Making air pollution visible: An ethnography of data practices in a multi-disciplinary public health project

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I, Emma Garnett, confirm that the work presented in this thesis is my own. Where information has been derived from other sources, I confirm that this has been indicated in the thesis.

Signed…………………………   Date…………………………..

Abstract

In this thesis I examine scientific practices which produce data on air pollution, and the ways in which these data are managed and co-ordinated by researchers to make claims about air pollution. In doing so, I attend to the everyday practices and experiences of scientific research, exploring the ways in which science is a social and cultural endeavour. Based on three years ethnographic fieldwork with a multi-disciplinary project studying the relationship between Weather, Health and Air Pollution (WHAP), I trace the local meaning of research, but also its implications, as part of a wider ensemble of environmental health and policy relations.

Managing different data and co-ordinating research practices was understood by scientists as a fundamental part of doing ‘collaborative research’. Collaboration was performed through the movement of data, and also became an ethnographic device by which I traced and followed the activities of scientists. Working with data was considered ‘real’ scientific work, and it was this appeal to authenticity that led me to examine data as a form of material practice. The craftwork involved in the production and use of data illustrates the embodied and tacit nature of research. The way in which these different types of knowledge were negotiated by researchers shows that ‘objectivity’ is situated, and that scientific legitimacy is contingent on the social and technical configuration of tools, technologies, people and standards.

Drawing upon research from social and cultural studies of science, I emphasise both the representational and performative shape of science. The reciprocal nature of data practices configure and enact accepted ways of seeing air pollution which make scientific claims appear as ‘natural’ and logical. These not only represent air pollution but mobilise policy norms and modes of intervening, and therefore particular ideas, values and power dynamics. The chapters in this thesis explore the collection, production, processing and re-use of multiple air pollution data, and how the relations - of technologies, people and ideas - imbricated in data mobilise air pollution as an environmental health concern.

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I was lucky to have a wonderful supervisor, whose time and support over the past few years has been enduring. I want to thank Judy Green for both teaching me so much about doing research and guiding me so carefully along the way. It was not only her intellectual contributions, but the genuine interest and energy which she showed for my project that made this PhD so enjoyable! I am also indebted to Paul Wilkinson for his open-mindedness about sharing knowledge, and for not only providing me with the opportunity to carry out my field work, but for his continued support along the way. I would like to thank Catherine Montgomery for sharing with me her expertise in STS, and, in particular, for introducing me to the study of ‘data’. Catherine’s insightful contributions have been invaluable, and I am very grateful for all the time, encouragement and thoughtful feedback she has provided me. Finally, I want to thank Simon Cohn for stepping in at the latter stages of my PhD, and whose contributions have been nonetheless invaluable. Not only did Simon help me consider my research as a ‘whole’, but he gave me the opportunity to talk through ideas with him in a way which was influential to the pulling together of this thesis at the end.

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Contents

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Abbreviations

ANT - Actor-Network Theory

AQI - Air Quality Index

AURN - Automatic Urban Rural Network

CO - Carbon Monoxide

CO_2 - Carbon Dioxide

COMEAP - Committee on the Medical Effects of Air Pollutants

DCLG - Department of Community and Local Government

DECC - Department for Climate Change

DEFRA - Department for Environmental and Farming and Rural Affairs

ESRC - Economic and Social Research Council

EU - European Union

HPA - Health Protection Agency

IPCC - Intergovernmental Panel on Climate Change

M&M - Modelled and monitored data

MRC - Medical Research Council

NAEI - National Atmospheric Emissions Inventory

NetCDF - Network Common Data Form

NOx - Oxides of Nitrogen

O_3 - Ozone

PI - Principal Investigator

PHE - Public Health England

PM - Particulate Matter

RR - Relative Risk

SNAP - Selected Nomenclature of Air Pollution

SO_4 - Sulphur Dioxide

STS - Science and Technology Studies

WHAP - Weather, Health and Air Pollution project

WHO - World Health Organisation

Chapter 1: An anthropology of air pollution

Introduction: The materiality of air

> Air is not a one, it does not offer fixity or community, but it is no less substantial. The question is whether we can feel it (Choy, 2012: 121).

Choy's description of air encompasses its materiality and immateriality, and its multiplicity and fluidity, inquiring, how do we feel this amorphous yet substantial thing? In his chapter entitled 'Air's Substantiations', Choy uses air as a heuristic to capture the many atmospheric experiences air provides, among them dust, oxygen, dioxin, smell, particulate matter, visibility humidity, heat, and various gases (Ibid: 127). His abstraction of air into 'atmospheric experiences' involves an interweaving of the multiple experiences air makes possible, producing what he calls a 'poetics of air'. This conceptualisation of air enables him to trace the particular and everyday experiences of 'honghei' (ambient air) in Hong Kong, alongside the scientific and technical practices which seek to measure and scale air as a universal category. These experiences, he shows, are different ways of *feeling* air.

I begin with Choy's descriptions of 'airy matters' because he captures both human material entanglements with air, and the different scales and registers at work in these entanglements. Sitting in the British Library writing up my thesis I am attempting to hold together the different scales of analysis my fieldwork has enabled me to examine. My fieldwork, an ethnography of a multi-disciplinary public health project based in the UK studying Weather, Health and Air Pollution, which I call in this thesis WHAP, was one such scale, where different air pollution data and health data were co-ordinated in a way that made the relationship between air pollution and health a statistically meaningful one. For WHAP researchers, the relationship between air and health is a means to study the parts of air considered 'polluting'. For these relations between particular components of air and health to be rendered visible in numerical form, however, requires measurements of air and health to be made in the first place. This is another scale and another way of getting a feel for bad air, that of the particular and local, where sensing instruments can be used to capture and measure an air sample in space and time. Yet, these local measurements need to be transformed into data, as validated, verified and standardised forms, in order to be used by different researchers at different scales of analysis. The co-ordination of these different scales of analysis and material forms in multi-disciplinary research, is demonstrative of the interweaving of multiple experiences of air, of feeling for air and making it visible as data and, ultimately, scientific knowledge. I interweave between these scales as an ethnographer and, like scientific researchers on WHAP, employ a combination of concepts and tools to help me get a feel for air as an object of scientific research practices.

In this chapter, I will introduce the WHAP project and provide a history of air pollution in order to frame my ethnographic study. Primarily, my aim is to provide a background to the chapters which follow and to set up the reader for some of the issues and arguments which ensue. A good starting point is air pollution's history because it works as a means for introducing some of the social, cultural and historical narratives built in and around air pollution as a contemporary public health problem. As Donna Haraway puts it, scientific practices are embedded in particular kinds of stories:

> Any scientific statement about the world depends intimately upon language, upon metaphor. The metaphor may be mathematical or may be culinary; in any case they structure scientific issues. Scientific practice is above all a story-telling practice in the sense of historically specific practices of interpretation and testimony (1990: 4).

