April 2015).
[263] Scott St George, Twitter post, 21 October 2013, 06:37, https://twitter.com/scottstgeorge/status/392283501552484352 (accessed 11 April 2015).
[264] Tamsin Edwards, Twitter post, 21 October 2013, 06:16, https://twitter.com/scottstgeorge/status/392283501552484352 (accessed 11 April 2015).
[265] “The Cartoons by Josh”, calendar page, http://www.cartoonsbyjosh.com/calendar2015order.html (accessed 11 April 2015).

number of pots or crocks of excrement people piled in front of their houses - to depict Mann wondering if his hockey stick graph is a "crockey stick".

Image 36. This cartoon shows how Rob's critical comment on the missing tree-rings hypothesis is used against Michael Mann by a blogger.



Source: "More battling", Josh 241. 21 October 2013.
http://bishophill.squarespace.com/blog/2013/10/21/more-battling-josh-241.html

7. 4. The (Temporary) Closure of the Controversy

While Anchukaitis et al. (2012) is being reviewed for publication, dendrochronologists conduct studies examining other aspects of the missing tree-ring hypothesis. These parallel conversations take place in more specialised journals of dendrochronology, volcanology and geophysics, which have a more restricted audience than *Nature Geosciences* where the controversy started.

The dendrochronologists' choice of journals where to continue the scientific discussion with Mann et al. (2012) might reflect the fact that, in the opinion of one dendrochronologist I talk to at the conference in Aviemore, the controversy has been publicised too widely. He tells me that Mann has been "disrespectful towards the community in airing his damaging hypothesis in a widely read journal like *Nature Geosciences* where other scientists might get a wrong impression from our work". In his opinion, it would have been more appropriate if Mann had published his hypothesis in a dendrochronology journal so that "we would have dealt with it first". This dendrochronologist tells me that, overall, the publication of the hypothesis has had a positive effect on dendrochronology. "If there's something we have to thank Mann for, is to force us to demonstrate in multiple ways the good work we've done for decades".

For a few months, dendrochronologists publish a series of "revisionist" articles where they re-examine aspects of their work in the light of the accusations made by Mann et al. (2012). In May 2013, a group of German and Swiss dendrochronologists validate the longest density chronology from Northern and Central Europe, which includes many of the chronologies used in DWJ2006[266]. In June 2013, the same authors use this density chronology to conclude that post-volcanic cooling has a limited effect on long-term climatic trends[267] and in this way, minimise the consequences for dendroclimatology of the fact that DWJ2006 shows a "muted post-volcanic cooling". In July 2013, three North-American dendrochronologists re-analyse all the 2359 archived ring-width chronologies across the Northern Hemisphere in the International Tree-Ring Data Bank and calculate a "percentage of the frequency of missing rings". These authors conclude "Recently, Mann et al. [2012] argued that discrepancies between climate model simulations and dendroclimatic reconstructions were due to unrecognized absent rings and resulting chronological errors. This scenario is not consistent with the pattern of absent-ring formation outlined by more than 17 million tree rings. Locally absent rings are extremely rare in tree-ring records from high latitudes (Figure 2b, Figure 3) and high elevations (Figure S4)". [268] Overall, dendroclimatologists interpret all these studies as a vindication of the dating of existing chronologies and a refutation of Mann's hypothesis.

Rob also co-authors with Rosanne D'Arrigo and Kevin Anchukaitis a "review" of the evidence of accurately dated tree-ring chronologies from Northern sites where Rob brings to bear

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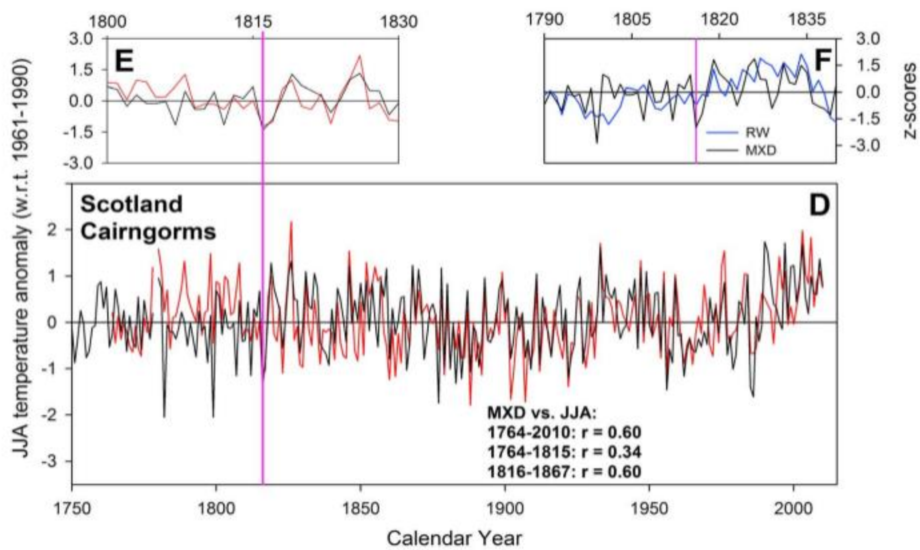
[266] Jan Esper et al, "Testing the hypothesis of post-volcanic missing rings in temperature sensitive dendrochronological data.", *Dendrochronologia*, 2013, Vol. 31.(3), pp. 216-222.
[267] Jan Esper, et al. "European summer temperature response to annually dated volcanic eruptions over the past nine centuries.", *Bulletin of Volcanology*, 2013, Vol.75.(7), p. 1-14.
[268] Scott George et al., "The rarity of absent growth rings in Northern Hemisphere forests outside the American Southwest.", *Geophysical Research Letters*, 2013, Vol. 40 (14), pp. 3730.

the Scottish dataset.[269] In particular, Rob employs the dataset from the Scottish Cairngorms, among other datasets, to refute the prediction made by Mann et al. (2012) with VS-Lite that that 90% of the chronologies from the Northern Hemisphere would present a missing ring in 1815, the year of the volcanic eruption. He creates a graph that shows the quantitative agreement between ring-width and density datasets from the Scottish Cairngorms against the long monthly temperature series that exist for Scotland over a long period as well as for the 1815 volcanic year (image 37 e and d). This synchrony is interpreted by Rob and co-authors as evidence of the inexistence of a missing tree-ring because “if the 1816 tree ring was missing from all Scottish trees then the correlation would break down. For the 52 year period from 1816 to 1867, MXD correlates with JJA mean temperatures at 0.60. Prior to 1816 (1764–1815), the correlation is 0.34”.[270]

[269] Rosanne D'Arrigo, Rob Wilson, and Kevin J Anchukaitis. "Volcanic cooling signal in tree ring temperature records for the past millennium.", *Journal of Geophysical Research: Atmospheres*, 2013, Vol.118.(16).
[270] Rosanne D'Arrigo, Rob Wilson, and Kevin J Anchukaitis, *Idem*, p.7.

Image 37. With the graph below, Rob mobilises the Scottish data to refute Mann's hypothesis. Rob infers that the Scottish chronologies are accurately dated from the graphs E and D that show the synchrony between the temperature data (red line) and the density chronologies from the Cairngorms (black line) throughout a long period from 1750 until 2000 as well as in the 1815 volcanic year (vertical line in E). In graph F, the relative difference in the synchrony between the ring-width and density chronologies at the year of the volcanic eruption (vertical line) is seen by Rob as a confirmation that, whilst density is known to be a better recorder of short term volcanic cooling, both chronologies are well dated.



Source: Rosanne D'Arrigo, Rob Wilson, and Kevin J Anchukaitis, *Idem*, p.8.

The authors also draw on Rob's intimate knowledge of the existence of disturbance in the Scottish dataset to justify the fact that the density chronologies from the Cairngorms show a lower correlation response (0.34 compared to 0.60) prior to the 1815 volcanic eruption. They argue that "although the correlation between MXD and growing season climate is weaker for this earlier period, this likely reflects both the markedly weaker replication in the MXD records in the 18th century as well as management related disturbance in these woodlands through the 18th and early 19th centuries [Wilson et al., 2012]."[271] The fact that the density chronologies from the Cairngorms suggest more cooling than the ring-width chronologies for the period 1815-1816 is seen by Rob, Rosanne D'Arrigo and Kevin Anchukaitis as a confirmation of the previous point

[271] Rosanne D'Arrigo, Rob Wilson, and Kevin J Anchukaitis, *Idem*, p.7.

made in Anchukaitis et al. (2012) that these two tree-ring parameters provide distinct climate information (density reflects more high frequency/annual climate changes, ring-width provides more low-frequency/centennial climate variability).

> Rob and his co-authors conclude their review by restating the value of the method of cross-dating proven by generations of dendrochronologists and the deficiencies of the simulations generated by Mann et al. (2012). "Given the past century of the proven methodology of cross-dating in dendrochronology, the MFR12a theory [Mann et al. (2012)] can only be validated by using evidence from real tree ring data rather than model simulations (which have been shown to not accurately reflect tree biology or the actual distribution of the DWJ06 network [Anchukaitis et al. (2012, 2012a)], and specifically tree ring data with a clean high-frequency volcanic signal (specifically, MXD, not RW)."[272]

Rob's re-examination of the Scottish dataset is a response to another article published by Mann, Rutherford and a few new co-authors ("Mann et al. (2013)" hereafter)[273]. In their new article, Mann et al. (2013) insist that cross-dating is not an adequate technique for detecting widespread missing rings across different chronologies because "the fundamental challenge is that one cannot identify what simply is not there (...) detection, empirically, of a regional-scale pattern of missing rings in tree line-proximal chronologies requires a more nuanced approach".[274] Authors offer a modelling technique (the Monte Carlo simulation) as an alternative "nuanced approach" for detecting the existence of missing rings. On the basis of the percentage of missing tree-rings hypothesised by the VS-Lite model in the Mann et al (2012), Mann and the new co-authors in Mann et al. (2013) make some predictions about the existence of missing tree-rings in the chronologies in DWJ2006.

In particular, Mann et al. (2013) employ the Monte Carlo modelling technique to perform simulated tree-ring chronologies that they use as a standard against which tree-ring chronologies in DWJ2006 should be "corrected" or "aligned" to express the predicted post-volcanic cooling. In the caption of a graph that illustrates the predicted "alignment" between simulated and observed tree-ring chronologies, Mann et al. (2013) present their modelling approach as a more

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[272] Rosanne D'Arrigo, Rob Wilson, and Kevin J. Anchukaitis, *Idem*, p. 9.

[273] Mann, Michael E et al. "Discrepancies between the modeled and proxy- reconstructed response to volcanic forcing over the past millennium: Implications and possible mechanisms.", *Journal of Geophysical Research: Atmospheres*, 2013, Vol.118.(14), pp. 7617-7627.

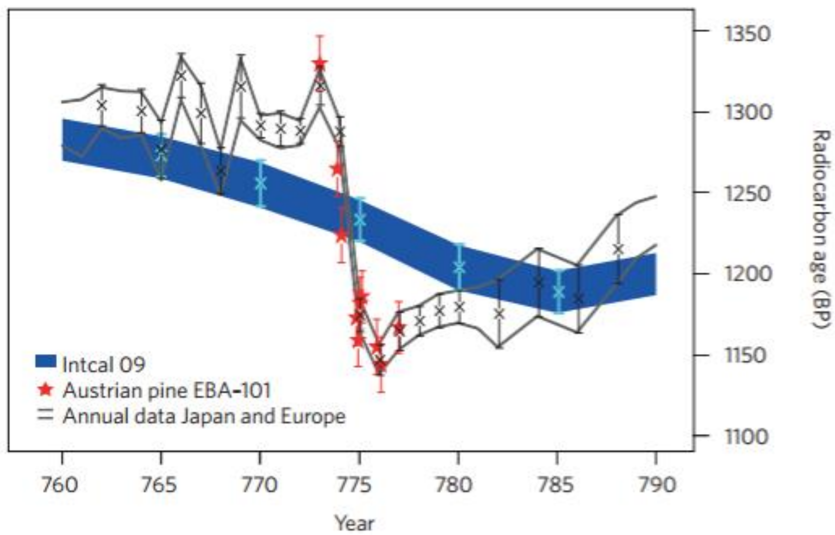
[274] Mann, Michael E et al. (2013), p. 7618.

"objective" technique for detecting missing rings than cross-dating. They say that "This example illustrates how the more objective chronological resampling procedure used in our analyses recovers volcanic cooling events that may have been obscured by chronological errors back in time due to missing rings".[275] Mann and Rutherford employ this modelling methodology for detecting missing rings in their last published exchange with dendroclimatologists.

In May 2014, a group of eight dendrochronologists from different universities in Europe ("Büntgen et al. (2014)" hereafter) claim in the journal *Nature Climate Change* to have found an "extraterrestrial confirmation of tree-ring dating".[276] These authors are referring to carbon dating as "extraterrestrial confirmation" because the isotope concentration in the wood on which carbon dating is based depends on physical and extraterrestrial processes. In particular, Büntgen et al. (2014) use the carbon dates of a ring-width chronology from the Alps included in DWJ2006 as an "independent" verification of the dating of all the remaining tree-ring chronologies from the Northern Hemisphere. Büntgen et al. (2014) report that the Alpine chronology shows an increase in concentration of carbon isotopes (14C) from the year 774 to the year 775, which coincides in time with a peak in carbon isotopes in two tree-ring chronologies from Japan and Germany respectively (image 38). Authors claim that the fact that the carbon date AD 775 coincides in three chronologies from different continents offers "an independent, geochemical age determination for dendrochronologically dated tree-ring chronologies"[277], and thus, a conclusive refutation of the hypothesis of missing rings.

[275] Mann, Michael E et al. (2013), p.7623.
[276] Ulf Büntgen et al., "Extraterrestrial Confirmation of Tree-Ring Dating." *Nature Climate Change*, 2014, Vol. 4 (6), pp. 404-405.
[277] Ulf Büntgen, et al. (2014), p.404.

Image 38. A group of European dendrochronologists present the graph below derived from carbon dating as a conclusive refutation of Mann's hypothesis. It shows the synchrony around the carbon year 774-775 AD between an independent Alpine chronology (red asterisks) and Japanese and German carbon dated chronologies (black and blue lines).



Source: Ulf Büntgen, et al. (2014), p. 404.

In their response in July 2014 in the journal *Nature Climate Change*, Michael Mann and Scott Rutherford (hereafter "Mann and Rutherford (2014)") accept the carbon dating evidence put forward by Büntgen et al. (2014) "as an independent time-marker necessary to directly test our hypothesis".[278] Mann and Rutherford (2014) use the 774-775 radiocarbon date as a standard against which to test the simulated chronologies they generate with their Monte Carlo modelling methodology. They explain, "Based on our previous results, we can make predictions that are consistent with our hypothesis and that can be tested using the ad 774–775 radiocarbon event and existing tree-ring chronologies". First, Mann and Rutherford (2014) create a simulated chronology that models the cooling conditions of the Alpine chronology that Büntgen et al. had previously used. Mann and Rutherford report that their simulated chronology and the Alpine one are very similar, which they interpret as a confirmation of both the hypothesis of missing rings and the claim made by Büntgen et al. that the Apian chronology is well dated. Second, Mann and Rutherford (2014) seek to scrutinise their hypothesis further by simulating the only four ring-

[278] Scott Rutherford and Michael E Mann, "Missing tree rings and the AD 774-775 radiocarbon event.", *Nature Climate Change*, 2014, Vol.4.(8), pp. 648-649.

width chronologies used in DWJ06 that reach back to AD 774, the dating of which could be potentially validated with carbon dating. They report that these four chronologies would need to be corrected a few years to account for the predicted missing rings and the AD 774-775 carbon date. However, as the dating of these four chronologies has not been yet validated with carbon dating, Mann and Rutherford (2014) say that they cannot accept or discard their simulated chronologies. They conclude their article by reinstating the promise of the carbon dating technique of resolving the controversy as "the discovery of the ad 774–775 radiocarbon event seems to be the key to testing our missing-ring hypothesis".[279]

Whilst for Mann and Rutherford (2014) the controversy remains open until dendrochronologists or others produce more carbon dated chronologies that determine whether they synchronise with their simulated predictions, Rob perceives Mann's latest evidence as irrelevant as he regards Büntgen et al. (2014) and the "revisionist" studies as sufficient refutation of the hypothesis of missing tree-rings. When I ask by email Rob's opinion about Mann and Rutherford (2014), he insists that it is a "crock of xxx" and explains that Mann "is adding in rings all over the place to make the tree-ring chronologies fit the models. Ignore it – it is awful work".