Such narratives offer a socio-technical account of the increasing visibility and institutionalisation of air: from the smog which hung over Britain's industrial cities during the industrial revolution, so viscerally depicted by Charles Dickens[1], and the contrasting 'sea air', 'country air' and 'mountain air', which became a form of medicine for the sick in the 20th century[2], to its recent iteration as 'healthy air'[3] in

[1] Charles Dickens opening to Bleak House (1853 (1993))
[2] See Gregg Mitman's (2004) 'Breathing Space' for a social history of air-bourne allergies and asthma.
[3] The Healthy Air Campaign claims to tackle the public health crises caused by air pollution. Co-ordinated by ClientEarth (an advocacy group) and supported by a coalition of partners, the campaign brings together a coalition of health, environment

public health and in the related discourse of environmental justice. The story of air is increasingly described in relation to human activities and the result of the life-styles of modern industrial societies (Sloterdijk, 2009: 88). Subsequently, what stops air being 'good air' and therefore polluted air, is the result of a complex history, which orientates and structures people's relationships with 'the environment'. Air pollution, then, is a hybrid thing (Haraway, 1991; Latour, 1999) and the product of human influences which contribute to the conceived changing constitution of air and what counts as 'healthy air'. The technical transformations which have made air measurable are also prescriptive, compartmentalised into different species of particles and gases, configuring metrics and methods of measurement and accordingly the generation of scales of risk. By tracing a history of air pollution, I draw upon the narratives which give air meaning and substance, but also to how these inscriptions of air produced by measurement practices, embody particular repertoires of action.

The researchers on WHAP understand air pollution as both 'natural' and 'man-made', and also as something that is inevitable. The forms it take, however, as signified in the large-scale data produced on air, means that it is also a phenomenon that can be controlled through the changing of human activities, as 'something we can do something about' (Sam[4], Interview 7th November 2011). The complex relationship between air and air pollution is captured in the more commonly used term within science and policy, 'air quality'. Indeed, as one researcher on WHAP told me, you can never have air with no air pollution. As such, deciphering pure air from polluted air is impossible. Instead, the concept of air quality captures the *in-betweeness* of air and air pollution, and the material circumstances within which it is produced. At the same time, the concept of air quality is also a device that stabilises air pollution's elusiveness, as a linear scale from poor air to good air, thereby making the air in a particular location meaningful and comparable with other locations.

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and transport groups to increase public awareness of air pollution in a call for 'clean air'.

[4] All names are pseudonyms.

In this way, air muddies the distinction between subjects and environments (Choy, 2012), it is a natural-cultural form, yet also the very essence of life itself. Indeed, as Choy elaborates, air is not only an object of cultural commentary and not only a non-human materiality but something that is embodied through and through (Ibid). At one bi-annual team meeting held by the WHAP project, during which all team members got together to present some of their work in progress, the air quality of the meeting room became a topic of much concern. Rather than air being something represented in data, or as an abstract public health problem, air's presence, as something embodied and sensed by participants in the meeting room, was made explicit and perceptible. Several researchers commented on the high levels of CO_2 in the room and the lack of air flow. The meeting was temporarily halted, and those hosting the meeting quickly checked and adjusted the dial on the air and temperature monitor located at the back of the room. Although this is a slight detour, what I highlight with this short anecdote is that people's relationship with air are co-productive engagements, where controlling and changing air through particular actions, such as increasing the temperature or air flow in a room, shapes the kind of air - the quality of air - that is breathed and experienced.

Air is also something embodied through the particular scientific research practices which render air pollution visible as an object of study. The conceptualisation of air pollution as a cultural and natural phenomenon meant researchers made explicit the relationship between human actions and air pollution's formation. As laboratory ethnographies (Latour and Woolgar, 1986; Traweek, 1988) show, in practice 'the social' and 'the natural' can't be separated, and it is only through the construction 'of order form disorder' that these distinctions get made:

> It is because the controversy settles, that a statement splits an entity and a statement about an entity…reality is formed as a consequence of this stabilisation (Latour and Woolgar, 1986: 180).

In the scientific study of air pollution, work does not always take place in a laboratory. Instead, measuring air pollution was often done outside, as a bodily engagement with air, and one which is mediated through particular instruments. Further, this entangled configuration of air pollution as a hybrid form was also reflected in the very structure of the WHAP project, which required multiple kinds of disciplinary knowledge, the combination of which was understood as enabling a better representation of air pollution as data to be made. The subsequent multiplicity of data (as contingent and local), as both entities and statements about the world, was a useful way to suspend any given distinction between fact and artefact. Data are, of course, inherently fabricated forms, yet what they stand for, how they are used and the knowledge capacities they enable means they are also generative of fact making.

My aim in this thesis is to interweave the different research practices in WHAP by focusing on the different data they produce, and the effects this movement has on relations between researchers, and between researchers and the object of research air pollution. I consider this movement of myself between research practices, and therefore the measures of air pollution with which researchers engage, important to my own 'feeling for' air pollution as multiple data in action. My overarching theme is that of visibility - what do these data do in the process of coming to know, get a sense of, and make visible air pollution as a research object?

Air pollution as a public health crises

One way in which air becomes visible is through the media's representation of 'dirty air' as a nation-wide health concern. Indeed, air pollution is now claimed to be a 'public health crises'. According to a recent BBC report, air pollution is causing nearly as many deaths as smoking (Gallagher, 2014), highlighting the 'evidence' and case for intervention 'to protect the nation's health'. Finding a scientific link between smoking and lung cancer is a defining moment in the history of public health, and has become a powerful social and political force in the field of epidemiology more broadly. The effect of public health knowledge and action around smoking can be seen in the organisation of public and private spaces, most recently with the introduction of smoking bans in public places. These practices mean 'air' and 'health' are managed and controlled. What is more, comparing air pollution to smoking delivers a strong and powerful message, one that implies action: from the introduction of new classification systems and methods of prediction, to financial incentives for alternative fuels and the influencing of the public's use of vehicles and modes of transport. The lasting message of the report is that, because air pollution is no longer invisible 'the government cannot continue to ignore this issue' (Ibid).

Air is all around us and invisible to the human eye. It is a taken for granted resource, until air pollution, as a component of this air that we breathe, similarly invisible in many forms and under certain conditions, is made visible. Although we may see fumes emitted from a taxi's exhaust pipe or smog set in forming a haze over the city scape, or, as many people experienced, the recent Saharan dust storm in April 2014, in which a layer of reddish sand dust on the cars and pavements across many parts of the UK, what air pollution 'is' seems rather intangible. The visibility of air pollution in the form of Saharan dust however, provided the material impetus for a re-articulation of concerns over air quality in the UK. This was demonstrated in the wide-coverage of the storm by the media and in the government response, or perceived lack of. A trajectory of air pollution emerged with poignant statements, such as, 'the worst air pollution since monitoring began', generating a sense of de-generation despite technological and scientific developments.

The story of Saharan dust, or at least how it was retold by experts, is interesting. On the UK Meteorological Office (Met Office) website this 'remarkable' tale of how the dust travelled 2,000 miles from northwest Africa to the UK emphasised the role nature played in this epic journey. The Met Office explained that, Saharan dust is a mixture of sand and dust from the Sahara, the vast desert area that covers most of North Africa. Once it is lifted from the ground by strong winds, clouds of dust can reach very high altitudes and be transported worldwide, covering thousands of miles. It was the gale force wind conditions in the Sahara, of over 40 miles per hour, which, it was claimed, contributed to this most recent example of the phenomenon, with emphasis that this was by no means its first occurrence. As Paul Hutcheon, a Met Office forecaster said: 'we usually see this happen several times a year when big dust storms in the Sahara coincide with southerly winds to bring dust here'. Upon their high-altitude arrival to the UK, the airborne particles combined with the warm air were deposited during rain showers, and when the rainwater evaporates a thin layer of dust remains on surfaces like cars. The article ends by stating that Saharan dust is ‘a contributing factor’ to air quality, but only combined with pollution levels and weather, thus down playing the strong and rather vehement reactions by members of the public and the media.

In contrast to this tale of ‘nature at work’, the Guardian environment editor John Vidal, whose article on 3rd April read ‘the toxic truth about air pollution: a lethal scandal of British inaction’, re-told the story of Saharan dust from a different perspective, as ‘a crisis of our own making’ (2014). This story is presented as the untold story: ‘what we are not being told’ about air pollution, which comes from our own traffic, power stations, farming, construction sites, central heating boilers and industry, of which Saharan dust is only a part of this ‘foul mix’. Similarly, The Evening Standard called it ‘a dirty cocktail of pollutants’ (Prynn, 2014) emphasising the Saharan dust as just one example of a wider problem of high levels of air pollution in the UK from traffic and our reliance on cars and industry. Social media, such as Twitter, enabled the sharing of pictures of the dust online, thereby contributing to a shared and very visible experience of air pollution by the public. Underlining the political value in making air pollution visible, the Green Party even called for people to write messages in the dust (See Figure 1).

Figure 1: Hashtag Saharan Dust: the physical and digital mobility of air pollution (Nelson, 2014).

My own experience, and from those I chatted to during the Saharan dust episode in April 2014, led me to reflect on these 'active experiences of air' (Gabrys, 2013), and the different instruments used to tell us about air quality are connected. Indeed, several friends who are aware of my research got in touch, asking what the Air Pollution Index meant: was it arbitrary and aligned to the pollen index, therefore only affecting 'some people'. Should they cycle into work? Some also commented on how poor the government's coverage of the episode was, emphasising the ambiguity around air pollution as a public health problem and the wider 'politics of air'. Although these uncertainties around the Saharan dust problem may not tell us much about public conceptualisations of air pollution, they do illustrate the difficulty of experiencing and responding to air pollution as an often invisible problem. There is a gap between these active experiences of air pollution and the different narratives of Saharan dust, and, for example, the unresponsiveness from those in authority. Indeed, air pollution transcends state boundaries and transcends 'nature' and technology, which disrupts an easily identification of where responsibility lies. The inconsistencies in claims about air pollution, in terms of how and where it is produced and its associated health effects, reinforce the need for scientific knowledge to make visible the relationship between cause and effect.

This wider political context was in fact my initiative for beginning the present chapter. Although the Saharan dust frame was not the main concern for researchers on WHAP, it does work as a symbol of the invisibility of a far bigger 'air pollution problem', either as a natural phenomenon, as suggested by the Met Office, or as part of an endemic of increasing home-grown emissions, as claimed by John Vidal. Both narratives point to the subsequent ambiguities involved in sourcing, locating and responding to a conceptualisation of air pollution that is in continual re-emergence. What made air pollution a tangible problem and legitimate concern for the public, and subsequently those in power, was air pollution's visibility. Air pollution was made visible to the public on surfaces such as lamp-posts and cars bonnets, therefore enabling its movement into the political sphere.

The WHAP scientists were similarly involved in a process of 'making air pollution visible', as the process of data production and negotiation will explore in Chapters

Four and Five. They also handled the different repertories of action and prescriptive processes this visibility encourages in terms of policy making, which I will explore in Chapters Six and Seven. Visibility is also a metaphor that describes the effect of data practices, which materialise air pollution as a research object that can be studied by epidemiologists. I found that 'making visible' also nicely described the kind of analytical work done by the epidemiologists in their re-use of the multiple air pollution data. I therefore use visibility as a descriptive tool to trace the work of multi-disciplinarity and what the act of making air pollution visible - as an object of combined data and therefore different types of information - enabled. Indeed, the practical work of combining different data on WHAP was rather invisible in formal accounts of the project. For example, the Project Protocol only mentioned the technology used and researcher carrying out the process in passing, rather than making its presence a contribution in and of itself (as a work package). Like the effects the visible coating of Saharan dust generated, the combining of data in epidemiological analyses made air pollution visible as patterns in graphs, as statistical results and digital maps. The production of visual forms is made possible by a historical process of developing technical expertise in recording air pollution and high-tech developments in the instruments used, but also through scientific capacities to study and see complex research objects.

A socio-technical history of air pollution

Air pollution is becoming increasingly politicised as a result of this looming crisis. In March 2014, the WHO released a report claiming that air pollution is the world's largest single environmental health risk (World HealthOrganisation, 2014), and the EU designated 2013 as the 'Year of Air' because 'air quality has become a significant environmental health issue' (European Environment Agency, 2013). Air transcends political and state boundaries, which makes governance of air pollution tricky. For who is to blame and from where should a response be initiated and directed? Indeed, working out where pollution starts (for example, emissions) and where it ends (for example, increased mortality rates from air pollution) is an infinite question.

According to the historical account provided by the Department for Agriculture and Rural Affairs (DEFRA) on their website, air pollution has been a problem in the UK since the nineteenth century as a consequence of the coal smoke produced by Britain's industrial cities. This is most famously known as the 'Great London Smog' of 1952 and 1956. This cultural and symbolic reference point is often used to signify pollution at its worst, but also to highlight how the problem was tackled through policy legislation, initially with the 1956 Clean Air Act. A list of subsequent acts since and to the present are detailed, showing the measures which have been taken to overcome the problem of industrial pollution and the regulation and cleaning of urban environments. This narrative also highlights a major change between 'then and now', stating that 'today the emphasis has shifted from the pollution problems caused by industry to the ones associated with motor vehicle emissions':

> Within the UK, focus has shifted progressively to the monitoring of pollutants generated (directly or indirectly) from vehicular emissions, which include Ozone, Nitrogen Dioxide and fine Particulate Matter (DEFRA, 2011).

This statement highlights the nature of labelling pollutants in environmental science, which involves a three way linkage between source - pollutant - effects. Each component relates to the other in multiple and complex ways, so that the changing source of pollution shapes the contemporary pollutants of interest according to a particular policy domain, such as environmental health. This linear notion of policy progress toward cleaner cities is explicit in DEFRA's historical account of air pollution, which narrates this history through the collecting of monitoring data:

> The monitoring data have shown trends such as the dramatic decline in both black smoke and sulphur dioxide concentrations due to the introduction of cleaner fuels and technologies, and successful legislation (Ibid).