Rob shows a similar lack of interest to discuss the evidence put forward in November 2014 by a group of three scientists (hereafter "Tingley et al. (2014)") in the journal *Geophysical Research Letters* who argue that density-based temperature reconstructions overestimate post-volcanic cooling.[280] These authors argue that the density parameter (rather than ring-width as argued by Mann et al.) does not offer accurate estimates of past climate because it does not capture the reduced availability of sunlight after an eruption. Rob personally knows the first co-author, Martin Tingley, who is the "Bayesian guy" who Rob thinks could be a useful collaborator for estimating more realistic errors in future climate reconstructions (Chapter 7). When I ask Rob by email what he thinks about the overestimation hypothesis and how it fits with Mann et al. (2012), he succinctly responds "Both [are] on the extreme end of the reality". He explains, "Mann focused on ring-width - he got it completely wrong. They [Martin Tingley and co-authors] focus on MXD [density] - they have a theoretical point but difficult to quantify as

[279] Rutherford and Mann, 2014, p.649.

[280] Martin Tingley, Alexander Stine and Peter Huybers, "Temperature reconstructions from tree-ring densities overestimate volcanic cooling", *Geophysical Research Letters*, 2014, Vol 41, (22), pp. 7838–7845.

you need to focus on early instrumental data which is problematic". Rob concludes by defending the value of density-based climate reconstructions as "there is no doubt we don't fully understand MXD response to climate, but it is still the best proxy for summer temperatures and hence climate response to volcanoes".

A few bloggers regard the hypothesis of overestimation of post-volcanic cooling as supporting their views about the politicised nature of climate science. Two guest bloggers on the blog *Wattsupwiththat* note that Mann has not written anything on his blog *RealClimate* responding to the overestimation hypothesis, "but regardless, there is no escaping the fact that the 'Tingley study' provides additional evidence that the earth's climate sensitivity to human greenhouse gas emissions is likely less than advertised by the UN IPCC and the Obama Administration. The direct result being that headlong pursuit of carbon dioxide emissions limits should be reconsidered in light of this and other scientific literature."[281] Another blogger evaluates the emergence of the overestimation hypothesis as evidence that climate science is not as consensual as the IPCC "advertises", as "the fact that this 'recent' major controversy about whether tree-ring reconstructions overestimate or underestimate cooling from aerosols is a stark reminder that very little about climate science is truly "settled science, despite the propaganda claiming otherwise".[282]

To date, Michael Mann has not responded or commented on the overestimation hypothesis in the peer-reviewed journals or elsewhere, and Rob tells me that he is not aware of any "community response" to the overestimation hypothesis because he thinks that, unlike Mann's hypothesis, "there is some theoretical basis to this [overestimation] idea at least".

The controversy over the underestimation (and potentially overestimation) of tree-ring based reconstructions ends because a few participants abandon it over time. After mounting a "community response" and publishing a series of "revisionist" studies, dendroclimatologists feel that their intervention has "settled" the dispute. Instead, Mann and Rutherford (2014) regard the controversy as temporarily open until someone provides new carbon dating evidence. Indeed, in

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[281] J. P. Michaels and C. P. Knappenberger, "Mann's not so Explosive Findings on Volcanos' Climate Influence in Tree Rings", *Wattsupwiththat*, 10 October 2014, http://wattsupwiththat.com/2014/10/10/manns-not-so-explosive-findings-on-volcanos-climate-influence-in-tree-rings/ (accessed 13 April 2015).

[282] New paper finds temperature reconstructions from tree-rings overestimate volcanic cooling", *The Hockey Schtick,* 8 October 2014, http://hockeyschtick.blogspot.co.uk/2014/10/new-paper-finds-temperature.html (accessed 13 April 2015).

August 2015, Rob tells me that the main author of the Büntgen et al. paper (Ulf Büntgen) is leading a project that seeks to validate the dating of other tree-ring chronologies around the world with carbon dating, and could potentially foreclose forever the controversy over the hypothesis of missing tree-rings.

7.5 Discussion

Dendroclimatologists respond to Mann's sceptical challenge with the mobilisation of evidence that they have subjected to civil and organised scepticism interwoven with the realignment and consolidation of the trust relations that constitute the boundaries of the dendroclimatological community.

Mann and dendroclimatologists are able to engage in a conversation in the first place because, as paleoclimatologists, they all share a few aspects of the fiduciary framework of dendroclimatology. The existence of this fiduciary framework also explains why Mann and Hughes were able to collaborate in the past on the creation of the hockey stick graph. In the particular case of the controversy over the hypothesis of missing tree-rings, the sharing of this framework occurs in relation to two commitments. First, Mann et al. (2012) and Anchukaitis et al. (2012) trust meteorological records to be the most credible source of evidence for past climates. Accordingly, they all use meteorological records as the standard against which to compare their simulated temperature and tree-ring chronologies. Second, Mann et al. (2012) and Büntgen et al. (2012) trust the carbon dating method as an indisputable source of verification of tree-ring dating. Therefore, they all accept that the evidence from carbon dating should potentially resolve the controversy. Even though none of the participants in the controversy explicitly state so, it is plausible to assume that, as competent paleoclimatologists, they are all aware of the potential limitations of meteorological records and the uncertainties regarding carbon dating that both Rob and Milos articulated in previous chapters (Chapters 3 and 6). The fact that all participants in the controversy suspend their scepticism and use carbon dating and meteorological records as standards against which to compare their evidence is the basis upon which they are able to have a certain common ground for discussion, and a potential definitive closure of the controversy might occur in the future when Ulf Büntgen and his team produce new carbon dating evidence.

Importantly, many of the aspects of Mann's hypothesis have already been addressed in a civil manner within the fiduciary framework of the dendroclimatological community. First, Mann's hypothesis suggests potential problems with the purposive sampling of trees in the sense that it might produce biased samples with widespread missing rings, which is similar to the civil scepticism practiced by dendroclimatologists with the "Modern Sample Bias". Second, the hypothesis suggests that tree-ring chronologies might contain missing tree-rings, which is a possibility and an epistemic conundrum that all dendroclimatologists face throughout the entire work of tree-ring dating and resolve by sceptically examining the interpretation, representation and comparison of tree-rings. Third, the hypothesis also claims that ring-width data might be limited in offering information about volcanic cooling and high-frequency climate variability more generally, which is exactly the point of the qualified sceptical examinations that Rob and others are currently carrying out with Blue Intensity and density. Fourth, the hypothesis suggests that tree-ring based temperature reconstructions underestimate the temperature cooling recorded by instrumental records, which is a similar phenomenon to the "divergence problem" that dendroclimatologists have been investigating over the last decades.

The controversy over the hypothesis of missing tree-ring represents a turning point in the sense that a few dendroclimatologists, including Mann's previous collaborator Malcolm Hughes, find Mann's hypothesis to be "offensive" and a gesture of uncivil scepticism towards them. From the perspective of dendroclimatologists, Mann's arguments challenge the fiduciary framework in a way that is different from civil scepticism. Yet, if many of Mann's arguments have been or are currently being examined in a civil way by dendroclimatologists in other contexts, what exactly is it about Mann's challenge that threatened the fiduciary framework of dendroclimatology? The difference between uncivil and civil scepticism, I would argue, lies in the content of the responses and the way dendroclimatologists put forward their evidence as the means to repair the fiduciary framework.

My interpretation is that many dendroclimatologists regarded Mann's formulation of the hypothesis of missing tree-rings as uncivil because they saw it as questioning their collective ability to subject the fiduciary framework of dendroclimatology to the kind of civil and organised scepticism that I have just documented in the previous chapters. The knowledge and professional identity of dendroclimatologists is rooted in a fiduciary framework and a set of shared beliefs and technical skills that all dendroclimatologists trust, not least because they have subjected them to the type of civil scepticism I have outlined throughout this thesis. With the formulation of his hypothesis, dendroclimatologists realise that Mann does not show an awareness of the care and critical attitude that dendrochronologists produce samples, date tree-ring chronologies, assess the different complementary qualities of tree-ring parameters and evaluate the reliability of extrapolations and climate reconstructions. Rob is particularly adamant about explaining Mann's unfamiliarity with the dendroclimatologists' fiduciary framework by referring to Mann's lack of experience and socialisation into dendroclimatological work. In Rob's opinion, if Man had ever cored a tree and tried to crossdate tree-ring chronologies, he would know that dendrochronologists subject these tasks to intense organised scepticism. In other words, Mann's hypothesis is regarded by a few dendroclimatologists as a gesture of mistrust in their ability to conduct their job properly, including being appropriately sceptical about their own knowledge.

The fact that Mann displayed his scepticism in a widely visible journal venue like *Nature Geosciences*, and at a time of vocal and uncivil public scepticism towards climate science and dendroclimatology is I think, an additional reason why dendroclimatologists regard Mann's hypothesis as uncivil. Basically, Mann's sceptical display in a mainstream scientific journal is seen by dendroclimatologists as providing grounds for scepticism about dendroclimatology among potentially more inclined uncivil sceptics. My sense is that the dendroclimatologist I talked to in Aviemore would still think of Mann's hypothesis as "offensive", but perhaps to a lesser extent, if Mann had published Mann et al. (2012) in a dendrochronology journal. Likewise, Malcolm Hughes perhaps would have not thought of Mann's hypothesis as "offensive" if Mann et al. had been reviewed by a dendrochronologist or as Rob expressed in *Bishop Hill*, "if Mann had taken the trouble to speak to some of his dendrochronological colleagues".

By addressing his scepticism to an audience beyond the dendroclimatology and dendrochronology community, Mann is seen by a few of those members to be betraying their trust and potentially undermining their shared fiduciary framework constituted on the basis of these trust relations. It is this sense of betrayal that might have led a few dendroclimatologists to regard Mann's scepticism as uncivil. Note that the dendroclimatologists' reaction is significantly different from how they react towards Tingley's overestimation hypothesis. As Rob explains to me, his reasons to be indifferent to the overestimation hypothesis are mostly related to the fact that Rob and other dendroclimatologists consider this hypothesis to be theoretically plausible and less of a challenge to the fiduciary framework of dendroclimatology. The fact that the Tingley study was published in a more restricted geosciences journal and that Tingley and the other co-authors did not have a record of relations of trust with dendroclimatologists (as represented in the inexistence of co-authorships) might also explain the different reaction. Unlike with Mann's hypothesis, the overestimation hypothesis is not perceived as a betrayal or uncivil scepticism by dendroclimatologists because trust relations did not exist and hence could not be broken in the first place.

The fact that dendroclimatologists regarded both the content of Mann's scepticism and the way it was aired as uncivil, explains the way they responded by making their own sceptical appraisal and reaffirmation of their own sceptical practices and work public. The "community response" and the "revisionist studies" of dendroclimatologists exemplify how participants in a fiduciary framework conduct civil scepticism: by cross-checking the dating of DWJ2006 and many other tree-ring based reconstructions, by testing the Scottish one, against independent methods such as carbon dating and simulated temperatures; by testing the ability of the VS-Lite model in simulating DWJ2006; and by quantifying the percentage of missing rings in all the datasets in the ITRDB. Hypothetically, if Mann had raised his concerns solely within dendroclimatological circles of communication, he and the other dendroclimatologists could have together pursued the type of civil sceptical investigation that dendroclimatologists conducted in their papers.

The examples of civil scepticism represented by the "community response" and "revisionist studies" not only depend upon but also eventually serve to strengthen the fiduciary framework of dendroclimatology. The 23 signatories of the community response trust that the VS-Lite experts have subjected this model to appropriate scepticism and "know what they are talking about", as Miloš puts it. Importantly, Rob's role as a spokesman of the community response implicitly indicates that the signatories entrust Rob - as an individual who has been appropriately sceptical and trustful about his previous and current dendroclimatology work, including the DWJ2006 reconstruction - to represent the discipline against the sceptical challenge. This trust is based on the pre-existing relations that Rob established with many of the signatories while he was a doctoral student as well as in his current project of creating the Scottish reconstructions. Overall, dendroclimatologists have relied on these existing trust relations among themselves to assimilate Mann's sceptical challenge to their own practices of organised scepticism and to conclude that cross-dating and climate reconstructions are even more robust and trustworthy than they previously supposed.

The "community response" and "revisionist" studies are also examples of sceptical display in the sense that dendroclimatologists seek to show their reasoned empirical refutation of Mann's evidence and to defend their sceptical identity to different audiences. Accordingly, dendroclimatologists choose to publish their results in specialised journals aimed at gaining the trust from their own dendrochronology colleagues and members of other communities of practitioners like volcanologists and geophysicists whose trust in dendroclimatology might have been damaged by Mann et al (2012). Alternatively, Büntgen et al. (2014) choose to publish what they consider to be the most conclusive piece of evidence from carbon dating in a mainstream generalist journal (*Nature Climate Change*).

Rob's invitation to Montford to attend a lecture in which he criticises Mann's hypothesis is an example of scepticism-as-an-account that becomes co-opted and reproduced by people whom Rob regards as uncivil sceptics. In turn, this form of amplified sceptical display results into different realignments of trust relations. Rob himself is worried that Mann and other colleagues might regard his scepticism-as-an-account as a form of uncivil scepticism. Effectively, Rob's "crock of xxx" comments published in Bishop Hill is the reason for the breaking of trust relations between Rob and Mann, possibly because Mann thinks of Rob's behaviour as uncivil scepticism and giving ammunition to uncivil sceptics like the cartoonist Josh. As a result, Mann regards and labels Rob a "climate denier". In turn, Mann's labelling triggers a reaction from Rob's colleagues who describe Rob as a "thoughtful researcher" and sanction Mann's accusation. Those who trust Rob regard Mann's scepticism towards Rob as another reason for distrusting Mann. Within the context of this controversy, Rob's progressive disengagement from Mann as someone he no longer regards as providing useful civil criticism and contributing to the fiduciary framework is the result of a combination of intellectual criticism of the content of Mann's evidence ("crock of xxx" statement") but also an affective detachment towards Mann as a person.

As a result of the controversy, a few dendroclimatologists – perhaps at least those that signed the "community response" - moved away from trusting Mann as a relatively competent producer of dendroclimatological knowledge to distrusting him as an uncivil sceptic who does not scrutinise dendroclimatological knowledge appropriately.[283] In order to safeguard their fiduciary framework and knowledge, dendroclimatologists exclude Mann – perhaps only temporally - from the community of people who are trusted as producers of dendroclimatological knowledge. Altogether, the intellectual debate over the hypothesis of missing tree-ring develops in parallel to the redrawing of the social alignments and trust relations between participants that determine the temporal membership in the trusting community of dendroclimatology.

[283] Importantly, the fact that a dendroclimatologist like Malcolm Hughes is part of the community response and might consider Mann's hypothesis to be uncivil scepticism and a sign of Mann's incompetence as a producer of dendroclimatological knowledge does not imply to say that he does not think of Mann as a friend and a competent scientist in other areas. In a private correspondence I exchange with Malcolm Hughes, he insists "It is important to me that you should know that I consider Mike Mann to be a personal friend and valued colleague. From time to time I get annoyed with almost as many colleagues as I have annoyed myself. There has been no rupture between Mike Mann and me, rather there has been a divergence of our research foci in the last few years."

8 Conclusions

In the preceding chapters I described the connections between the epistemological and the sociological narratives at each stage of the production of dendroclimatological knowledge. I have shown that Rob and Miloš faced different epistemological conundrums throughout the creation of a climate reconstruction, which they partly and temporarily resolved by building up and managing trust relations that allowed them to conduct appropriate and civil forms of scientific scepticism.

In the fieldwork chapter, Rob and the members of the Scottish Pine Project expedition resolved the challenge of producing objective samples that can be used for dendroclimatology by appropriating the practice and "principle" of site selection, which has been constituted as an explicit component of the fiduciary framework of dendroclimatology, and by relying on the expertise of colleague fieldworkers. Through different exercises of civil scepticism and observations of each other's work in the field, fieldworkers mutually recognised their competence in producing quality samples and constituted an intimate community of expert producers of samples. In the chapter on tree-ring chronologies, Rob and Miloš were able to produce tree-ring data that could be accepted by colleagues as properly dated and tested for the existence of missing and false rings by simultaneously trusting and subjecting to scepticism the method and the people who carried out the cross-dating. On the one hand, Rob and Miloš employed the "principle" of cross-dating as a foundational method of dendrochronology and the fiduciary framework upon which they can expect to find pattern-matching and create ever longer tree-ring chronologies. On the other hand, Rob and Miloš consistently scrutinised their work and that of neophytes like me in interpreting, representing and comparing tree-rings. In the chapter on tree-ring parameters, Rob built up agreement among fellow dendroclimatologists that Blue Intensity is a credible and complementary method for generating climate data from trees by enrolling a community of trusted students and colleagues who conducted sceptical experiments and scrutinised each other's work in workshops, conferences and peer review of articles. In the standardisation chapter, Rob and Miloš resolved the difficulty of ascertaining whether the methods for identifying and eliminating non-climatic noise in the Scottish dataset produced an accurate historical representation of the climate signal by inviting trusted others to examine sceptically the various forms of standardised data they had produced with the "ensemble approach". In turn, Rob and Miloš' sceptical strategy of "cleaning and showing" the data was enabled by a network of senior colleagues and students like Leah, Miloš, Chloe, Dan Druckenbrod and Kevin Anchukaitis whom Rob entrusted to collaborate on the basis of an agreed division of cognitive labour (noise/signal) sustained by Cook's conceptual model of tree growth, which is a constituent part of the fiduciary framework of dendroclimatology. In the reconstruction chapter, Rob and Miloš resolved the uncertainty of interpreting finite extrapolations of past climate by relying on trusted colleagues like Hughes, Cook and the authors of European and British reconstructions and temperature datasets whose work allowed Rob and Miloš to constrain and verify nature's selection of the "best" reconstruction. More generally, their work of reconstruction was enabled by different trust relations that justified their temporary suspensions of scepticism about the limits of uniformitarianism, the temperature datasets and the CCT chronologies. Finally, Rob was able to address Mann's sceptical challenge and to perform sceptical re-examinations of dendroclimatological work by mobilising a large group of reputed and trustworthy dendroclimatologists.