The development of new technologies in combating air pollution has encouraged an institutionalisation of air pollution, with the setting up of new organisations and research bodies which produce data on air quality.

In the UK, DEFRA, along with UK's National Atmospheric Emissions Inventory (NAEI), and in the past ten years the Intergovernmental Panel on Climate Change

(IPCC), demonstrate the governmental role in co-ordinating a local, national and global response to air pollution. Governing air pollution involves the recording of air pollution through measurements and computer simulations, and thereby the generation of data on air pollution. These data are orientated towards particular responses, which are co-produced in these material discursive formations used to govern air pollution. The practices of recording air pollution and producing data are generally categorised according to the instrument used to produce the data, either monitors or models, or according to what is being measured, for example, the measurement of emissions which produce air pollutants compared to the actual concentrations of the air pollutants in the atmosphere. In 1961 the UK established the world's first co-ordinated national air pollution monitoring network, called the National Survey. Over the years, several further pieces of legislation and additional monitoring networks have been introduced to measure air quality. Since the development of a nationwide automatic urban monitoring network in 1987, the network has expanded and consolidated. This has meant integrating local authority with national networks and also the previously separate UK urban and rural automatic networks to form the current Automatic Urban and Rural Network (AURN), claimed to be 'the most important and comprehensive automatic national monitoring network in the country... made up of 127 sites, across the UK'. This method of unifying and controlling air pollution is responsive to the different sources of air pollution that change over time. For example, policy directives from the European Commission shape UK legislation and therefore policy on air quality and associated standards.

The key pollutants of interest in air pollution monitoring and modelling are shaped by the perceived health and environmental risks with which they are linked. The Committee for the Medical Effects of Air Pollutants (COMEAP) is an independent body which provides information for central government on the health effects of air pollutants using 'health evidence'. The five main pollutants which are measured, modelled and evaluated in the Air Quality Index (AQI) are: Ozone (O_3), Nitrogen Dioxide (NO_2), Sulphur Dioxide (SO_2) and Particulate Matter: $PM_{2.5}$ and PM_{10}. One of the pollutants which has caused much concern in terms of negative health effects is $PM_{2.5}$, which are particles classified according to their size of two point five micrometres in diameter or less. These finer grain particles (in contrast to ‘coarse’ particles of 2.5 -10 micrometres in diameter) can be detected only with an electron microscope. The sources of fine particles include all types of combustion, including motor vehicles, power plants, residential wood burning, forest fires, agricultural burning and some industrial processes.

Policy initiatives to reduce air pollutants and the changing use of fuels in industrial combustion processes, for example, mean that air pollution has shifted in form over time. This is because the sources of pollutants are understood as shaping the kind of air pollution that emerges and is then problematised by public health. The changing dynamics of urban living, transportation mechanisms and the ways in which we heat our homes all influence the character of air pollution. As the 2011 Committee on the Medical Effects of Air Pollutants (COMEAP) report states:

> The current AQI covers PM_{10}, O_3, SO_2, CO and NO_2. As part of the review, we considered which pollutants should be included in a revised index. In view of the dramatic reductions in outdoor concentrations of CO and SO_2 since the current index was introduced, we concluded that an index for CO in ambient air was no longer necessary. We decided to retain an index for SO_2 as levels exceeding the current Low pollution band are occasionally experienced at certain locations (COMEAP, 2011: 14).

The Air Quality Index is used to communicate information about real-time and forecast levels of outdoor air pollution. These data collected at monitoring station sites provide high resolution data at an hourly rate, which are communicated to the public using a wide range of electronic, media and web platforms. Below is an example table of the air quality index, with the banding systems moving from green (a symbolic colour of clean air) to black (a symbolic colour of urban and industrial air). The current UK air quality index is a ten-point index, with one representing ‘Low’ and ten representing ‘Very High’ pollution levels. These ten points are organised into four bands: ‘Low’ (1–3), ‘Moderate’ (4–6), ‘High’ (7–9) and ‘Very High’ (10).

Band	Index	Ozone	Nitrogen dioxide	Sulphur dioxide	$PM_{2.5}$ particles	PM_{10} particles
		Running 8-hour mean ($\mu g\ m^{-3}$)	1-hour mean ($\mu g\ m^{-3}$)	15-minute mean ($\mu g\ m^{-3}$)	24-hour mean ($\mu g\ m^{-3}$)	24-hour mean ($\mu g\ m^{-3}$)
Low	1	0–26	0–66	0–88	0–11	0–16
	2	27–53	67–133	89–176	12–23	17–33
	3	54–80	134–200	177–265	24–35	34–50
Moderate	4	81–107	201–267	266–354	36–41	51–58
	5	108–134	268–334	355–442	42–46	59–66
	6	135–160	335–400	443–531	47–53	67–75
High	7	161–187	401–467	532–708	54–58	76–83
	8	188–213	468–534	709–886	59–64	84–91
	9	214–240	535–600	887–1063	65–70	92–100
Very High	10	241 or more	601 or more	1064 or more	71 or more	101 or more

Figure 2: Air Quality Index (Ibid: 33).

The history of air pollution is, then, made through the actions and interventions which turn air pollution into a measurable material phenomenon. This history demonstrates ‘a history of perceptibility’ (Murphy, 2006), where the material arrangements which govern air pollution work to produce the very object of which they are trying to measure. The history of air pollution can, therefore, be tacked on to these revelatory moments, where air pollution is made visible through particular technical and discursive interventions.

Modelling in contemporary climate science

The other way in which information is produced on air pollution is through computer modelling. Models are used to simulate air pollution in the atmosphere, at a scale and level of detail and coverage that can’t be measured manually. In Paul Edwards’ (2010) acclaimed historical account of climate science, he argues that studying the natural environment as a system requires modelling because without models there are no data, for everything we know about the world’s climate we know through models. Thus, the very notion of simulating the atmosphere reconfigures knowledge on phenomena because of the informational capacity this grants. Indeed, Edwards uses the notion of ‘global’ to demonstrate that not only do models make data global but enable global data (Ibid: 188). It is this latter point which resonates with the socio-technical history of making air pollution data because it is simultaneously about centralised and standardised systems of management, such as AURN, and the piecemeal processes of instruments practices and expertise.

Producing modelled data on air pollution involves running a simulation, which is a theoretical representation of the atmosphere in space and time, to re-produce the concentrations of particles and gases in the atmosphere. There are a number of different uses of modelled data. However, in terms of the history of air pollution, model simulation is significant because it can simulate into the future. This is particularly useful for policy making (for example, air pollution forecasting is a way to make data on ‘future air quality’ in the UK) because models enable the examination of multiple pollutants in interaction rather than as singular entities, like the output of monitoring stations. This latter focus is significant in terms of the WHAP project, where the study of multiple pollutants in interaction is claimed to be a novel feature of the project. As one of the senior modellers on WHAP told me, the main pollutants of concern are NO_2, O_3 and PM, the latter consisting of many different chemical species in particles of widely-varying size, and understanding these interactions is complex because they are largely produced by chemical reactions within the atmosphere, rather than directly emitted as air pollutants.