In this last chapter, I reflect upon the trajectories of the broad epistemic and sociological narratives. Do we observe any pattern in and between the production of dendroclimatological knowledge and the way trust relations upon which scepticism is parasitic are established and sustained throughout this process? My answer to this question and the first conclusion of this thesis is what I call the parallel "externalisation" of dendroclimatological knowledge and trust relations. What is the wider significance of these overall patterns, particularly in relation to the central argument of this thesis about the dependency of scepticism on trust? My answer to this question focuses on the distinction between civil and uncivil scepticism. The second conclusion of this thesis is that what counts as civil or uncivil scepticism is a function of one's "embeddedness" and positioning in an externalised network of trust, particularly those trust relations that the sceptic has with the members of the core-set in dendroclimatology. I develop each of the conclusions below.

8.1. The "Externalisation" of Dendroclimatological Knowledge and Trust

To develop my argument about the patterns between the epistemic and sociological narratives, I employ a theoretical framework put forward by the sociologist Trevor Pinch as a starting point.[284] Pinch suggests that the credibility of scientific claims can be characterised by its *externality* and *evidential context*.[285] Externality refers to the degree of abstraction and distancing of a scientific claim from the immediate empirical world and "sense experience" (in Pinch's words) that the claim is taken to represent. The evidential context refers to the multiplication of contexts in which a scientific claim becomes relevant. Trevor Pinch's empirical work is on the study of solar neutrino, and specifically, the work of a group of physicists in providing evidence that stars like the Sun consist of nuclear explosions as predicted by the "conventional theory" in nuclear physics.[286] According to Pinch's account, these physicists placed a tank of cleaning fluid 5,000 feet below the Earth, and through experimental and representational work, aimed to record the interactions of 37Ar atoms as a surrogate for the observation of solar neutrinos to which they did not have direct access.

On the basis of their experiments, Pinch's scientists could formulate claims of different credibility by moving up the axes of externality and evidential context. Scientists could claim to have observed "splodges" of atoms on a graph (a claim of low externality) - which is fairly indisputable and irrelevant claim (with low evidential context) - or that they had observed 37Ar atoms (a claim of medium externality) - which is a more disputable and relevant claim (with medium evidential context) - or, finally, that they had observed solar neutrinos (a claim of high externality) - a very disputable and relevant claim (with high evidential context).

Pinch interprets controversies in nuclear physics in terms of their "damage" to the network of background assumptions sustaining externality claims. In the solar neutrino example, if the conventional theory is challenged or "damaged", the physicists' claim that the observation of 37Ar proves the existence of solar neutrinos will not be accepted as credible by relevant

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[284] I thank my supervisor Professor Steve Sturdy for this suggestion.

[285] Trevor Pinch, "Towards an analysis of scientific observation: The Externality and Evidential Significance of Observational Reports in Physics", *Social Studies of Science*, 1985, Vol.15 (1), pp.3-36.

[286] Trevor Pinch, *Confronting Nature: the Sociology of Solar-Neutrino Detection*, (Dordrecht, Holland; Boston: D. Reidel Pub. Co Higham, Mass, 1986).

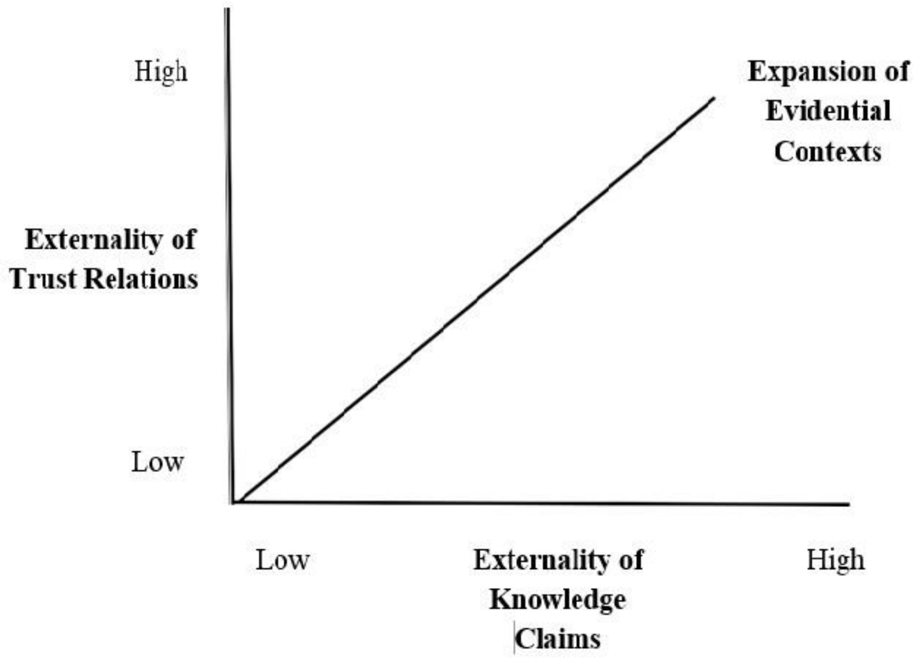
colleagues and, according to Pinch, the “chain of inferences will be broken”.[287] Pinch also argues that nuclear physicists face a dilemma between formulating claims of low externality that are likely to suffer less damage but are of less significance and formulating riskier high externality claims that bring more scientific and social recognition.

Overall, Pinch’s framework is extremely useful for conducting sociology of scientific knowledge as it links the content of knowledge, in terms of its degree of *abstraction* and *relevance*, to the economy of risk and rewards of a specific society and scientific community. In the case of dendroclimatological knowledge, I would argue that Pinch’s framework is valuable because it helps to understand the simultaneous evolution of the epistemological and sociological narratives.

Drawing on Pinch’s schema of the externalisation of scientific knowledge and observations, I have identified a pattern between the epistemological and sociological narratives: as dendroclimatological knowledge becomes less abstract and more relevant, and thus more likely to be disputed by others, the trust relations upon which Rob and Miloš’ scepticism is dependent become increasingly more “external” to the intimate and close group of fieldworkers participating in the early stages of production of knowledge. Another way of thinking about this relationship is to say that the evidential contexts are also social contexts, so the expansion or multiplication of evidential contexts means the expansion of the network of social relations. I summarise my conclusion with the descriptive figure below (image 38) where the *x* and *y* axis represent the externalisation (in Pinch’s terms) of trust relations upon which scepticism is parasitic. As I see it, the expansion of the evidential contexts in which a knowledge claim becomes relevant (as represented by the *z* axis) explains the relationship between the simultaneous externalisation of dendroclimatological knowledge and trust.

[287] Pinch, "Towards an analysis of scientific observation”, p.16.

Image 39. The Externalisation of Dendroclimatological Knowledge and Trust Relations.



The epistemic narrative could be seen as one of an increased externality of dendroclimatological knowledge; from the immediate observational work carried out by Rob and his team in the field site and in the laboratory with wood samples to the statistical work and highly abstract and mediated reconstruction of past climate at the later stages of work where "sense experience" arguably plays a less important role. The chronology of dendroclimatological work I have described in this thesis could be regarded as a chain of progressive abstractions, each level supported by a network of tested assumptions that are part of the fiduciary framework of dendroclimatology. For example, wood samples are taken to represent the climate of the larger natural world from where they are extracted on the basis of the acceptance of the sampling principle of site selection. Likewise, measurements of tree-ring parameters are taken as indirect and complementary estimates of climate on the basis of the law of limiting factors and knowledge about tree physiology and the effect of climate on tree-growth. Finally, standardised tree-ring chronologies are taken as historical reconstructions of climate on the basis of the principle of uniformitarianism.

As Pinch suggests, controversies like the hypothesis of missing tree-rings advanced by Mann could be interpreted as damaging the auxiliary assumptions that support these levels of externality. From the perspective of many dendroclimatologists, Mann's suggestion of the existence of widespread missing tree-rings in chronologies represents a challenge, at least, to the principle of cross-dating that sustains the practice of dendrochronology and the use of ring-width chronologies as historical estimates of climate. Others have formulated damaging claims at other levels of the chain of abstraction supporting the making of dendroclimatological knowledge. For example, some critics argue that the sampling strategy in dendroclimatology is "biased"[288]. Other critics claim that the standardisation techniques and the methods like Principal Component Analysis that dendroclimatologists use to reduce the tree-ring data used for the reconstruction to "inflate" temperature estimates and produce unrealistic global warmings[289]. Finally, others suggest that the fact that some ring-width and density chronologies "diverge" from temperature records in the present period indicate that tree-ring chronologies cannot be taken as reliable "thermometers" of past climate.[290] Taken altogether, these criticisms could potentially be destructive for dendroclimatology, and paraphrasing Pinch, could break the chain of inferences that leads to the creation of climate reconstructions. Yet, this thesis shows that this is not the case, and that dendroclimatologists like Rob and Miloš are generating reconstructions. Like the Azande community, which resists all refutations from the European colonialists against poison-oracle (Chapter 1), the fiduciary framework of dendroclimatology has been immune to radical refutations so far.

Like in the controversy with Mann, dendroclimatologists have assimilated critics' challenges with different *ad hoc* adjustments and sceptical re-examinations of their work that

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[288] Whatsupwiththat, "Oh Mann! Paper demonstrates that tree-ring proxy temperature data is 'seriously compromised'" http://wattsupwiththat.com/2013/08/16/oh-mann-paper-demonstrates-that-tree-ring-proxy-temperature-data-is-seriously-compromised/

[289] Steve McIntyre and Ross McKitrick, "Hockey Sticks, principal components and spurious significance", *Geophysical Research Letters*, 2005, Vol. 32 (3); "Corrections to the Mann et al. (1998) Proxy Data Base and Northern Hemisphere Average Temperature Series, *Energy and Environment*, 2003, Vol. 14 (6).

[290] *Climate Audit*, "Yamal: A 'Divergence' Problem", 27 September 2009, accessed 15 July 2015, http://climateaudit.org/2009/09/27/yamal-a-divergence-problem/

have allowed them to continue making dendroclimatological knowledge. For instance, they are examining the effects of a “Modern Sample Bias” and potential solutions. They are also developing new standardisation techniques like ‘Signal Free’ and are devoting resources and time to research the “divergence problem”. In the case of the distinctive epistemological conundrum that Rob and Miloš faced in Scotland regarding the presence of disturbance in the dataset, they used different standardisation methods that allowed them to produce dendroclimatological knowledge. Dendroclimatologists like Rob and Miloš justify the “tweaks” to their tree-ring data on the basis of one unshakeable belief: the growth of trees is a response to their surroundings and the environmental conditions in which they are located, and therefore, trees contain a climate signal that dendroclimatologists should strive to extract in order to generate climate reconstructions. Rob and Miloš often express this dogma in the language and practice of “maximisation” of the climate signal and the climatic information from trees. For instance, when Rob and his team practiced site selection with the limited number of Scots Pine woodlands and potentially relevant lakes in the Scottish Highlands. When Miloš and I cross-dated, averaged and excluded a few tree-ring chronologies with the purpose of “strengthening” the existing climate signal. Likewise, when Rob and Miloš combined blue intensity and ring width data into the “band-pass approach” with the aim of making the most of the climate signal given by different parameters; and finally when Rob and Miloš selected July and August temperature as the months of the reconstruction, when they employed the PCA chronologies and accepted the “best” outcome from the ensemble and combo approach.

As I infer from Pinch’s framework, the progressive externalisation of dendroclimatological knowledge and its incorporation and use in a greater number of evidential contexts could be driven by Rob and Miloš’ expectation of higher rewards. In theory, as dendrochronologists, Rob and Miloš could have been satisfied with the creation of an extended tree-ring chronology for Scotland. Yet, they are not happy with “just” producing tree-ring chronologies. The rewards in terms of scientific credibility, influence and future funding are higher if Rob and Miloš produce a climate reconstruction.[291] I do not mean to say that the pursuit

[291] The existence of an unequal distribution of rewards across different areas of dendrochronology could explain the tensions I noticed between dendroclimatologists, dendroecologists and dendroarchaeologists. In Chapters 3 and 5, I mention that a few dendroarchaeologists and dendroecologists expressed some resentment to me about the fact that dendroclimatology has become the predominant “application” of dendrochronology. One dendrochronologist complained to me that “now it seems that chronologies are

of such rewards is the main reason why Rob and Miloš' personal motivation in producing climate reconstructions. Instead, my argument is that Rob and Miloš' research interest in producing dendroclimatological knowledge is part of an institutional reward system (participated in by Rob, Miloš and many others) that favours such intellectual pursuits.

The reward structure in dendroclimatology is part of the wider social system of professional merits in the UK. These incentives are contained, among others, in the criteria that the UK government employs for funding scientific projects like the Scottish Pine Project. In a conversation with Rob in April 2015, Rob tells me that he has decided not to apply for funding next year once the current project ends in April 2016 and "to let things lay fallow for about a year". The reasons that Rob gives for suspending the Scottish Pine Project temporally and not extending the 800-year long temperature reconstruction further back in time are diverse, including the fact that Rob and his team might have already sampled all the existing sites in the Scottish Highlands with Scots pine trees. Rob also mentions the grading criteria of the "Research Excellence Framework" (REF), which is the method used by the British government to assess the "quality" of publicly-funded research in the UK and to allocate public funding, as a minor disincentive to continue with the Scottish Project. Rob complains that the REF awards "only" one or two stars to "regional" and "national" work like the Scottish temperature reconstruction in comparison to the three or four stars potentially awarded to "international" or "world-leading work". At the time I have this conversation with Rob, he has just returned from a conference where he has presented his current research project about on an "updated" global temperature reconstruction. This new project called "N-TREND" could bring Rob more rewards, but also more risks and conflicts with others as a global reconstruction could be considered another level of externality of dendroclimatological knowledge (from "regional" to "global" reconstructions) and of evidential context.

The end point of the making of dendroclimatological knowledge of Scotland is the creation of a few graphs (p.47 in this thesis) that dendroclimatologists and others will deploy as an *object* of representation of past climates and mobilise for their own causes in multiple evidential contexts. The "objectification" of climate starts with the creation of material wooden samples and continues with the transformation of these samples into scanned images,

worth nothing unless you use them for climate reconstructions". This comment expresses an awareness of the different allocation of rewards and authority to professional groups within dendrochronology.

measurements and graphs through different mediating and representational laboratory technologies and practices that the philosopher and STS scholar Bruno Latour refers to as "inscription devices"[292]. Latour also talks about scientific texts and representations such as climate reconstruction graphs as "immutable objects" [293] that allegedly travel and remain unchangeable when they are used by people in different contexts. Over the last decades, millennium climate reconstructions have travelled a long way from scientific articles and policy reports like those of the IPCC; along the way, these graphs have changed and become progressively more quantitative.[294]

Rob and Miloš have strived to create an objective and widely acceptable climate reconstruction of Scotland by presenting this knowledge as the result of their empirically-grounded and subjective expertise developed with others in the field and in the laboratory. Specifically, Rob and Miloš mobilise their personal knowledge in a process that Bruno Latour refers to as "circulating reference" (the word "reference" comes from the Latin "referre", which means "to bring back"). [295] Specifically, they referred to their knowledge about the field sites in the Scottish Highlands and the material features of wood samples to make interpretations of unknown features of the data at later stages. Similarly, when Rob and Miloš found out that cross-dating was impossible with most series of measurements from the Alladale site and that the data showed consistent "spikes" around the period of the Napoleonic wars, they relied on their second-hand knowledge of the area (through the Scottish historians' account) and the suspicion that these anomalies were related to historical logging. Also when they discovered the anomalies in the reconstructed temperatures in the West, they referred to their experience of the field site and their previous difficulties with cleaning the datasets of disturbance. The controversy with Michael Mann also reveals the importance that dendroclimatologists give to this background knowledge. In the opinion of dendroclimatologists, the fact that Mann does not have any

[292] Latour and Woolgar, *Laboratory Life*, p. 51.

[293] Bruno Latour, *Science in Action: How to Follow Scientists and Engineers through Society*. (Cambridge, MA: Harvard University Press, 1987), p.227.

[294] For a history of the evolution of millennium climate reconstruction as told by dendroclimatologists themselves, including Rob, read Frank, David et al. "A Noodle, Hockey Stick, and Spaghetti Plate: a Perspective on High Resolution Paleoclimatology", *Wiley Interdisciplinary Reviews: Climate Change*, 2010, Vol.1(4), pp.507-516. For an explanation of the changing nature of paleoclimatological knowledge more generally in relation to the evolution of its scientific and social uses in relation to climate models read David Demeritt, "The Construction of Global Warming and the Politics of Science." *Annels of the Association of American Geographers*, 2001, Vol. 91.(2), p.315.

[295] Latour, "Circulating Reference" in *Pandora's Hope*, pp. 24-80.

experience in the field or in the laboratory explains that he formulated an "unrealistic" hypothesis. The strategy of invoking the knowledge of the field site as a means to contextualise subsequent abstract knowledge seems to be characteristic of fieldwork-based sciences more generally.[296]

Rob and Miloš can mobilise their personal expertise because their community shares their understanding of what constitutes "evidence" and what does not. Note that when I ask Rob to articulate the importance of participating in fieldwork to conducting dendroclimatology, he responds in the first personal plural, "We've been in the sites, we've done the data, and we know them so well". Rob establishes a connection between the verbs "being", "doing" and "knowing" through the existence of the collective "we". The collectively constituted personal knowledge of the physiological, ecological and preparatory conditions of tree-ring data is therefore the basis upon which dendroclimatologists secure the credibility of their extrapolations.

Overall, my conclusion about the epistemic narrative is that dendroclimatological knowledge is externalised into an object of progressively wider scientific and social relevance in parallel with the *inter-subjectification* of dendroclimatological knowledge. This inter-subjectification means that Rob and Miloš consistently refer the epistemological object they have created (the temperature reconstructions graphs) to their personal knowledge of the field conditions and the laboratory work generated in interactions with others. The *inter*-subjective nature of this community-sanctioned expertise is precisely what makes dendroclimatological knowledge objective.

Whilst the epistemic narrative becomes increasingly more abstract and socially relevant, the narrative of trust also becomes more "external" in the sense that it moves from the face-to-face management of trusting relationships within Rob's intimate circle of fieldworkers to a larger circle of trust relations with colleagues whom Rob knows more tenuously. The multiplication of

[296] In their study of petroleum geophysicists, Petter Almklov and Vidar Hepsø argue that these scientists use their field trips to offshore reservoirs to create "analogues" that inform the interpretation of remote data sources back in the office. The authors ask the very pertinent question of "What comes back?" when geologists leave the field site, to which Almklov and Hepsø give an answer that could apply to dendroclimatology too: "The data are the objects of attention, both on field trips and in the office, but the answer to the question of what comes back from the logging trips is not primarily a set of immutable mobiles (Latour, 1987), but instead a group of professionals with an increased understanding of the context from which geological data are extracted". "Between and Beyond Data: How Analogue Field Experience Informs the Interpretation of Remote Data Sources in Petroleum Reservoir Geology", *Social Studies of Science.*

trust relationships might be considered a direct corollary of what Pinch regards as the multiplication of evidential contexts.[297]

In the fieldwork chapter, the production of credible samples is rooted in a set of very intimate relationships among fieldworkers. Trust relationships between expert fieldworkers were first constituted elsewhere (Rob and Björn met on a European project, and Miloš was Rob's undergraduate student and technician before becoming a PhD student), and these relations have been reinforced in the field. Fieldwork instructions are an opportunity for new close relations to emerge between expert and students or amateurs like myself. The production of a well dated long tree-ring chronology is also dependent on trust relations between closely acquainted teachers and students. Miloš was the expert who scrutinised my work and that of other students and, his cross-dating work with fossil samples was in turn scrutinised by his supervisor. Likewise, Rob tested the Scottish long chronology against the Scandinavian chronology created by his fieldwork Swedish colleague Björn.

The chapter about the development of Blue Intensity and a new method for generating climatic data that is potentially able to move from one laboratory to another marks the beginning of a shift outwards from the immediate and relatively intimate setting of the field sites and laboratory. Rob first draws upon these intimate trust relations with Miloš and other fieldworkers to conduct sceptical experiments with Blue Intensity. He builds upon these close relations (including with me) to constitute a larger community of less familiar researchers like Jesper, Andrzej and the Tasmanian, Canadian and Argentinian researchers through interactions at workshops and conferences. For the work of standardisation, Rob liaises between members of the intimate group of fieldworkers (Leah, Chloe and Miloš) and outsiders to the Scottish Pine Project that Rob knows less intimately (the Scottish historians, Dan, Kevin Anchukaitis and Edward Cook) in order to create a larger group of collaborators that assist Rob in establishing a credible solution to the problem of disturbance in the Scottish dataset. Furthermore, in order to enable nature's choice of the 'best' reconstruction, Rob and Miloš rely on the work of trusted experts such as Phil Jones' temperature datasets, Malcolm Hughes' pioneering work in Scotland, and Edward Cook's method of spatial reconstruction. Rob and

[297] Pinch himself seems to acknowledge this idea when he says, "The data base for lower-level reports is not usually publicly available. This means that factors such as trust, and the personal relationships that exist between different experimenters and theorists, play an ever-increasing role the more externalised observational reports become". "Towards an Analysis of Scientific Observation", p. 27.

Miloš also evaluate the accuracy of nature's selection of a long-term temperature reconstruction for Scotland by comparing it against the climate reconstructions created by paleoclimatologists whom they know only by name. In the controversy over Mann's hypothesis, Rob mobilises a broad cross-section of the dendroclimatology community against the challenge raised by Mann. At this last epistemic stage, the relations of trust become as "external" as they can possibly be, since the work of securing trust in climate reconstructions is not only the business of a single individual like Rob and his intimate circle of collaborators; it expands to the entire dendroclimatology and dendrochronology community.

Overall, the community of dendroclimatologists seems to be constituted by individuals who have known each other personally for a long time. When I asked Miloš by email how he would define Rob, he emphasised the fact that "Rob seems to know just about everyone in our research community". Rob told me that field weeks like the one I attended in Tasmania are crucial places where he first got to know colleagues from around the world. On my first day at one of these field weeks, I already appreciated the critical role of these training courses in allowing the development of personal relations between dendroclimatologists. After our first dinner together, attendees of the field week gathered in a room that we used as a laboratory during the day and as a bar during the night. Conversations started with senior researchers asking students (not the other way around) their names, the laboratory or university at which the student was based and the species of tree that the student was working on. The senior researchers' responses followed a consistent structure: "Well, if you are based in [the name of a city/university], you must know [the name of a person], don't you? We collaborated years ago" or "Is [the name of a person] your supervisor? Say hi to from me, I haven't seen her/him for ages!" Conversations between junior researchers included similar questions, and often said to each other "I know your supervisor because I read this paper" or "I think our supervisors worked together". In this way, field week participants made use of their own structures of familiarity in order to classify unfamiliar people. In ever growing communities of specialised knowledge like dendroclimatology, familiarity is a "shortcut" for establishing trust relations and interaction between unknown individuals.

I would argue that *recommendation* is the specific resource of familiarity that Rob and Miloš have employed in order to expand circles of trust relations on the basis of familiar ones. In the fieldwork chapter, Hans and Emily joined the expedition through Björn. It becomes more obvious from the chapter on Blue Intensity onwards that Rob and Miloš start trusting people who have been *recommended* by familiar colleagues. For instance, Jesper's participation in the Blue Intensity experiments resulted indirectly from Hans and Björn being his doctoral supervisor. Also, the participation of the Argentinian researcher in the Blue Intensity experiments partly derived from me liaising between Antonio and Rob during a road trip in Australia. Edward Cook had a crucial role as a mediator in terms of suggesting Dan and the CCT method to Rob, in instructing Miloš in the use of his method for spatial reconstruction and in spreading the "lite" version of Signal Free. Rob' arrangement of a laboratory visit for Miloš to meet Kevin Anchukaitis is also a form of sponsorship.

Recommendation also takes place in cases where it seems that there is no one to trust personally, for example with technologies such as carbon dating, scanner, calibration cards or abstract principles and laws like uniformitarianism and limiting factors. In such cases, it was quite difficult and often impossible for Rob and Miloš to trace the "source" of recommendation when I asked them to do so. Miloš recalls first hearing about uniformitarianism *from Rob* during his undergraduate lectures and reading about the law of limiting factors *in textbooks*. Rob decided to use a specific laboratory that specialises in producing carbon because this laboratory is the one sponsored *by the public research council in the UK that funds natural sciences (NERC*). Rob bought a certain calibration card because he had read *in one dendroclimatology article* that this was the standard card used in previous experiments. All these more or less personal sources of recommendation of anonymous expert systems are what the sociologist Anthony Giddens refers to as "access points" to anonymous systems of expertise. [298] By the time Rob and Miloš get to the more abstract stage of creating the climate reconstruction, the network of trust relations extends well beyond the community of dendroclimatology to include trust in anonymous scientists and their technologies and respective communities.

Overall, recommendation is a mechanism for the construction and extension of trust networks and circles of trust based on reputation for trustworthiness and authority.

[298] In our everyday lives, while commuting, shopping and paying our bills, we trust expert strangers - the bus driver, the shop assistant and the banker - to conduct their job properly. According to Giddens, the trust we grant (or do not grant) to the abstract institutions such as transportation, food and banking is dependent upon our trust and interaction in these expert people or "access points" that represent the institution. In Giddens, *Consequences of Modernity*, (Cambridge: Polity Press, 1990),p 83. I would argue that the work of science is no different, and scientists from different disciplines need different access points to navigate across unfamiliar and abstract expertises.

Recommendation can take the form of "word-of-mouth" and informal oral communications, or more formal written expressions like "letters of recommendation", sponsorship for laboratory visits and "suggestions for further research" in articles.[299] The role of authority is crucial in determining whose recommendations are themselves worth trusting.

The "community response" to Mann's sceptical challenge illustrates the critical role of familiarity and recommendation in the constitution of large networks of trust relations. An analysis of the co-authorship of the community response unveils the close ties and mutual endorsement between dendroclimatologists (see Appendix 1). 16 out of the 23 signatories have written his/her most highly cited paper with another signatory of the community response. Rob, for instance, has co-authored papers with 12 signatories out of the total 23, and four of them are directly involved in the Scottish Pine Project (Kevin Anchukaitis, Edward R. Cook, Björn Gunnarson and Malcolm K. Hughes).

The names of 11 researchers appear repeatedly across co-authorships of the "community response" (Edward Cook, Rosanne D'Arrigo, David Frank, Eugene Vaganov, Jan Esper, Kevin Anchukaitis, Ulf Büntgen, Malcolm Hughes, Keith Briffa; Valerie Trouet and Fritz Schweingruber), which might be an indication of the existence of what the sociologist Harry Collins calls "core-sets" in science.[300] According to Collins, core-sets are small groups of specialist scientists present in most scientific disciplines, which function on the basis of trust, familiarity and face-to-face relationships sustained in conferences (or fieldweeks) and online media. Collins insists that members of core-sets do not necessarily need to be friends or close collaborators; they can be "enemies" in the sense that they disagree on substantial issues.

What brings the members of a core-set like the signatories of the community response together is the eventual resolution of a given scientific controversy on the basis of shared criteria for evaluating evidence (or in the language of this thesis, practicing scepticism civilly). Collins explicitly warns that citation patterns cannot delimitate the diffuse and changing boundaries of a core-set. For instance, the fact that Mann is listed as a co-author of one of the signatories' most

[299] In this sense, the mechanisms of recommendation could be similar to what Mark Granovetter refers to as "The Strength of Weak Ties" or "acquaintances" (as opposed to "close friends") that have a positive impact on diffusion of influence, information, social mobility and social action. *American Journal of Sociology*, 1973, Vol. 78, 6, pp. 1360-1380.

[300] Harry Collins, "The Place of the Core-Set in Modern Science: Social Contingency with Methodological Propriety in Science in Innovation and Continuity in Science.", *History of Science,* 1981, *Vol.*19.(1), pp. 6-19.

cited papers (Hughes[301]) is an indication of the changing membership of core-sets; Mann might have once been a member of the core-set, but after the controversy, he is not considered to be so anymore. According to Collins, the best way to find out who is a member of the core-set is to ask those identified by sociometric analysis who else they think has made a contribution to the controversy.[302] Rob - even if his name does not emerge in the analysis of citations - was the main person that a few of the most cited dendroclimatology authors I talked to mentioned as responsible for being the corresponding author. Interestingly, when I asked Rob who else had contributed to the debate, he mentioned Andrew Montford and people who wrote comments on the Bishop Hill blog and in Twitter. With his response, Rob effectively extended the boundaries of the core-set of dendroclimatology to outsiders and to people who lack the personal knowledge that dendroclimatologists develop by virtue of being trained in a community of experts. [303]

Overall, the conclusion of the sociological narrative is that the externalisation of Rob and Miloš' network of trust relationships from the intimate group of fieldworkers and close collaborators shows persistent patterns of familiarity between producers of knowledge. Most people within the dendroclimatology community – including Rob and Miloš – seem to know each other first or second hand through different mechanisms of recommendation that contribute to the progressive enlargement of circles of trust, which in Rob's case includes outsiders to the core-set of dendroclimatology.

My first conclusion summarising the trajectories of both the epistemological and sociological narratives is that in order to make dendroclimatological knowledge that is considered by colleagues and others to be an objective and relevant representation of historical changes of climate, Rob and Miloš build upon the intimate trust relations constituted in fieldwork and mobilise an increasingly larger network of recommended colleagues.

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[301] Michael Mann, Raymond Bradley and Malcolm Hughes, "Northern Hemisphere Temperatures During the Past Millennium: Inferences, Uncertainties, and Limitations", *Geophysical Research Letter*, 1999, Vol. 26 (6), pp. 759-762.

[302] Collins, "The Place of the Core-Set", p. 9.

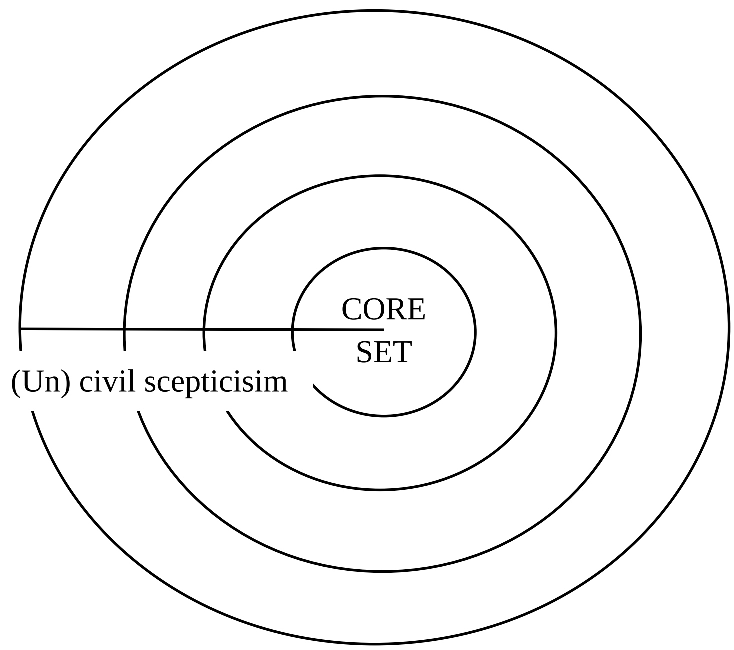
[303] Harry Collins talks about these cases of over-extension rather normatively as "core-set distortions". Collins explains, "One kind of over-extension occurs where inexperienced and untrained outsiders assume, and are widely granted, the right to comment authoritatively on scientific matters". "Public Experiments and Displays of Virtuosity: The Core-Set Revisited." *Social Studies of Science*, 1988, Vol. 18 (4), p. 741.