Furthermore, existing air quality standards focus on each air pollutant separately, to the extent that policy action to limit one pollutant can sometimes lead to an increase in another (Elizabeth, Interview 8th November 2011). This statement demonstrates the role these interacting agents play in the construction of air pollution as a research problem, which modelling can offer purchase on. Modelling as a particular kind of socio-technical practice both expands the scale of analysis whilst also making the intricacies of particular air pollutants visible. This shapes subsequent practices and methods of doing science, which suggests air pollution needs to be approached as an object of research that transcends scales of analysis, realms of practices, concepts of time, space and, perhaps, scientific ‘reality’ itself. As Edwards writes on the role of computer modelling in the construction of climate science:

> […] the real atmosphere had first to pass through this abstract gridspace, which transformed them into new data calculated for the model's demands. Henceforth all predictions would be based on these calculated or simulated data, rather than on actual observations alone (Ibid: 253).

What I point to in my discussion of modelling is the symbiosis of the different technological and scientific elements of this socio-technical history of air pollution. Air pollution is configured as complex through modelling, as a phenomenon that is contingent on large scale atmospheric processes. Yet, the local processes of air pollution remain in focus, where the standardisation of air pollution, as legal thresholds or as a health risk, make the particular and contingent essential in coming to know and manage air. As Choy suggests, perhaps it is best to consider air pollution as a 'collective condition', one that is neither particular not universal, but one which orientates us, 'to the many means, practices, experiences, weather events and economic relations which co-implicate use at different points as "breathers"' (2012: 128).

A multi-disciplinary public health project

As this brief history of air pollution attests, modes of measurements and related systems of governance contribute to the increasing number of measurements, data and methods of controlling and containing air pollution, which make it an increasingly visible phenomenon. This is a process which Choy has loosely termed 'air politics' (2012), to account for the co-operative developments of environmental monitoring, public health concerns and trans-boundary governance of air. Air politics captures the simultaneously particular and local with the universal and global, a symbiosis that I also highlight within scientific practices which produce data on air pollution. My focus is only on a very small part of this 'politics': I am concerned with the particular bodily and technical ways in which air pollution is made visible for scientific knowledge production and associated policy claims. The very act of measurement is entangled with a wider politics of air, such as around the public health concerns that poor air quality manifests. The Air Quality Index (AQI) is a way to make particular measurements universally readable, through which contingent thresholds of good and unhealthy concentrations of pollutants in the air are made.

By focusing on one scientific project, I trace the ways in which measurements get made and used, in practice, but also how classification and thresholds of pollutants govern universal systems like the AQI. WHAP was very much a part of a wider system shaping and reconfiguring these processes of controlling and governing air as a public health concern. Growing out of a pilot study, which involved several of the more senior researchers on the project, WHAP was the outcome of further, and increased, funding for the research team. Although smaller in scale and focus, single pollutant health impacts in London only, the original pilot study, referred to as 'baby WHAP', was drawn upon as a discursive resource by researchers to frame the emergence and rationale for their continued research on air pollution. This frame also works as a nice example of the expansion of health related concerns on air pollution, and the scaling process of scientific knowledge production through the structure and orientation of research funding. The extension in the larger project to 'multi pollutant analysis' and 'indoor and outdoor air pollution' across the whole of the UK required joining forces with several institutions. This resulted in the drawing upon different disciplinary expertise, and extending the research remit and diversity of collaborators. Indeed this expanded remit even included an ethnographic component, on which this thesis is based.

WHAP is defined by many team members as primarily an epidemiological research project situated in the field of public health. Using air pollution data, at a multi pollutant, multi scale and multi-environment level, the project examines the effects of air quality and climate policies on air pollutant exposures and health. Linking 'the environment' with 'health' is a key priority of the project, and one undertaken by the epidemiologists on WHAP, who connect large environmental data sets with population data sets in order to produce evidence on human exposure to, and the disease burden of, air pollution. The practices and processes involved in data production, connection and transformation into evidence defines the WHAP project as more than a purely environmental or health project. Indeed, bridging the environment and health, in terms of policy making, was also managed in the project through a specific work package, a formalised mathematical framework to assess the value of evidence and the uncertainties inherent to it, in the making of policy decisions.

The project was split across three cities in the UK, which I refer to as City A, B and C. The Principal Investigator (PI), an environmental epidemiologist was based in 'City A' and the co- Principal investigator (Co-PI), an atmospheric chemist, was based in 'City B'. In City A there were three different universities within which different research members were located, which I will refer to as 'The Institute', 'The College' and 'The Health Group'. At City B there was one university, which I refer to as 'The University' and in City C there was one university, 'University 2' because of its close affiliation with 'The University'. Of course the project also moved outside of these institutional boundaries. WHAP had a strong policy focus, with an advisory committee consisting of 'science-policy experts' in air quality. Each research group also had particular policy contacts, access to resources and other research groups, which all contributed to the research process. Throughout the subsequent chapters I will draw upon these 'other networks' of scientists, policy experts, technicians, data, technologies and resources, which were mobilised at different points in the carrying out of the WHAP project.

Each institution involved provided the expertise of a contributing scientific 'discipline'. This was understood as crucial to the research at hand. As one team member explained, you need to combine the expertise of all the disciplines involved to answer the research question:

> […]so you need disciplines to tell you what type of stuff is in particles, then if you want an estimate, so how much exposure does someone have living in Ipswich, what are they exposed to? […] we know how many people die in Ipswich, but there might not be a monitor there so you are going to have modellers to tell you, to derive a model for air pollution. So we might not have a monitor there but we know, because of the way the wind blows and where the emissions are coming from, we can tell you how much air pollution will be there. So you need a chemist, a modeller, you need as epidemiologist to be able to link the exposure and the health… so the question needs all those things (Peter, Interview 8th November 2011).

Here, air pollution is constructed as a multi-dimensional problem, the different dimensions of which relate to different disciplinary expertise and the combination of these were considered as providing a more complete picture of air pollution. The logic, then, of doing multi-disciplinary research, is deductive because the project is built on the premise that air pollution has negative health effects[5], expanded in WHAP to a multi-dimensional level. Thus, the very organisation of scientific practice, to study air pollution, is based on this classificatory infrastructure of 'pollutant - health effect', arranging the environmental scientists (atmospheric chemists, environmental chemists) and health scientists (epidemiologists and statisticians) roles accordingly.