8.2. (Un) Civil Scepticism as a Function of "Embeddedness"

To develop my second conclusion, I use Mark Granovetter's concept of "embeddedness", which refers to the networks of social relations that constrain and enable particular forms of economic behaviour. Granovetter developed this concept in a paper published in 1985 as a reaction to what he identified as the two existing accounts of economic action: the "undersocialised" explanation, which regards economic life as a separate sphere of modern society resulting from the independent and self-interested calculation of atomised individuals; and the "oversocialised" or functionalist explanation, which regards economic activity as the result of the individual complying with norms and values internalised through socialisation. Granovetter clarifies that "the embeddedness argument stresses instead the role of concrete personal relations and structures (or "networks") of such relations in generating trust and discouraging malfeasance".[304] Analogously, and drawing on my previous conclusion about the extension of Rob and Miloš' network of trust relations, the second conclusion I develop below is that what counts as civil and uncivil scepticism depends upon one's "embeddedness" in a system of trust and one's proximity to the core-set of dendroclimatology. In the image below (Image 40), I represent this network of trust relations as circles of progressively external and unfamiliar relations with members of the core-set of dendroclimatology, and (un)civil scepticism as a continuum across these circles upon which Rob and Miloš have relied to make dendroclimatological knowledge.

[304] Mark Granovetter, "Economic Action and Social Structure: The Problem of Embeddedness", *American Journal of Sociology*, 1985, Vol.91(3), p.490.

Image 40. Continuum of (un)civil scepticism as a function of specific "embeddedness" in circles of trust relations constituting dendroclimatological knowledge.



Source: [305]

Rob is exceptional in the extent to which he is willing to trust external members of the core-set in dendroclimatology. This exceptionality is shown by the fact that Rob's colleagues think of him as slightly "crazy" to be interacting with me and a few "validating and cautious sceptics" like Steve McIntyre and Andrew Montford. When, in Tasmania, I first introduce myself as "studying the work of Dr Rob Wilson and his team", a few attendees seem to be used to Rob engaging with strangers like me. One dendroclimatologist bursts into laughter and says to me, "This is so typical of Rob!" One of Rob's colleagues assures me that dendroclimatologists seem to trust Rob as a mediator with outsiders, as "Rob is kinda our representative in the sceptical world". The colleagues of Rob with whom I talk do not think that Rob's relations of trust with outsiders are dangerous or a reason for mistrusting him. Even Miloš, who regards his supervisor's interactions with "sceptics" as "risky", thinks of Rob's

[305] To produce such this graph, I draw on Harry Collins' representation of the core-set Collins, Harry. "Working paper 140: Inside and Outside Science: Beware of acting too hastily on Climategate". http://www.cf.ac.uk/socsi/resources/wp140.pdf, p.3.

attitude as amusing. Rob is well aware that his colleagues and students think of his behaviour as strange. Rob says that colleagues think of him as "nuts" and describes his engagement with a few "sceptics" as "stressful" and "masochist". The reasons for Rob's exceptional trustful predisposition, I believe, are mostly related to his psychology and generous, curious and affable character, but could also be related to his undergraduate training.[306]

Like me, McIntyre and Montford have been long acquainted with Rob, but their "embeddedness" and positioning in the circles of trust relations upon which Rob has relied to constitute dendroclimatological knowledge is further away from the core-set than me. McIntyre and Montford, unlike me, have not developed the type of personal knowledge I detailed in the previous section and the intimate trust relations associated with the production of this knowledge. My proximity to the core-set and the fact that over time I have become known and trusted by Rob and his trusted students and colleagues is the main difference between me and McIntyre and Montford as peripheral members of the community of dendroclimatology. My "breaching questions" and interventions in dendrochronology conferences could have potentially been seen by Rob and the rest of the dendroclimatology community as examples of uncivil scepticism like those formulated by McIntyre and Montford. Instead, Rob and many dendroclimatologists were keen to recruit me as a dendroclimatologist (they made jokes about me being a "dendro-sociologist", Rob included me in the acknowledgements of his paper and the scientific committee of the dendrochronology conference awarded me with a corer as a welcoming gesture to their community). Whether Montford and McIntyre's scepticism is considered by specific dendroclimatologists to be a civil contribution to the fiduciary framework depends on the nature of their trust relations with Montford and McIntyre. As Montford and McIntyre do not seem to have trust relations with any member of the core-set of dendroclimatology except with Rob, they are regarded by most dendroclimatologists as uncivil sceptics.

[306] I once had a conversation with Dr Sarah Parry in which she offered a more sociological hypothesis for Rob's "exceptional" character. She mentioned a few names of geographers known for their critical thinking and openness to interdisciplinary conversations and collaborations, who share the fact that they have been trained at the same institution (Durham University in the UK).

Rob's motivation for interacting with McIntyre and Montford represents, I would argue, a "mixed" view about the relationship between scientists and members of the public.[307] On one hand, Rob aims to re-educate and "to enlighten" the sceptical public. Rob defines McIntyre and Montford as "validating" and "cautious" sceptics respectively (Chapter 5), because in his view, they are more "open-minded" and susceptible to scientific evidence than other types of sceptics. As a dendroclimatologist, Rob shows a decidedly "science-centred" modernist view of society. He thinks that the best climate science should inform policy-making with regards to dealing with the effects of climate change. Rob does not express any strong conviction or interest in environmental politics, but he is anxious that public scepticism might get it in the way of offering a clear scientific picture of the risks posed by climate change. On the other hand, Rob aims to partake in dialogue with the sceptical public in order to encourage reflection and self-appraisal among dendroclimatologists. Rob does not portray McIntyre and Montford as ignorant, but as being differently knowledgeable of dendroclimatology. Rob explains "Montford cannot do the science and asks someone else to do it for him", whilst McIntyre can replicate reconstructions like the ones he did of Mann's hockey stick.[308] Unlike most members of his community, Rob trusts that outsiders like McIntyre and Montford, for the most part, provide useful civil scepticism and can contribute to make dendroclimatology more accurate.

In many subtle ways, Rob integrates McIntyre and Montford's public scepticism into the self-critical practices of dendroclimatologists in a phenomenon that I refer to as *inside-out scepticism*.[309] The best example of inside-out scepticism occurs when Rob invokes McIntyre or the "sceptics" as an imagined public in conferences and conversations with Miloš, and he uses the outsiders' scepticism as an internal standard for evaluating the evidence provided by colleagues and students. Another example is Rob's civil scepticism of his colleague Jan Esper

[307] This "mixed view" takes as constituent parts the two allegedly divergent approaches to science (the modern "enlightenment" and postmodern "critical") outlined by Alan Irwin in *Citizen Science A Study of People, Expertise and Sustainable Development*, (Hoboken: Taylor and Francis, 2002).

[308] In the language of Harry Collins and Robert Evans, Rob believes that Montford has "interactional expertise" and is able to talk about dendroclimatology whereas McIntyre has "contributory expertise" and is able to contribute to the science. *Rethinking Expertise*, (Chicago: University of Chicago Press, 2008).

[309] I regard "inside-out scepticism" as one empirical example of the notion of "ethno-epistemic assemblage" put forward by Alan Irwin and Mike Michael as the means for an analytical reinterpretation of science–social relations. They say, "Instead of assuming the contrast between science and society, we need new categories and ways of thinking, which, to use our phrase, 'mix things up'". *Science, Social Theory and Public Knowledge* (Maidenhead: Open University Press, 2003), p. xii.

for using his expert judgement in the selection of the “best” reconstruction. As Rob explicitly explains in his conference talk, Rob’s alternative combo approach is motivated by his awareness of the outsiders’ scepticism. In particular, Rob claims to be wearing “the McIntyre’s hat” every time he has to make a decision about what reconstruction variant to choose. Another example of inside-out scepticism and emphatic strategies is Hughes’ devil’s advocate question to Miloš during the Melbourne conference where he invoked the scepticism from statisticians regarding the “problem of multiplicity”. All these examples show how in the interest of making new dendroclimatological knowledge as robust as possible against potentially uncivil scepticism from outsiders, Rob and colleagues consistently practice civil scepticism with colleagues and students.

Rob’s engagement with McIntyre and Montford has also influenced the boundaries of the core-set and the trusting community of dendroclimatology. One example of the changes in the boundaries of the community is shown in the controversy chapter. As a result of Rob’s collaboration with Montford and the publication of Montford’s blog post, Mann broke trust relations with Rob (if these ever existed). The emotional to and fro that occurred on Twitter contributed to the foreclosing of the scientific controversy and the subsequent redrawing of the core-set. From that moment onwards Rob lost any interest in continuing to have a technical conversation with Mann, and many of Rob’s friends and close colleagues considered Mann’s accusation of Rob as being a “climate denier” to be uncivil scepticism and perhaps a reason for expelling Mann from their community of trusted producers of dendroclimatological knowledge. Mann’s temporary exclusion from the narrow circle of trust relations of the core-set in dendroclimatology does not preclude the possibility that Mann becomes trusted again in the future by the core-set members or the fact that Mann is indeed part of larger circles of trust relations that honour his expertise.[310]

The paradox, as I see it, is that McIntyre and Montford are effectively much more involved in the making of dendroclimatological knowledge than Mann is, in the sense that Rob seeks to address himself, or asks his students and colleagues to address, scepticism from trusted

[310] Since the controversy ended, Michael Mann has received multiple awards. In 2012, he was elected a Fellow of the American Geophysical Union and awarded the Hans Oeschger Medal of the European Geosciences Union. He became a new Fellow of the the American Meteorological Society in 2013. In 2013 Mann was also appointed distinguished professor in Penn State's College of Earth and Mineral Sciences change deniers". During the same year, he also received the National Wildlife Federation's National Conservation Achievement Award for Science. On April 2014, the National Center for Science Education awarded its first annual Friend of the Planet to Mann.

civil sceptics such as Montford and McIntyre and yet disregards Mann as an uncivil sceptic after the controversy. This conclusion is paradoxical in the sense that it unsettles common sense and academic definitions of who the "climate sceptics" are.

Individuals participating in the debate about the reality and risks of climate change often apply the label of "climate sceptic" to each other to structure their interactions in terms of roles and status. In this thesis I have shown how scientists themselves - as is the case with Mann's accusation that Rob was a "climate denier" - use these labels to categorise certain forms of scepticism. More generally, social scientists have long concluded that stereotyping is an essential mechanism of social coordination in complex societies. When we stereotype unfamiliar others with labels, we regard these labels to be the result of self-explanatory and undisputable forms of behaviour and personal traits. When we call someone a "thief" we do so because we think he or she has done something clearly unlawful; when we refer to someone as a "woman" we relate this category to undisputable biological traits. Similarly, when Mann refers to Rob as a "climate denier" it is probably because he thinks that Rob has behaved like a climate sceptic would do. Thus, the ordinary usage of labels is based on a "realist" and "reified" understanding of personal identities and labels. That is, labels are commonly seen to describe someone's identity regardless of whoever is using the label and the circumstances of that labelling.

Many scholars studying those who participate in the debate about climate change have erred in using labels in the same realist and reified way as the participants that they were supposed to be studying.[311] These scholars have adopted the participants' label of "climate sceptic" as a taken for granted explanation of behaviour rather than a starting point for analysis. In this way, multiple studies claim to have identified political and moral viewpoints associated with the identity of "climate sceptics". Likewise, other scholars have created taxonomies of climate sceptics and new reified labels such as "climate denier" or "climate contrarian". A few of these scholars have also used these labels derogatively to criticise others.

Most of the scholarly work about labelling in the climate change debate is fundamentally flawed in the sense that it does not appreciate the changes and the "situational adjustment" (using

[311] Howarth and Sharman offer a review of such scholarly work in "Labelling Opinions in the Climate Debate: A Critical Review", *Wiley Interdisciplinary Reviews: Climate Change*, 2015, Vol. 6 (2), pp. 239–254.

Howard Becker's words[312]) involved in the formation of personal identities, including the role of labels in shaping these identities. For scholars within the sociological tradition of the "labelling theory"[313], labels are not descriptive categories of the action and attributes of individuals but rather are performative categories that individuals use to affect each other's identity and that result from the adjustment of one's personality to the demands of others in specific situations.

Similarly, this thesis has shown that the identity of the "climate sceptic" is relative to one's expectations about what constitutes civil scepticism in a specific context, and therefore one's provisional membership in a community of trust. For Rob and the other 23 dendroclimatologists of the community response, Mann's hypothesis represented a challenge to the fiduciary framework that constitutes the basis of their trust relations and work. For them, Mann's identity has evolved and changed from being a trusted colleague with whom to produce iconic dendroclimatological knowledge like the "hockey stick" to be considered an uncivil sceptic that offends his friends and colleagues. Perhaps, if Mann ever demonstrates conformity to the dendroclimatologists' fiduciary framework, he can be regarded again as a civil sceptic. Likewise, Rob's opinion that Montford and McIntyre are trustful civil sceptics is not shared by many of his dendroclimatologists colleagues who have not developed trust relations with them. So again, Montford and McIntyre's identity as climate sceptics is relative to the "eye of the beholder".[314]

My second conclusion, about the importance of "embeddedness" and one's specific trust relations and positioning in increasingly larger circles of trust relations that sustain and qualify the practice of scientific scepticism, demonstrates that the boundaries of the core-set in dendroclimatology are more permeable to the scepticism of outsiders than some of these outsiders themselves perhaps imagine.[315] My hope is that my analysis of the roles of trust and

[312] Howard Becker, "Personal Change in Adult Life", Sociometry, 1964, Vol.27 (1), pp.40-53.

[313] Some of the authors that have contributed to the labelling theory are from the interactionist tradition of sociology. The most important books in developing the labelling theory are George Herbert Mead*'s Mind, Self, and Society: from the Standpoint of a Social Behaviorist* (Chicago, Ill. ; London : The University of Chicago Press ;1934); Howard Becker's *Outsiders: Studies in the Sociology of Deviance, (New York : Free Press of Glencoe, 1963);* and Erving Goffman's *Stigma: Notes on the Management of Spoiled Identity,* (London: Penguin Books, 1968).

[314] Becker, "Personal Change in Adult Life".

[315] Mosher and Fuller criticise that climate scientists are an exclusive group ("The Team") in "We are tough on the scientists we call The Team, and we think deservedly so. But we want to stress from the outset that we do not for one minute believe there is any evidence of a long-term conspiracy to defraud the public about global warming, by the Team or anyone else. What we find evidence of on a much-

scepticism in the production of dendroclimatological knowledge, and my conceptual distinction between civil and uncivil scepticism, has rendered the vernacular category of “climate sceptic” analytically problematic for social analysts and that it encourages others to study empirically the contingent social negotiations about the distribution of trust and the boundaries of trusting communities that determine which scepticism is perceived as civil or uncivil and by whom.

smaller scale is a small group of scientists too close to each other, protecting themselves and their careers, and unintentionally having a dramatic, if unintended effect on a global debate”. in *Climategate: The CRUtape Letters*, (Lexington, KY: CreateSpace, p.9.

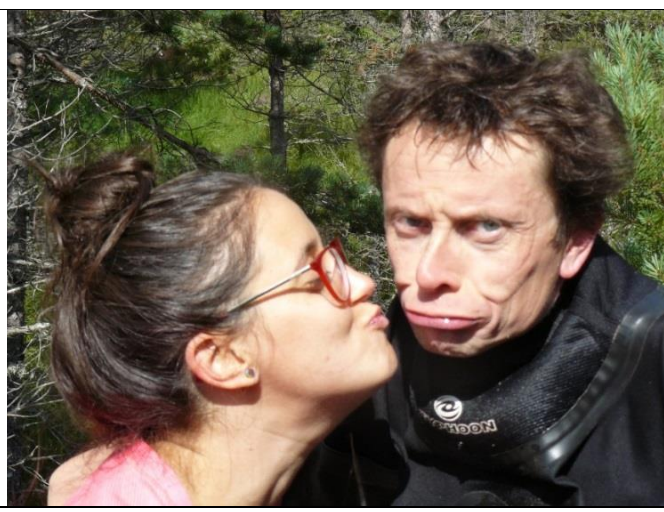
CODA Back to the Field

I write this concluding passage in Edinburgh on the 8th of September 2015, a few days after returning from my last fieldwork expedition with the members of the Scottish Pine Project. This time, Rob rented two small cottages in Tomich, near some lakes in Glen Affric. Before the funding for the project runs out in April 2016, Rob is keen to finish sampling what he calls the "Northern Cairngorms". At the end of the expedition, he is pleased that we have generated 130 samples and finished sampling the area. I write down and remember the code of the last sample I label ("G7S94"), which turns out to be a "very good looking sample" and allows us to finish the expedition on a good note. Rob and the others do not think this will be their last sample or expedition together. This is the end of the current funding of the Scottish Pine Project, but not the end of the project. "We have made a first stab at the climate story, but I am sure this will not be the end for the late Holocene work", Rob says.