The complexity of air pollution as an environmental problem is recognised in the shape and form of the WHAP project. The very organisational structure of researchers and technologies mobilised resources and tools for the drawing together of different kinds of information. As Fortun (2004) argues, environments and environmental problems are shaped by the technological developments of environmental information systems. Like the information system studied by Fortun, which measured and mapped local air pollution levels for different end users, the multi-disciplinary structure of the WHAP project enabled different systems of knowing to be used in combination for the making of particular claims. The very design of the WHAP project was understood by researchers as enabling the combination of different information, and thereby materialisation as multiple data, which are potentially capable of rendering a multiply visible air pollution.

Air pollution is defined as a multi-disciplinary research problem in the Project Protocol, because 'the *integration* of data from different disciplinary fields is a complex methodological challenge for research and policy' (Project Protocol, emphasis added). The logic being that, by bringing different data and the information they carry together, a more complex and multi-dimensional understanding of air pollution is enabled. It is the social and technical resources of multi-disciplinarity, and the resulting data, which are highlighted as productive in the protocol,

[5] As highlighted in the previous study, 'baby WHAP', researchers found a strong correlation between increased daily levels of an air pollutant and increased mortality on that day. This finding is mirrored in particular local forms within the epidemiological and atmospheric chemistry literature.

emphasising the material dimensions of bringing together different disciplines as well as different epistemologies.

One interesting early observation was that there is no shared definition of air pollution or health, despite these being the concepts and material outcomes which orientate the WHAP study: ‘health and weather related impacts of air pollution’. When I asked team members, ‘what is air pollution?’ the responses I received were ambiguous, highlighting the slipperiness of the very concept shaping their research:

> A definition of air pollution? Umm, that is really interesting I can’t immediately think. Well, pollution is erm, I’ll look on Wikipedia (laughs). Well pollution is compounds released into the atmosphere which have an impact on human health, and eco systems, and the air bit means these compounds are released into the air. That is the best I can do (Elizabeth, Interview 8th November 2011).

> I don’t think I can answer this as [I have] no expertise in air pollution (Jo, Interview 25th October 2011).

> That is a great question. What is a weed? A plant in the wrong place. What is dirt? Matter in the wrong place. Pollution is, gases and particles in the wrong place (Peter, Interview 6th November 2011).

The first quote illustrates the ways in which air pollution is considered polluting in terms of its impact on both human health and environmental systems. The compounds in the air which are polluting, are so because of their effect on something else. However, this suggests that what gets distinguished as a polluting effect is already pre-known, because it would be logical to assume that other components of air are not pollutants, and therefore what counts as polluting is dependent on their effects. This contingency is something drawn upon by Peter in the third quote, where he suggests that air pollution is ‘stuff’ that is in the wrong place, it is the context rather than the thing itself which is polluting. This conceptualisation of pollution is analogous to anthropologist Mary Douglas’ theory of ritual purity and pollution where dirt is ‘matter out of place’ (1966). The middle quote is helpful on this because Jo suggests that researching air pollution doesn’t require knowledge about air pollution. Again, air pollution only exists as a result of the context within which it is present (the researcher quoted in the middle researches buildings and dwellings), such as spaces where humans live and breathe. Therefore, if we follow Mary

Douglas' line of argument, researching, classifying and coming to know air pollution is contingent on the particular arrangements of humans and environments and the maintenance of these boundaries.

Distinguishing air and air pollution: classifications in practice

In her classic text *Purity and Danger*, Douglas argues that understandings and beliefs about pollution are culturally influenced and meaningful phenomena. Accordingly, like all classifications, pollution beliefs are part of an ideological system of representation, which construct social entities in ways that shape behaviour and produce us as people. Pollution is disorder:

> it spoils pattern, [but] it also provides the material of pattern... because it implies restriction...and from all the possible materials, a limited selection has been made and from all possible relations a similar set has been used (Douglas, 1966: 95).

Labelling things as pollutants is, then, instrumental in (re)producing practices that re-enforce such beliefs and cultural systems of meaning. As Carolan claims, recognising something as a 'problem' requires a pre-existing set of values as to what is 'normal', 'natural' and thus 'right' (2008), both in terms of what counts as pollution, but also to the patterns of human, material and immaterial relations which enact these beliefs. Understanding air pollution as representative of a wider social and cultural system of meaning requires understanding it as both an instrumental and symbolic manifestation of socially and historically contingent classification practices.

Douglas' structural theory of pollution compares patterns of ordering and rituals of profanity, and to show that what counts as polluting gets made and re-made through particular practices and patterns of behaviour. Her comparative approach argues that what counts as pollution is socially constructed: 'there is no such thing as absolute dirt: it exists in the eye of the beholder' (Ibid: 2), a point recognised by the scientists on WHAP. However, I found that this relativistic notion of pollution didn't capture the multiple and inter-weaving kinds of classification work that took place on WHAP. Indeed, in acknowledging that what counts as pollution lies in the eye of the beholder, didn't stop researchers studying air pollution. It was this ambiguity around what counts as air pollution that was the momentum for, rather than the impasse to, doing collaborative research.

In 'The Social Construction of What?' (1999) Hacking problematises the relativistic notion of social constructionism in relation to the philosophy of knowledge, with particular reference to 'the science wars'[6]. For Hacking, contingency in science is to be found in the framing of the questions, but once questions are framed the contents of science are non-contingent. If one follows Hacking's line of argument, it is important to pay attention to the practices and processes of science in the making rather than on the classifications themselves. Accordingly, it is the lively nature of classification, its construction, movement, effects and affects that distinguishes Hacking's philosophy of classification from Douglas' more structural account. In a later article entitled 'Kinds of people: moving targets' (2007), Hacking demonstrates the way in which classifications interact with people, affecting them with the effect of changing those kinds of people. By showing the performative effects of classification, he demonstrates the material and ontological shaping of classification, suggesting that 'our sciences create kinds of people that in a certain sense did not exist before. That is making up people' (Ibid: 293).

In terms of epidemiology, a defining discipline of the WHAP project and public health more generally, Hacking's work is useful, for it directly concerns the counting of people, their classification and the actions which perform these classifications. The social and cultural ways in which classifications are constructed are also performative, constructing populations and social groups as numerical forms, and as the potential starting points for prescriptive actions. With the example of 'obese kinds of people', Hacking shows that the act of 'counting people' enables quantification, correlation and thereby objectification through medicalisation, normalisation and administration (2007). Bowker and Star's now seminal work 'Sorting things out: Classification and Its Consequences' (1999) similarly claims that classifications not only interact with, but actually construct the very thing they seek

[6] The science wars took place in the 1990s, involving a series of intellectual exchanges between scientific realists and postmodernist critics about the nature of scientific theory and intellectual inquiry.

to classify. By demonstrating the material and discursive processes by which ‘kinds’ are made, the social and historical shape of classification is made explicit. Hacking and Bowker and Star’s work demonstrate the relations between place, people, labels, institutions and expertise, which compose, make active and sustain classifications over time.

The entwining of classifications and standards is relevant to the study of the scientific classification practices involved in studying air pollution. Indeed, in the subsequent thesis I trace a number of different, contingent and sometimes conflicting modes of classifying air pollution: as a natural or man-made phenomena, between urban and rural settings, between air pollution in Europe and air pollution in America, as a system of evaluation (the AQI), to the standardised thresholds which distinguish what levels of air pollution are bad for your health. Furthermore, these practices of classification relate to social and cultural orderings in the world, such as the spatial and temporal organisation of people and buildings, transport systems and urban infrastructures. As a result, air pollution is experienced by particular social groups dis-proportionately. Thus, air pollution in science and policy knowledge practices is a performance of multiple classifications across different spaces, times and peoples.

Classification is not only a constitutive part of practices and their material effects, but is also a way to examine the inter-relations between techno-scientific standards and procedures and the everyday work of scientific knowledge production (Bowker and Star, 1999). By focusing on the material and non-material processes though which classifications get made and re-made the complex network of relations becomes more visible. Indeed, it is the fluidity of these classifications and the very problem of fixing an emergent research object that makes the nature of the implicit classificatory category of air pollution a complicated one. I use the concept of classification as an analytical tool to give causal prominence to the small, implicit and taken for granted processes which underpin the playing out of knowledge production. Moreover, as Hacking argues, it is the construction of the question that is contingent, rather than the contents of the subsequent classifications. Studying the relationships between different classification practices then, is also a way to examine the way in which research questions emerge and stabilise. As such, classifications were also an analytical tool for the scientists on WHAP, in that they can be used to reveal and make apparent particular kinds of air pollution relations.

This discussion of classification has led me to my starting point for the thesis that follows. The point I want to conclude on, in terms of the performativity and liveliness of classifications in process, is that scientists on WHAP used classifications to make visible particular kinds of air pollution relations. This suggests a level of reflexivity, because what kind of classification to use involved reflecting on the kind of air pollution that the project was to produce knowledge on. How to order and organise air pollution as a research object was of interest both for me and researchers on WHAP. In the moment of observation a distinction[7] is made, a difference between the objectified thing and the stuff that resides outside of these boundaries. What is useful about reflecting on an observation (a second order distinction) is that it collapses any simple division between observers and observed, a practical reality for scientists working on a multi-disciplinary project. My interest, then, is in how differences get held together in particular ways, and therefore what classifications do when multiple classifications are at work. Accordingly, the focus becomes the actor making the observation, which assumes that what becomes form and what remains mess does not pre-exist the act of distinction. This means that, by studying scientific practices and processes I am not only studying scientific classifications in practice, but also air pollution in the making.

In the section which follows, Part I, I am going to trace the construction of research boundaries, both in terms of my fieldwork encounter and the theoretical approaches which shape my study. In the next chapter, I am going to introduce my fieldwork and discuss the ways in which I carried out a study of data practices in practice. Paying attention to the different constraints and capacities of following the multiple actors involved in knowledge making, I describe the ways in which I managed to trace the movement and transformation of multiple kinds of information, data and

[7] Luhmann's (2002) concept of 'second order distinctions' is of relevance here because of its ability to shift between scales of observation. For Luhmann, the act of observation can also be observed as an observation itself.

infrastructures. I will also discuss the methodological implications of studying science in practice, paying particular attention to the ways in which data mediate and co-fabricate research worlds and objects of research.

Part I - Constructing research boundaries

Chapter 2: An ethnography of data practices

Introduction: The 'Weather Health and Air Pollution' project (WHAP)

This ethnographic research is based on three years' fieldwork with a multi-disciplinary public health project that was studying the relationship between Weather, Health and Air Pollution (WHAP). The WHAP project was based across several universities in the UK, co-ordinated as part of the 'Environmental & Health Exposure Initiative' (EEHI) of a prominent research council. The EEHI initiative is a joint research programme between NERC, ESRC and MRC, and one of the first to combine 'human health' and 'the natural environment' in their call for bids. A larger project than its predecessor 'baby WHAP', the WHAP project brought together five senior investigators, and around ten contract researchers across five institutions in the UK[8]. As a result of further and increased funding, WHAP's research remit was more encompassing than the pilot project, focusing on multi-pollutants rather than one air pollutant, on human exposure to pollutants outside and inside buildings, and studying the whole of the UK instead of one UK city. This extension of their research remit highlights the expansion of health related concerns around air pollution. It also suggests the scaling process of scientific knowledge production through the structure and orientation of research funding in the UK, and how the actual building of scientific knowledge takes place.

The challenge of doing collaborative research between different disciplines and their knowledge boundaries was a topic raised by researchers themselves. Furthermore, as the anthropologist on the team, and therefore as someone with 'expertise' in understanding 'the social context', my role was often portrayed by other researchers as responding to the perceived difficulties of communicating across diverse scientific languages and epistemic cultures. The multi-disciplinary nature of the WHAP collaboration was described by researchers as relating to the different types of information and expertise which researchers brought to the project. These were

[8] There were around fifteen researchers on the WHAP project; this of course changed as the project progressed with new researchers joining and leaving as particular parts of the project were started and finished.

considered essential for the study of air pollution *and* health. Indeed, linking ‘the environment’ with ‘health’ was a key priority, and one undertaken primarily by the epidemiologists who connected large environmental data sets with population, health data sets in order to produce evidence on human exposure to, and disease burden from, air pollution. Furthermore, the practices and processes involved in data production, connection and transformation into evidence distinguished the WHAP project from a purely environmental or health project.

Researchers were located across five different institutions in the UK: ‘The Institute’, ‘The College’, ‘The University’, ‘University 2’ and ‘The School’. Each institution involved provided the expertise of a contributing ‘discipline’. This was understood as crucial to the research at hand. The research tasks were divided into six work packages in total and each work package was broadly split across disciplinary research groups (environmental epidemiologists and statisticians, atmospheric chemists, building physicists and social epidemiologists) and according to the particular types of data being produced. There was, in practice, a lot of cross-over between these research groups, as I will go on to show in this thesis, but formally the project was organised by institution and discipline.

The Principal Investigator (PI) of the WHAP project, Tim, was based at ‘The Institute’, and he had worked with co-PI, Elizabeth, based at ‘The University’, in the previous baby WHAP project. Their relationship was based on a shared interest in air quality and health, the study of which was understood as relying on contributions from their distinct knowledge and expertise. Tim’s research group was based at ‘The Institute’ and ‘The School’ (both in City B), and included another senior epidemiologist, a professor in statistical epidemiology, Peter, and a more junior researcher called Ann. This small research group, however, also involved other colleagues within the same field and whose contributions could be observed in the project output as names on papers, even if they were not recognised as formal contributors to the WHAP project in the protocol. The topic of air pollution linked to other areas of work by the research team and points to the often arbitrary boundaries of a singular project. This expansion of labours was similar for the other research groups. Elizabeth’s research group, based in City B, consisted of three other atmospheric chemists with expertise in the modelling of atmospheric relations, and whose research on global atmospheric computer modelling was well-renowned. Tom, Craig and Sam from The University played central roles in both the production of modelled data and the construction of future scenarios for policy, and were active in weekly meetings and multi-disciplinary discussions.