In many ways, this fieldwork expedition was similar but also different from the ones in which I took part in previous years. The work of generating samples from submerged logs was equally hard, if not worse as the midges were particularly unbearable. As usual, fieldworkers enjoyed each other's company both in the site and at home around the dining table. This fieldwork was different because of its distinctive natural and social features. The availability and climate sensitivity of submerged trees in Glen Affric is different from other areas of the Cairngorms, and Rob and his team adjusted their judgement regarding what pieces of wood to accept to that specific natural environment. Likewise, the social dynamics among fieldworkers also changed slightly. This time, Miloš did not join us because he had already returned to the Czech Republic (where he is from originally) and started working in a tree-ring laboratory in Prague. Similarly, my participation in this expedition was not meant to generate data on how Rob and his team produced samples, but rather to negotiate the content of my thesis with them. Almost every evening, I sat for a couple of hours with Rob and asked him a few questions about his comments on my thesis. I also took two days "off" from the field to rewrite sections of the thesis. Rob was the only one who had read the thesis and often made jokes about its content with the other fieldworkers. "Björn, your boots are in the thesis!" or "According to Meri, I always sit at the head of the table!" Rob would say. The other fieldworkers were curious - and perhaps slightly worried - regarding what they would read in the thesis. I gave them an electronic copy the night before we departed from Tomich and we agreed that they would tell me by email if they approved its content.

Now that the funding for the Scottish Pine Project is coming to an end and Miloš will start publishing the reconstructions very soon, Rob is keen for others to use the Scottish data. He is starting to organise a workshop next March 2016 where he will invite other scientists (dendroclimatologists, dendroecologists and climate modellers) who he thinks might be interested in using the dataset. "There's so much more one could do with the Scottish data other than what we've done with Miloš", he says. Rob expects that the establishment of new collaborations through the sharing of the Scottish data could contribute to investigating other research problems, as well as boosting his citation index with 4-star REF papers and hence demonstrating the "impact" of the Scottish Pine Project to its funding bodies. One day in the field, I suggest to Rob that he could also use our collaboration as an example of "impact". Rob tells me, "I honestly don't know how many others would find your thesis interesting but maybe that is something we can discuss". Rob and Miloš cannot - and neither can I – know what "impact" my work will have on other people and how others will react to my work. We know for certain that my work has already impacted our lives and it has been well worth it so far. I agree with Rob that there is perhaps no better proof of the positive "impact" of this thesis than to re-create the Jane Goodall picture that others have previously used to identify us (Image 41).

Image 41. Showing the "positive impact" of my work with a recreation of the Goodall picture.



Appendix 1 the "Core-Set" of Dendroclimatology

Names in colour green have been co-authors of one or multiple Rob's papers.

Names in red are names of signatories that overlap across multiple cited papers

Names in blue are names of non-signatories that overlap across more than one cited paper.

Names of the signatories of the 'community response'	**Title and co-authors of the most cited paper of each signatory.**
1. Kevin J. Anchukaitis	*Asian monsoon failure and megadrought during the last millennium.* Edward R Cook, Kevin J Anchukaitis, Brendan M Buckley, Rosanne D D'Arrigo, Gordon C Jacoby, William E Wright.
2. Petra Breitenmoser	*Solar and volcanic fingerprints in tree-ring chronologies over the past 2000 years.* Petra Breitenmoser, Juerg Beer; Stefan Broennimann; David Frank; Friedhelm Steinhilber; Heinz Wanner.
3. Keith R. Briffa	*Low-frequency temperature variations from a northern tree ring density network.* Keith R Briffa, Timothy J Osborn, Fritz H Schweingruber, Ian C Harris, Philip D Jones,

	Stepan G Shiyatov, Eugene A Vaganov.
4. Agata Buchwal	*Temperature modulates intra-plant growth of Salix polaris from a high Arctic site (Svalbard).* Agata Buchal; Grzegorz Rachlewicz; Patrick Fonti; Paolo Cherubini; Holger Gaertner.
5. Ulf Büntgen	*2500 Years of European Climate Variability and Human Susceptibility.* Ulf Buentgen; Willy Tegel; Kurt Nicolussi; Michael McCormick; David Frank; Emma Emma; Jed O. Kaplan; Fritz Herzig; Karl-Uwe Heussner; Heinz Wanner; Juerg Luterbacher; Jan Esper.
6. Edward R. Cook	*Low-frequency signals in long tree-ring chronologies for reconstructing past temperature variability.* Jan Esper; Edward R Cook; Fritz H Schweingruber.
7. Rosanne D. D'Arrigo	*Asian Monsoon Failure and Megadrought During the Last Millennium.* Edward R Cook; *Kevin Anchukaitis*; Brendan Buckley; Rosanne D D'Arrigo; Gordon Jacoby; William E. Wright.
8. Jan Esper	*Low-frequency signals in long tree-ring*

	chronologies for reconstructing past temperature variability. Jan Esper; Edward R Cook; Fritz H Schweingruber.
9. Michael N. Evans	*Persistent solar influence on North Atlantic climate during the Holocene.* Gerard Bond; Bernd Kromer; Juerg Beer; Raimund Muscheler; Michael N Evans; William Showers; Sharon Hoffmann; Rusty Lotti-Bond; Irka Hajdas; Georges Bonani.
10. David Frank	*Persistent positive North Atlantic Oscillation mode dominated the medieval climate anomaly.* Emma Emma; Jan Esper; Nicholas E Graham; Andy Baker; James D Scourse; David Frank.
11. Håkan Grudd	*Long-term summer temperature variations in the Pyrenees.** Ulf Büntgen; David Frank; Håkan Grudd; Jan Esper. * *This is the second most cited article, as the first one is single authorship).*
12. Björn E. Gunnarson	*Improving a tree-ring reconstruction from west-central Scandinavia: 900 years of warm-season*

	temperatures. Björn E. Gunnarson, Hans W Linderholm; Anders Moberg.
13. Malcolm K. Hughes	*Northern hemisphere temperatures during the past millennium: inferences, uncertainties, and limitations.* Michael E Mann; Raymond S Bradley, Malcolm K Hughes.
14. Alexander V. Kirdyanov	The importance of early summer temperature and date of snow melt for tree growth in the Siberian Subarctic. Alexander Kirdyanov; Malcolm Hughes; Eugene Vaganov; Fritz Schweingruber; Pavel Silvin.
15. Christian Körner	*A world-wide study of high altitude treeline temperatures.* Christian Körner and Jens Paulsen.
16. Paul J. Krusič	*Tests of the RCS method for preserving low-frequency variability in long tree-ring chronologies.* Jan Esper; Edward R Cook; Paul Krusič ; Kenneth Peters; Fritz Schweingruber.

17. Brian Luckman	*Impact of climate fluctuations on mountain environments in the Canadian Rockies.* Brian Luckman and Trudy Kavanagh.
18. Thomas M. Melvin	*A "signal-free" approach to dendroclimatic standardisation.* Thomas M Melvin and Keith Briffa.
19. Matthew W. Salzer	Medieval drought in the upper Colorado River Basin. David M Meko; Connie A Woodhouse; Christopher A Baisan; Troy Knight; Jeffrey J Lukas; Malcolm K Hughes; Matthew W Salzer.
20. Alexander V. Shashkin	*Growth dynamics of conifer tree rings: images of past and future environments.* Eugene A Vaganov; Malcolm K Hughes, Alexander V Shashkin
	Climate and carbon cycle changes from 1850 to 2100 in MPI-ESM simulations for the Coupled Model Intercomparison Project phase 5. Marco A. Giorgetta; Johann Jungclaus; Christian H. Reick; Stephanie Legutke; Jürgen Bader; Michael Böttinger; Victor Brovkin; Traute

21. Claudia Timmreck	Crueger; Monika Esch; Kerstin Fieg; Ksenia Glushak; Veronika Gayler; Helmuth Haak; Heinz-Dieter Hollweg; Tatiana Ilyina; Stefan Kinne; Luis Kornblueh; Daniela Matei; Thorsten Mauritsen; Uwe Mikolajewicz; Wolfgang Mueller; Dirk Notz; Felix Pithan; Thomas Raddatz; Sebastian Rast; Rene Redler; Erich Roeckner; Hauke Schmidt; Reiner Schnur; Joachim Segschneider; Katharina D. Six; Martina Stockhause; Claudia Timmreck; Jörg Wegner; Heinrich Widmann; Karl-H. Wieners; Martin Claussen; Jochem Marotzke; Björn Stevens.
22. Eugene A. Vaganov	*Low-frequency temperature variations from a northern tree ring density network.* Keith R Briffa; Timothy J Osborn, Fritz H Schweingruber, Ian C Harris, Philip D Jones, Stepan G Shiyatov; Eugene A Vaganov
23. Rob Wilson	*On the 'divergence problem in northern forests: a review of the tree-ring evidence and possible causes.* Roseanne D'Arrigo, Rob Wilson, Beate Liepert, Paolo Cherubini.

Bibliography

Almklov, P. G.; Hepso, V, 2011. "Between and Beyond Data: How Analogue Field Experience Informs the Interpretation of Remote Data Sources in Petroleum Reservoir Geology", *Social Studies of Science*, 2011, Vol.41 (4), pp.552.

Atkinson, Paul, 1992. *Understanding Ethnographic Texts*. Newbury Park; London: Sage Publications.

Baier, Anette, 1986. "Trust and Antitrust," *Ethics*, Vol. 96 (2), pp. 231-260.

Barnes, Barry, Bloor, David, and Henry, John, 1996. *Scientific Knowledge: A Sociological Analysis*. Chicago; London: University of Chicago Press.

Barnes, Barnes, 1982. *T.S. Kuhn and Social Science*. London: Macmillan.

———, 1983. "Social Life as Bootstrapped Induction", *Sociology*, Vol.17 (4), pp. 524-545.

———, 1995. *Elements of Social Theory*. London: UCL Press.

———, 2005. "The Credibility of Scientific Expertise in a Culture of Suspicion", *Interdisciplinary Science Reviews*, Vol. 30, (1), pp. 11-18.

———, 2007. "Catching up with Robert Merton: Scientific Collectives as Status Groups", *Journal of Classical Sociology*, Vol.7 (2), pp.179-192.

Beck, Ulrich, 1992. *Risk Society: Towards a New Modernity*. London: Sage.

Becker, Howard and Geer, Blanche, 1957. "Participant observation and interviewing: A comparison." *Human Organization*, Vol. 16. (3), pp. 28-32.

———, 1958. "Problems of Inference and Proof in Participant Observation", *American Sociological Review*, Vol.23 (6), p. 653.

———, 1963. *Outsiders: Studies in the Sociology of Deviance.* New York: Free Press of Glencoe.

———, 1964. "Personal Change in Adult Life", *Sociometry*, Vol.27 (1), pp.40-53.

Becker, Howard; Geer, Blanche; Hughes, Everett; Strauss, Anselm, 1961. *Boys in White: Student Culture in Medical School*. Chicago: University of Chicago Press.

Berger, Peter *L,* 1963. *Invitation to Sociology: A Humanistic Perspective*. Hammondsworth, Middlesex, England: Penguin Books.

———, 1967. *The Social Construction of Reality: A Treatise in the Sociology of Knowledge.* London: Penguin Books.

Bloor, David, 1976. *Knowledge and Social Imagery*. Chicago; London: University of Chicago Press.

———, 1983. *Wittgenstein: A Social Theory of Knowledge*. London: Macmillan.

———, 1998. "A Civil Skepticism.", *Social Studies of Science*, Vol.28 (4), pp.655-665.

———. 1971. "Two Paradigms for Scientific Knowledge"?, *Science Studies*, Vol.1(1), pp.101-115.

Burgess, Robert, 1982. *Field Research: A Sourcebook and Field Manual*. London: Allen & Unwin.

Callon, Michel and Law, John, 1982. "On Interests and Their Transformation: Enrolment and Counter-Enrolment", *Social Studies of Science*, Vol.12 (4), pp. 615-625.

Callon, Michel, 1986. "The Sociology of An Actor-Network". In M. Callon, J. Law, & A. Rip, eds. Mapping the Dynamics of Science and Technology. London: Macmillan. p. 19–34.

———, 1986. "Some Elements of a Sociology of Translation: Domestication of the Scallops and the Fishermen of St Brieuc Bay." in *Power, Action and Belief: A New Sociology of Knowledge,* edited by John Law. London: Routledge & Kegan Paul. pp. 196–233.

Cetina, Karin Knorr, 1999. *Epistemic Cultures: How the Sciences Make Knowledge.* Cambridge, Mass.; London: Harvard University Press.

Collins, Harry and Evans, Robert, 2008. *Rethinking Expertise*. Chicago: University of Chicago Press.

Collins, Harry, 1981. "The Place of the Core-Set in Modern Science: Social Contingency with Methodological Propriety in Science in Innovation and Continuity in Science.", *History of Science, Vol.*19.(1), pp. 6-19.

———, 1984. "Researching Spoonbending: Concepts and Practise of Participatory Fieldwork". In: Bell, C. and Roberts, H. eds. *Social Researching: Politics, Problems, Practise.* London: Routledge and Kegan Paul, pp. 54-69.

———, 1988. "Public Experiments and Displays of Virtuosity: The Core-Set Revisited." *Social Studies of Science*, Vol. 18 (4), p. 741.

———, 1992. Changing Order: Replication and Induction in Scientific Practice. Chicago; London: University of Chicago Press.

———, 2001. "Tacit knowledge, Trust and the Q of Sapphire.", *Social Studies of Science*, Vol.31 (1), p. 82.

———, 2004. *The Gravity's Shadow: the Search for Gravitational Waves*. Chicago; London: University of Chicago Press.

———, 2010. "Working paper 140: Inside and Outside Science: Beware of acting too hastily on Climategate". http://www.cf.ac.uk/socsi/resources/wp140.pdf

———, 2010. *Tacit and Explicit Knowledge*. Chicago/London: University of Chicago Press.

———, 2011. *The Gravity's Ghost: Scientific Discovery in the Twenty-First Century*. Chicago: University of Chicago Press.

———, 2014. *Are We All Scientific Experts Now?*. Cambridge, UK: Polity.

Daston, Lorraine, 2007. "How to Think About Science: Lorraine Daston", *Canadian Broadcasting Corporation*, http://www.cbc.ca/radio/ideas/how-to-think-about-science-part-2-1.464988

Demeritt, David, 2001."The Construction of Global Warming and the Politics of Science." *Annals of the Association of American Geographers*, Vol. 91.(2), p.315.

Dritsas, Lawrence, 2011. “Expeditionary Science: Conflicts of Method in Mid-Nineteenth Century Geographical Discovery', in *Geographies of Nineteenth-Century Science*, eds. Charles WJ Withers and David Livingstone. Chicago: University of Chicago Press.

Edward J. Hackett et al., 2008. *The Handbook of Science and Technology Studies*. Cambridge, Mass.: MIT Press: Published in cooperation with the Society for the Social Studies of Science.

Edwards, Paul, and Schneider, Stephen, 2001. “Self-Governance and Peer Review in Science-for Policy” in Clark Miller and Paul N. Edwards, eds., *Changing the Atmosphere: Expert Knowledge and Environmental Governance.* Cambridge: MIT Press, 2001.

Evans-Pritchard, Edward Evan, 1937. *Witchcraft, Magic, and Oracles among the Azande*. Oxford: Clarendon Press, 1937.

Fabian, Joannes, 1983. *Time and the Other*. *How Anthropology Makes its Object.* New York: Columbia University Press.

Fine, Arthur, 1993. “Fictionalism”, *Midwest Studies In Philosophy*, Vol.18 (1), pp.1-18

Forsythe, Diane, 1999. “It’s Just a Matter of Common Sense: Ethnography as Invisible Work”, *Computer Supported Cooperative Work* (CSCW), Vol.8 (1), pp.127-145.

Frigg, Roman, 2010. "Models and fiction.” *Synthese*, Vol.172 (2), pp.251-268.

Garfinkel, Harold, 1967. *Studies in Ethnomethodology*. Cambridge: Polity Press.

Gieryn, Thomas, 1983. "Boundary-Work and the Demarcation of Science from Non-Science: Strains and Interests in Professional Ideologies of Scientists”, *American Sociological Review*, Vol.48 (6), pp.781-795.

Goffman, Alice, 2015. *On the Run: Fugitive Life in an American City*. New York: Picador/Farrar, Straus and Giroux.

Goffman, Erving, 1968. *Stigma: Notes on the Management of Spoiled Identity.* London: Penguin Books.

———, 1989. “On Fieldwork”, *Journal of Contemporary Ethnography*, Vol.18 (2), pp.123-132.

Golinski, Jan, 1998. *Making Natural Knowledge: Constructivism and the History of Science.* Cambridge: Cambridge University Press, 1998.

Gooday, Graeme, 2004. *The Morals of Measurement: Accuracy, Irony, and Trust in late Victorian Electrical Practice.* Cambridge: Cambridge University Press.

Goodwin, Charles, 1994. "Professional vision", *American Anthropologist*, Vol.96 (3), pp.606-633.

Granovetter, Mark, 1973. “The Strength of Weak Ties”, *American Journal of Sociology*, Vol.78, 6, pp. 1360-1380.

———, 1985. “Economic Action and Social Structure: The Problem of Embeddedness”, *American Journal of Sociology*, Vol.91 (3), pp. 481-510.

Hagstrom, W, 1982. "Gift Giving as an Organizing Principle in Science” in *Science in Context: Readings in the Sociology of Science*. Milton Keynes: Open University Press.

Hammersley, Martyn, 1998. Standards for Assessing Ethnographic Research*” in Reading Ethnographic Research: A Critical Guide.* New York; London: Longman.

Hampel, Mathis, 2015. *Climate Reconstruction and the Making of Authoritative Scientific Knowledge*, Unpublished Ph.D. thesis, King’s College London.

Hatherly, David; Leung, David and MacKenzie, Donald, 2008. "‘The Finitist Accountant: Classifications, Rules and the Construction of Profits” in *Living in a Material World: Economic Sociology meets Science and Technology Studies*, eds. Trevor Pinch, Richard Swedberg. MIT Press. pp. 131-160

Hess, David, 2001. “Ethnography and the Development of Science and Technology Studies”. In: Atkinson, P. et al. eds. *Handbook of Ethnography.* London, Thousand Oaks and New Delhi: Sage Publications, 2001.

Hevly, Bruce, 1996. "The Heroic Science of Glacier Motion”, *Osiris*, Vol.11 (1), p.66.

Howarth, Candice, and Sharman, Amelia, 2015. “Labelling Opinions in the Climate Debate: A Critical Review”, *Wiley Interdisciplinary Reviews: Climate Change*, Vol. 6 (2), pp. 239–254.

Hulme, Mike and Mahoney, Martin, 2010. "Climate Change: What Do We Know About the IPCC?" , *Progress in Physical Geography*, Vol.34 (5), pp. 705-718.

Hulme, Mike, 2009. *Why We Disagree about Climate Change: Understanding Controversy, Inaction and Opportunity.* Cambridge: Cambridge University Press.

Hwang, Ruey-Chyi; Li, Zheng-Feng; Huang, Chih-Tung; Chu, Rong-Xuan; Fan, Xiang, 2010. "Dropping the Brand of Edinburgh School", *East Asian Science, Technology and Society,* Vol.4 (4), p.601).

Irwin, Alan, 2001. "Constructing the Scientific Citizen", *Public Understanding of Science,* Vol.10 (1), pp.1-18.

———, 2002. *Citizen Science A Study of People, Expertise and Sustainable Development*. Hoboken: Taylor and Francis.

Irwin, Alan, and Michael, Mike, 2003. *Science, Social Theory and Public Knowledge*. Maidenhead: Open University Press.

Jasanoff, Sheila et al; 1995. *The Handbook of Science and Technology Studies.* Thousand Oaks, Calif.; London: Sage Publications, 1995.

Kant, Immanuel, 1781. *Critique of Pure Reason*. Cambridge: Cambridge University Press, (editon 1998).

Kohler, Robert, 1999. "Moral Economy, Material Culture and Community in Drosophila Genetics." in *the Science Studies Reader*. New York; London: Routledge, 1999.

———, 2002. *Landscapes and Labscapes: Exploring the Lab-Field Border in Biology*. Chicago: University of Chicago Press.

———, 2006. *All Creatures: Naturalists, Collectors, and Biodiversity, 1850-1950.* Princeton, N.J.: Princeton University Press.

Knorr Cetina, Karin, 1999. *Epistemic Cultures: How the Sciences Make Knowledge.* Cambridge, Mass.; London: Harvard University.

Kuhn, Thomas, 1962. *The Structure of Scientific Revolutions*. Chicago: University of Chicago Press.

———, 1970. "Logic of Discovery or Psychology of Research?" in Imre Lakatos and Alan Musgrave (Eds.), *Criticism and the Growth of Knowledge: Proceedings of the International Colloquium in the Philosophy of Science*. Cambridge: University Press.

Kuklick, Henrika and Kohler, Robert, 1996. *Science in the Field Science in the Field*. Chicago: University of Chicago Press.

Latour, Bruno, 1979 *Laboratory Life: the Social Construction of Scientific Facts,* (Princeton, N.J; Chichester: Princeton University Press.

———, 1987. *Science in Action: How to Follow Scientists and Engineers through Society*. Cambridge, MA: Harvard University Press.

———, 1999. *Pandora's Hope: Essays on the Reality of Science Studies.* Cambridge, Mass.; London: Harvard University Press.

Law, John and Lynch, Mike, 1988. "Lists, Field Guides and the Descriptive Organization of Seeing: Birdwatching as an Exemplary Observational Activity.", *Human Studies*, Vol. 11.(2), pp. 271-303.

Luckmann, Thomas, 1967. *The Invisible Religion*: *The Problem of Religion in Modern Society*. London: Macmillan.

Lynch, Michael, 1985. "Discipline and the Material Form of Images: An Analysis of Scientific Visibility." *Social Studies of Science*, Vol. 15.(1).

———, 2010. *Truth Machine: the Contentious History of DNA Fingerprinting*. Chicago: University of Chicago Press.

MacKenzie, Donald, 2001. *Mechanizing Proof: Computing, Risk and Trust.* Cambridge, Mass.; London: MIT Press.

———, 2009. "Making Things the Same: Gases, Emission Rights and the Politics of Carbon Markets." *Accounting, Organizations and Society*, Vol. 34, (3), pp. 440-455

Mazzoti, Massimo, 2008. Knowledge as Social Order: Rethinking the Sociology of Barry Barnes. Aldershot: Ashgate._

Mauss, Marcel, 1950. *The Gift: Forms and Functions of Exchange in Archaic Societies*. London: Cohen & West.

Mead, George Herbert, 1934. *Mind, Self, and Society: from the Standpoint of a Social Behaviorist.* Chicago, Ill.; London: The University of Chicago Press.

Merton, Robert, 1942. "The Normative Structure of Science." in *The Sociology of Science: Theoretical and Empirical Investigations.* Chicago; London: University of Chicago Press, 1973.

———, 1963. "The Ambivalence of Scientists." *Bulletin of the Johns Hopkins Hospital*, 112.

———, 1976. "Postscript: The Ambivalence of Scientists." in Merton, Robert K. *Sociological Ambivalence and Other Essays*. London: Simon and Schuster, 1976.

———, 1978. "The Sociology of Science: An Episodic Memoir", pp. 3–14 in R.K. Merton and J. Gaston (eds) *The Sociology of Science in Europe*. Carbondale, IL: Southern Illinois University Press.

Mitroff, Ian, 1974. "Norms and Counter-Norms in a Select Group of the Apollo Moon Scientists: A Case Study of the Ambivalence of Scientists." *American Sociological Review*, Vol.39 (4), pp.579-595.

Mosher, S. and Fuller, T, 2010. Climategate: The CRUtape Letters. Lexington, KY: CreateSpace.

Mulkay, Michael, 1976. "Norms and Ideology in Science", *Social Science Information*, Vol. 15, pp. 637–656.

O'Neill, Onora, 2002. *A Question of Trust: the BBC Reith lectures 2002.* Cambridge: Cambridge University Press.

Owen-Smith, Jason, 2001. "Managing Laboratory Work through Skepticism: Processes of Evaluation and Control", *American Sociological Review*, Vol.66 (3), pp.427-452.

Partridge, Eric, 1958. *Origins:* A *Short Etymological Dictionary of Modern English*. London: Routledge & Paul.

Pearce, Fred, 2010. *The Climate Files: The Battle for the Truth about Global Warming.* London: Guardian Books/Random House.

Pinch, Trevor, and Bijker, Wiebe, 1984. “The Social Construction of Facts and Artefacts: or How the Sociology of Science and the Sociology of Technology might Benefit Each Other”, *Social Studies of Science*, Vol.14 (3), p.399-441.

Pinch, Trevor, 1985. "Towards an Analysis of Scientific Observation: The Externality and Evidential Significance of Observational Reports in Physics”, *Social Studies of Science*, Vol.15 (1), pp.3-36.

———, 1986. *Confronting Nature: the Sociology of Solar-Neutrino Detection*. Dordrecht, Holland; Boston: D. Reidel Pub. Co Higham, Mass.

Polanyi, Michael, 1958. *Personal Knowledge: Towards a Post-Critical Philosophy*. London: Routledge & K. Paul.

———, 1966. “The Logic of Tacit Inference”, *Philosophy*, Vol.41 (155).

Pollner, Melvin, 1987. *Mundane Reason*: *Reality in Everyday and Sociological Discourse*. Cambridge: Cambridge University Press.

Popper, Karl, 1963. *Conjectures and Refutations: the Growth of Scientific Knowledge*. London; New York: Routledge & Kegan Paul, 2002.

Ramírez-i-Ollé, Meritxell, 2015. "Rhetorical Strategies for Scientific Authority: a Boundary-Work Analysis of ‘Climategate’”, *Science as Culture*, p.1-28.

———, 2015. “The Social Life of Climate Science”, *Method Quarterly*, Issue 2, http://www.methodquarterly.com/2015/02/the-social-life-of-climate-science/ .

Rees, Amanda, 2009. *The Infanticide Controversy: Primatology and the Art of Field Science.* Chicago, IL: University of Chicago Press.

Reyes-Galindo, Luis, 2014. "Linking the Subcultures of Physics; Virtual Empiricism and the Bonding Role of Trust", *Social Studies of Science*, Vol.44 (5), pp.745

Ribeiro Duarte, Tiago, 2013. *Expertise and the Fractal Model Communication and Collaboration between Climate-Change Scientists*, PhD dissertation, University of Cardiff.

Rohlf, Michael, 2015. "Immanuel Kant", *The Stanford Encyclopedia of Philosophy (Summer 2014 Edition)*, Edward N. Zalta (ed.), accessed 14 August 2015 <http://plato.stanford.edu/archives/sum2014/entries/kant/>..

Ryghaug Marianne, and Skjølsvold, Tomas Moe, 2010. "The Global Warming of Climate Science: Climategate and the Construction of Scientific Facts", *International Studies in the Philosophy of Science*, Vol. 24, p.287-307;

Schutz, Alfred, 1974. *The Structures of the Life-World.* London: Heinemann.

Serrès, Michel, *1980. The Parasite*. Paris: Grasset.

Shapin, Steven, 1984. "Pump and Circumstance: Robert Boyle's Literary Technology", *Social Studies of Science,* Vol. 14 (4), pp. 481-520.

———, 1988. "The House of Experiment in Seventh Century England", *Isis,* Vol.79 (3), p.373.

———, 1994. *A Social History of Truth: Civility and Science in Seventeenth-Century England.* Chicago; London: University of Chicago Press.

———, 1995. "Here and Everywhere: Sociology of Scientific Knowledge", *Annual Review of Sociology*, Vol.211 (1), p.289-321.

Sharman, Amelia, 2014. "Mapping the Climate Sceptical Blogosphere.", *Global Environmental Change,* Vol.26, pp.159-170

Shove, Elizabeth, 2010. "Beyond the ABC: climate change policy and theories of social change", *Environment and Planning A*, Vol. 42, (6), pp.1273-1285.

Skrydstrup, Martin, 2013. "Tricked or Troubled Natures? How to Make Sense of 'Climategate'", *Environmental Science & Policy,* 28, pp. 92–99.

Thaler Richard and Sustein Cass, 2008. *Nudge: Improving Decisions about Health, Wealth, and Happiness*. New Haven; London: Yale University Press.

Tillmann-Healy, Lisa, 2003."Friendship as Method." *Qualitative Inquiry*, Vol.9 (5), p. 734.

Vaihinger, Hans, 1924. *The Philosophy of 'As-If: a System of the Theoretical, Practical, and Religious Fictions of Mankind'.* London: K. Paul, Trench, Trubner & co., ltd. New York: Harcourt, Brace & company.

Van Der Sluijs, Jeroen; Van Eijndhoven, Josee; Shackley, Simon; Wynne, Brian, 1998. "Anchoring Devices in Science for Policy. The Case of Consensus around Climate Sensitivity." *Social Studies of Science*, Vol. 28. (2), pp. 291-323.

Vertesi, Janet, 2012. "Seeing like a Rover: Visualization, Embodiment, and Interaction on the Mars Exploration Rover Mission", *Social Studies of Science*, Vol.42 (3), pp.393-414.

Wagenknecht, Susan, 2015. "Dialoguing Practices and Explanatory Responsiveness", in "Facing the Incompleteness of Epistemic Trust: Managing Dependence in Scientific Practice", *Social Epistemology*, Vol.29 (2), p.160-184.

Wax, Rosalie, 1986. *Doing fieldwork: Warnings and Advice*. University of Chicago Press.

Webb, Beatrice, 1926. *My Apprenticeship*. London: Longmans, Green and Co.

Whyte, William Foote, 1955. *Street Corner Society: the Social Structure of an Italian Slum.* Chicago; London: University of Chicago Press.

Yearley, Steven, 1997. "The Changing Authority of Science", *Science Studies*, Vol. 10, pp. 65-75.

———, 2009. "Sociology and Climate Change after Kyoto", *Current Sociology*, Vol.57 (3), pp. 389-405

Sources of Data

Anchukaitis, Kevin J.; Breitenmoser, Petra; Briffa, Keith; Buchwal, Agata; Büntgen, Ulf; Cook, Edward; D'Arrigo, Rosanne D.; Esper, Jan; Evans, Michael N.; Frank, David; Grudd, Håkan; Gunnarson, Björn E.; Hughes, Malcolm K.; Kirdyanov, Alexander V.; Körner,Christian; Krusič , Paul J.; Luckman, Brian; Melvin, Thomas M.; Salzer, Matthew W.; Shashkin, Alexander V.; Timmreck, Claudia; Vaganov, Eugene A.; Wilson, Rob; 2012. "Tree Rings and Volcanic Cooling.", *Nature Geoscience*, Vol. 5.(12), pp. 836.

Baillie, Michael, 1982. *Tree-Ring Dating and Archaeology*. Chicago, Ill.: University of Chicago Press.

———, 1995. *A Slice through Time: Dendrochronology and Precise Dating*. London: Batsford.

———, 2015. "Tree-ring patterns are intellectual property, not climate data", *Guardian*, January 19, 2013, accessed 1 July 2015 http://www.theguardian.com/environment/2010/may/11/climate-science-tree-ring-data>

Björklund, Jesper; Gunnarson, Björn, Seftigen; Kristina, Esper; Esper, Jan; Linderholm, Hans, 2013. "Is Blue Intensity Ready to Replace Maximum Latewood Density as a Strong Temperature Proxy? A Tree-Ring Case Study on Scots pine from Northern Sweden.", *Climate of the Past Discussions* 9, pp. 5227-5261.

Bouldin, Jim, 2015. “Sever analytical problems with dendroclimatology, Part One, *The Ecologically Orientated*, accessed 15 July 2015 https://ecologicallyoriented.wordpress.com/2012/11/10/severe-analytical-problems-in-dendroclimatology-part-1/

Briffa, Keith and Melvin, Thomas, 2011. "A Closer Look at Regional Curve Standardization of Tree-Ring Records: Justification of the Need, a Warning of Some Pitfalls, and Suggested Improvements in its Application." in M. K. Hughes, H. F. Diaz, and T. W. Swetnam (Eds), *Dendroclimatology: Progress and Prospects*. Dordrecht; London: Springer. pp. 113-145.

———, 1984. *Tree–Climate Relationships and Dendroclimatological Reconstruction in the British Isles*, PhD thesis, University of East Anglia.

Büntgen, Ulf; Wacker,Lukas; Nicolussi, Kurt; Sigl, Michael; Güttler, Dominik; Tegel, Willy; Krusič , Paul J.; Esper, Jan, 2014. "Extraterrestrial Confirmation of Tree-Ring Dating." *Nature Climate Change*, Vol.4.(6), pp. 404-405.

Campbell, Rochelle; McCarroll, Danny; Robertson, Iain; Loader, Neil J; Grudd, Håkan; Gunnarson, Björn, 2011. "Blue intensity in Pinus Sylvestris Tree Rings: a Manual for a New Paleoclimate Proxy.", *Tree-Ring Research*, Vol. 67. (2), pp. 127-134.

Christopher Smout, Alan R MacDonald, and Fiona Watson, 2005. *A History of the Native Woodlands of Scotland, 1500-1920.* Edinburgh: Edinburgh University Press.

Cook, Edward R.; Meko, David M ; Stahle, David W.; Cleaveland, Malcolm K.*;* 1999, "Drought Reconstructions for the Continental United States", *Journal of Climate*, Vol.12 (4), pp.1145-1163.

Cook, Edward R. and Kairiukstis, L.A. 1990. *Methods of Dendrochronology: Applications in the Environmental Sciences.* Kluwer Academic Publishers, Dordrecht, the Netherlands.

Cook, Edward R., 1985. "A Time Series Analysis Approach to Tree-Ring Standardisation", PhD dissertation, The University of Arizona.

Cook, Edward R., 1987. "The Decomposition of Tree-Ring Series for Environmental Studies." *Tree-Ring Bulletin*. Vol. 47.

Copenheaver, Carolyn; Goldbeck, Kyrille and Cherubini, Paolo, 2010. "Lack of Gender Bias in Citation Rates of Publications by Dendrochronologists: What is Unique about this Discipline?." , *Tree-Ring Research*, Vol. 66. (2), pp. 127-133.

D'Arrigo, Rosanne; Wilson, Rob and Jacoby, Gordon, 2006. "On the long-term context for late twentieth century warming", *Journal of Geophysical Research: Atmospheres (1984–*2012), Vol. 111.D03103.

D'Arrigo, Rosanne; Wilson, Rob; Liepert, Beate; Cherubini, Paolo, 2008."On the 'Divergence Problem in Northern Forests: a Review of the Tree-Ring Evidence and Possible Causes", *Global and Planetary Change*, Vol. 60.(3), pp. 289-305.

D' Arrigo, Rosanne; Wilson, Rob; Anchukaitis, Kevin J., "Volcanic cooling signal in tree ring temperature records for the past millennium.", 2013. *Journal of Geophysical Research: Atmospheres*, Vol.118.(16).

Dawson, Alistair, 2009. *So Foul and Fair a Day: a History of Scotland's Weather and Climate*. Edinburgh: Birlinn.

Douglas, Andrew, 1914. "A Method of Estimating Rainfall by the Growth of Trees", *Bulletin of the American Geographical Society*, Vol.46 (5), pp.321-335.

———, 1937. "Tree Ring Work", *Tree-Ring Bulletin*. .

———,, 1943. "Notes on the Technique of Tree-Ring Analysis", *Tree Ring Bulletin*.

Druckenbrod, Daniel L; Pederson, Neil; Rentch, James; Cook, Edward, 2012. "A Comparison of Times Series Approaches for Dendroecological Reconstructions of Past Canopy Disturbance Events.", *Forest Ecology and Management*, Vol. 302.

Edwards, Tamsin, 2013. *Twitter* post, 21st of October 2013, 6:16am https://twitter.com/scottstgeorge/status/392283501552484352 (Accessed 11th of April 2015).

Esper, Jan; Büntgen, Ulf; Luterbacher, Jürg; Krusič , Paul J., 2013. "Testing the hypothesis of post-volcanic missing rings in temperature sensitive dendrochronological data.", *Dendrochronologia*, Vol. 31.(3), pp. 216-222.

Esper, Jan; Schneider, Lea; Krusič , Paul; Luterbacher, Jürg; Büntgen, Ulf; Timonen, Mauri; Sirocko, Frank; Zorita, Eduardo, 2013. "European summer temperature response to annually dated volcanic eruptions over the past nine centuries.", Bulletin of Volcanology, Vol.75.(7), pp. 1-14.

Frank, David; Esper, Jan; Zorita, Eduardo; Wilson, Rob, 2010. “A Noodle, Hockey Stick, and Spaghetti Plate: a Perspective on High Resolution Paleoclimatology”, *Wiley Interdisciplinary Reviews: Climate Change*, Vol.1(4), pp.507-516.

Fritts Harold and Swetnam, Thomas, 1989. “Dendroecology: A Tool for Evaluating Variations in Past and Present Forest Environments, *Advances in Ecological Research*, pp. 19111-188.

Fritts, Harold, 1976. *Tree Rings and Climate*. London: Academic Press.

Gagen, Mary; McCarrol, Daniel; Loader J. Neil; Robertson, Iain, 2010. "Stable Isotopes in Dendroclimatology”: Moving Beyond ‘Potential’." in *Dendroclimatology: Progress and Prospects*, eds. Hughes, Malcolm K, Thomas W Swetnam, and Henry F Diaz. Dordrecht; London: Springer.

Gould, J.S. 1965. “Is Uniformitarianism Necessary?”, *American Journal of Science*, Vol. 263, (3), pp. 223-228; JAmes Shea, “Twelve Fallacies of Uniformitarianism,”, *Geology*, 1982, Vol.10 (9), pp.455.

Grissino-Mayer, Henri and Fritts, Harold, 1997. "The International Tree-Ring Data Bank: an enhanced global database serving the global scientific community.", The Holocene, Vol. 7 (2), pp. 235-238.

Harris, I.; Jones, P. D.; Osborn, T. J.; Lister, D. H., 2014. "Updated high-resolution grids of monthly climatic observations – the CRU TS3.10 Dataset”, *International Journal of Climatology*, Vol. 34, (3), pp. 623–642.

Holmes, Richard, 1983. "Computer-Assisted Quality Control in Tree-Ring Dating and Measurement.", *Tree-Ring Bulletin* , Vol. 43.(1), pp. 69-78.

Hughes, Malcolm. 2002, "Dendrochronology in Climatology – the State of the Art.", *Dendrochronologia*, Vol. 20.(1), pp. 104.

Hughes, Malcolm, 2011. "Dendroclimatology in High-Resolution Paleoclimatology." in *Dendroclimatology: Progress and Prospects*, (Eds) Malcolm Hughes, Thomas W Swetnam, and Henry F Diaz. Dordrecht; London: Springer.

Hughes, Malcolm, 2012. “[ITRDBFOR] Comment to Mann et al. (2012) at Nature Geoscience”, 26 November, 16:42.

Hughes, Malcolm; Schweingruber, Fritz; Cartwright, D.; Kelly, P. M, 1984. "July– August Temperature at Edinburgh between 1721 and 1975 from tree-ring density and width data", *Nature*, 308, pp. 341–343.

IPCC Third Assessment Report, Climate Change *2001*, http://www.grida.no/climate/ipcc_tar/vol4/english/pdf/wg1spm.pdf (accessed 8th April 2008).

Jones, P. D.; Lister, D., 2004. "The development of monthly temperature series for Scotland and Northern Ireland", *International Journal of Climatology*, Vol. 24, (5), pp.569-590

Josh, 2013. "More battling", 241. 21 October 2013. http://bishophill.squarespace.com/blog/2013/10/21/more-battling-josh-241.html

Mann, Michael, 2013. *Twitter* post, 21st o October, 8:27 am https://twitter.com/michaelemann/status/392311221371678720 (Accessed 11th of April 2015).

———,, 2013. *Twitter* post, 21st of October, 7:11 am https://twitter.com/michaelemann/status/392292189176619008 (Accessed 11th of APril 2015)

———,, 2013.*The Hockey Stick and the Climate Wars: Dispatches from the Front Lines*. New York City: Columbia University Press.

———,, 2012. "Global Temperatures, Volcanic Eruptions, and Trees that Didn't Bark", *Real Climate*, 6 February, Accessed 15 July 2015, http://www.realclimate.org/index.php/archives/2012/02/global-temperatures-volcanic-eruptions-and-trees-that-didnt-bark/#sthash.GsxX0Hid.dpuf

Mann, Michael; Bradley, Raymond and Hughes, Malcolm, 1998. "Global-scale temperature patterns and climate forcing over the past six centuries", *Nature*, Vol.392 (6678), pp. 779–787

Mann, Michael; Bradley, Raymond and Hughes, Malcolm, 1999. "Northern Hemisphere Temperatures During the Past Millennium: Inferences, Uncertainties, and Limitations.", *Geophysical Research Letters,* Vol. 26.(6), pp. 759-762

Mann, Michael; Bradley, Raymond and Hughes, Malcolm, 2004. "Corrigendum: Global-scale temperature patterns and climate forcing over the past six centuries", *Nature,* Vol. 430 (6995): pp. 105–105.

Mann, Michael; Fuentes, Jose D and Rutherford, Scott, 2012. "Underestimation of Volcanic Cooling in Tree-Ring Based Reconstructions of Hemispheric Temperatures.", *Nature Geoscience*, Vol. 5.(3), pp. 202-205.

Mann, Michael E.; Rutherford, Scott; Schurer, Andrew; Tett, Simon F. B.; Fuentes, Jose D., 2013. "Discrepancies between the modeled and proxy-reconstructed response to volcanic forcing over the past millennium: Implications and possible mechanisms.", *Journal of Geophysical Research: Atmospheres*, Vol.118.(14), pp. 7617-7627.

Mccarroll, Daniel, Pettigrew, E.; Luckman, A.; Guibal, F, 2002. "Blue Reflectance Provides a Surrogate for Latewood Density of High-Latitude Pine Tree Rings.", *Arctic, Antarctic, and Alpine Research*, pp. 450-453.

McCarroll, Danny and Loader, Neil J., 2004. "Stable Isotopes in Tree-Rings", *Quaternary Science Reviews*, Vol. 23(7), pp.771-801.

McIntyre, Steve and McKitrick, 1998. "Corrections to the Mann et al. (1998) Proxy Data Base and Northern Hemisphere Average Temperature Series, *Energy and Environment*, 2003, Vol. 14 (6).

———, 2005. "Hockey Sticks, principal components and spurious significance", *Geophysical Research Letters*, 2005, Vol. 32 (3);

McIntyre, Steve. "Yamal: A "Divergence" Problem", *Climate Audit*, 27 September 2009, Accessed 15 July 2015 http://climateaudit.org/2009/09/27/yamal-a-divergence-problem/

Melvin, Thomas and Briffa, Keith, 2008. "A 'Signal-Free' Approach to Dendroclimatic Standardisation", *Dendrochronologia*, Vol.26 (2), pp.71-86.

Melvin, Thomas; Grudd, Hakan and Briffa, Keith, 2013. "Potential Bias in 'Updating' Tree-Ring Chronologies Using Regional Curve Standardisation: Re-processing 1500 years of Torneträsk Density and Ring-Width Data.", *The Holocene*, Vol. 23.(3), pp. 364-373.

Michaels, J. P., and Knappenberger. C, P. 2014. "Mann's not so Explosive Findings on Volcanos' Climate Influence in Tree Rings", Wattsupwiththat, 10 October 2014, http://wattsupwiththat.com/2014/10/10/manns-not-so-explosive-findings-on-volcanos-climate-influence-in-tree-rings/ (accessed 13th April 2015).

Montford, Andrew, 2013. "Wilson on Millennial Temperature Reconstructions", *Bishop Hill blog*, 21st of October 2013, http://bishophill.squarespace.com/blog/2013/10/21/wilson-on-millennial-temperature-reconstructions.html (accessed 11 April 2015).

———, 2012. Bishop Hill blog - Lonely old Mann." 2012. 24 November 2012 <http://www.bishop-hill.net/blog/2012/11/26/lonely-old-mann.html> (accessed 8th of April 2015)

Nehrbass, Christoph; Babst, Flurin; Klesse, Stefan; Nötzli, Magdalena; Bouriaud, Olivier; Neukom, Raphael; Dobbertin, Matthias; Frank, David, 2014. "The Influence of Sampling Design on Tree-Ring -Based Quantification of Forest Growth.", *Global Change Biology*, Vol. 20.(9), pp 2867-2885.

Pilcher and Baillie, Michael, 1980. "Six modern oak chronologies from Ireland" and "Eight modern oak chronologies from England and Scotland", *Tree-Ring Bulletin*, Vol.40.

Rutherford, Scott and Mann, Michael, 2014. "Missing tree rings and the AD 774-775 radiocarbon event.", *Nature Climate Change*, Vol.4.(8), pp. 648-649.

Rydval, Miloš, 2015. *Dendroclimatic Reconstruction of Late Holocene Summer Temperatures in the Scottish Highlands*, Unpublished Thesis Dissertation, The University of St Andrews.

Rydval, Miloš; Larsson, Lars-Åke; McGlynn, Laura; Gunnarson, Björn E.; Loader, Neil J.; Young, Giles H.F; Wilson, Rob, 2014. "Blue Intensity for Dendroclimatology: Should We Have the Blues?" Experiments from Scotland." *Dendrochronologia*, Vol. 32, pp. 191–204.

Schmidt, Gavin, 2013. *Twitter* post, 21st of October 2013, 13:39 https://twitter.com/climateofgavin/status/392384557523025920 (Accessed 11th of April 2015).

Schulman, Edmund, 1951. "Tree-Ring Indices of Rainfall, Temperature, and River Flow, *American Meteorological Society*.

———, 1945. "Tree-Rings and Runoff in the South Platte River Basin.", *Tree-Ring Bulletin*, 1945, Vol. 11, (3).

Schweingruber, Fritz, 1988. *Tree Rings-Basics and Applications of Dendrochronology*. D. Reidel Publishing Company.

Speer, James, 2010. *Fundamentals of Tree-Ring Research*. Tucson: University of Arizona Press.

———, 2014. "WorldDendro Conference in Melbourne", *DendroSabbatical*, 8th January 2014. http://dendrosabbatical.blogspot.co.uk/2014/01/worlddendro-conference-in-melbourne.html

St George, Scott, 2013.*Twitter* Post, 21st of October 2013, 6:37am https://twitter.com/scottstgeorge/status/392283501552484352 (Accessed 11th of April 2015).

St George, Scott; Ault, Toby and Torbenson, Alex, 2013. "The rarity of absent growth rings in Northern Hemisphere forests outside the American Southwest.", *Geophysical Research Letters*, Vol. 40 (14), pp. 3730.

Stokes, Marvin and Smiley, Tarah, 1969. *An Introduction to Tree-Ring Dating*. Tucson: University of Arizona Press.

The Hockey Schtick, 2014. "New paper finds temperature reconstructions from tree-rings overestimate volcanic cooling", The Hockey Schtick, 8th October 2014, http://hockeyschtick.blogspot.co.uk/2014/10/new-paper-finds-temperature.html (accessed 13th of April 2015).

Tingley, Martin; Stine, Alexander and Huybers, Peter, 2014. "Temperature reconstructions from tree-ring densities overestimate volcanic cooling", *Geophysical Research Letters*, Vol 41, (22), pp. 7838–7845.

Tolwinski-Ward, Susan; Evans, Michael; Hughes, Malcolm; Anchukaitis, Kevin, 2011. "An Efficient Forward Model of the Climate Controls on Interannual Variation in Tree-Ring Width.", *Climate Dynamics*, Vol. 36.(11-12), pp. 2419-2439.

Watts, Anthony. 2012. "Dendros Stick it to the Mann", *Whatsupwiththat*, 25 November, http://wattsupwiththat.com/2012/11/25/dendros-stick-it-to-the-mann/ (accessed 8th of April 2015)

Wilson, Rob and Hopfmueller, M., 2001. "Dendrochronological investigations of Norway spruce along an elevational transect in the Bavarian Forest, in Germany", *Dendrochronologia*, Vol.19, (1), pp. 925-936

Wilson, Rob and Luckman, Brian, 2002. "Tree-Ring Reconstruction of Maximum and Minimum Temperatures and the Diurnal Temperature Range in British Columbia, Canada." *Dendrochronologia*, Vol. 20 (3), pp. 257-268.

———, 2003."Dendroclimatic Reconstruction of Maximum Summer Temperatures from Upper Treeline Sites in Interior British Columbia, Canada." *The Holocene*, Vol. 13 (6), pp. 851-861; Rob

Wilson, R ; Loader, Nj ; Rydval, M ; Patton, H ; Frith, A ; Mills, CM ; Crone, A ; Edwards, C ; Larsson, L ; Gunnarson, B., 2011. "Reconstructing Holocene climate from tree rings: The potential for a long chronology from the Scottish Highlands", *Holocene*, Vol.22(1), pp.3-11.

Wilson, Rob, 2012. "[ITRDBFOR] Comment to Mann et al. (2012) at Nature Geoscience", 25 November 2012 20:43pm, Not available online.

———, 2012. Comment to "Lonely old Mann", *Bishop Hill blog* , 26 November 2012, 5:28pm.

———, 2012. Comment to *Real Climate*, "Global Temperatures, Volcanic Eruptions, and Trees that Didn't Bark", 8th of February 2012, Accessed 15 July 2015. http://www.realclimate.org/index.php/archives/2012/02/global-temperatures-volcanic-eruptions-and-trees-that-didnt-bark/

———. 2012. "Epson Expression 10000XL Scanner and Memory Issues," *ITRDB Dendrochronology Forum*, http://listserv.arizona.edu/cgi-bin/wa?A2=ind1202&L=itrdbfor&T=0&P=570. (accessed 5 September 2013, not available anymore).

Wilson, Rob, and Bates, Richard, 2012. "Lake sonar surveys and the search for sub-fossil wood", *Dendrochronologia* 30, pp .61–65.

Wilson, Rob; Rao, Rohit; Rydval, Miloš; Wood, Cheryl; Larsson, Lars-Åke; Luckman, Brian H, 2014. "Blue Intensity for Dendroclimatology: The BC blues" A case Study from British Columbia, Canada.", *The Holocene*.