Another research group based at 'The University' were the environmental chemists, whose research was field-based rather than digital in nature (like computer modelling). Their office building was situated next door to the modellers in City B and during my field work I met with both research groups. These were separate meetings and each department was distinct, yet their relationship was slightly different to those held with the other research groups because the environmental chemists' contribution to the WHAP project was to validate the modelled data produced by the atmospheric chemists. Chris was the lead investigator for this validation work package. The actual collection of field data was carried out by a post doc researcher called Elliot. Both Chris and Elliot were in close communication with the modellers and the epidemiologists because their data analysis comparing the data produced by the model and the data collected in 'the field' contributed to the epidemiologists' use of this data in their own analysis. My focus in this thesis revolves around the inter-relations between these three disciplinary based research groups.

There were also two other research groups in WHAP, and I will briefly introduce these researchers because they played a formative role in my understanding of the WHAP project more broadly, even if I didn't ultimately focus on their data practices in detail. I spent much time with the indoor building modellers at 'The College'. The weekly team liaison meetings took place at 'The College' in City B every Monday, and over the two and a half years I carried out my field work I got to know many of the researchers quite well. The senior building physicist who overlooked work package two - 'Developing markers for indoor temperature and pollution' - called John, worked closely with Tim on several other projects relating to buildings, air pollution, temperature and health. Indeed, during the weekly liaison meetings at 'The College' WHAP was one of several different collaborations led by Tim and John with over-lapping themes, such as the relationship between buildings characteristics and health impacts. These long-lasting professional relationships between the lead investigators on WHAP point to the lifetimes of academic projects and the concomitant successful access to research funds. During my time on WHAP bids were put in for new projects and successful bids were taken up as collaborations between ‘The Institute’ and ‘The College’ were sustained. The final institution where senior investigators were based was The College, where building physicists were based. Although I am not going to focus on the building physicist’s data practices in this thesis, their role in my research was formative in my settling in to the project and my experience of the everyday practice of multi-disciplinary, multi-institutional collaborative research.

The other research contribution, which I do not discuss in detail was the quantitative analysis of the socio-economic differentiation of exposure to air pollution, carried out by researchers based at The University. This work package was one of the final to take place in WHAP, and involved using the epidemiological data alongside data on socio-economic status classifications. It is significant that the relationship between air pollution and health was studied in terms of their socio-economic differences. Indeed, it points to the ambitious focus of the WHAP project in its aim to link air pollution, health and ‘environmental injustice’ together. Furthermore, it highlights the social and political motivation behind the study of air pollution. Although much of my research was carried out at the weekly liaison meeting and the different institutional research sites in City A and City B, I also focus on a process referred to as ‘stakeholder engagement’, which involved several meetings with policy makers (referred to as ‘key stakeholders’) in the field of air quality. Air pollution is a political concern and priority in contemporary environmental health policy making, and the engagement with policy makers from early on in the project points to the co-generation of knowledge on air pollution between science and policy. The political nature of air quality, as I highlighted in Chapter One, is also shaped by the production of scientific knowledge.

In Part II and Part III, I am going to focus on the data practices of the epidemiologists based at ‘The Institute’ and ‘The School’, and the atmospheric chemistry modellers and environmental chemists based at ‘The University’. My main field site was the weekly liaison meetings held at ‘The College’ where team members met both physically and remotely via video conferencing, and during which updates on work packages, technical issues and future planning took place. This was the key method of bringing the different research groups together in WHAP and I found it a useful place for me to join the team because it was an open site. Moreover, the liaison meeting was a space where data practices were talked about, results shared and meanings negotiated. As researchers were not experts in all the different disciplines on WHAP, the liaison meeting was a great location for an anthropologist, where many of the implicit, subtle and tacit assumptions which underpin scientific work and expert knowledge were made explicit. Indeed these discussions were often useful starting points for topics, inciting my interest and becoming useful reference points for subsequent conversations and meetings with individuals outside of the liaison meeting.

I visited the different research groups on WHAP and attended, when I could, internal disciplinary research group meetings held at the different institutions. In general, my field work involved moving between the multi-disciplinary spaces of the liaison meeting to the disciplinary sites of data production. A primary communication method used by the researchers was email because of its speed, ease and means for sharing documents which were in process. Email made boundaries visible because it made the connections being made, crossed and re-made in particular instances tangible and visible. At the same time, following email traces was incredibly difficult because they were sent informally, and only to selected colleagues, often with the purpose of continuing discussions which were not considered relevant to other researchers on the project. This move therefore also established small research groups, which were used as a way to achieve consensus or make a decision on a topic of shared interest.

The few occasions I was added to an email thread were informative of the kind of work that permits movement and negotiations in-between data practices. I wasn't considered relevant to many of the often quite technical and specific communications, and indeed did not participate directly in them. However, when I was 'cc'ed' into email conversations (often as the result of a recent request and it being sent by a very open researcher, or because team members considered it of interest to me), another means by which work was done and choices made became visible. For example, I joined an email discussion between the modellers and the epidemiologists, which was concerned with the metrics of modelled data and how these relate to certain measures of pollutants. This eventually led to the formation of a ‘wiki space’, highlighting the generative process of email communication between researchers in the building of shared spaces of work (the wiki space was intended as a continually updated reference point for the project). Often discussions involved adding parts to an ongoing shared document, attaching relevant academic papers or demonstrating results from work in progress.

The other space where I carried out my fieldwork was at the weekly liaison meeting, which took place in a basement meeting room at ‘The College’. Present team members sat around a large table and in the middle of the table was a microphone (the subject of much debate about size, capacity and volume), a selection of biscuits and coffee. The central focus was two large screens at one end of the table, where the web conferencing screens were located, in front of which team members spoke. The web conferencing was managed by one (‘who’ managed the web conferencing changed throughout my fieldwork) junior member of the team based at The College via a laptop. The laptop’s desktop was displayed on the big screens, visually sharing presentations slides, emails or results with all participants. Conferencing software enabled those who were not present at the meeting to join in remotely. ‘Who was present’ at the table and ‘who called in remotely’ was generally consistent and dictated by geographic location. Two institutions, The University and University B, were based several hundred miles away from each other and the conferencing tool was a means of reducing physical distance, creating a shared field site both for other team members and myself. The web-conferencing technology was, then, a practical tool, but one which played a formative role in the realisation of work across different spaces of practice.

The different modes of scientific work also shaped my ethnographic research practices and the types of knowledge practices which came to the fore. Beaulieu (2010) uses the term ‘co-presence’, to describe a strategy that pays close attention to the mediated dimensions and distributed nature of knowledge production. Here, co-presence could be considered as both an ethnographic practice and in vivo practice, in order to emphasise the active process of ‘field making’. For example, as my participation in the weekly meetings continued, my focus became the movement of data. One of the key reasons I focused on data practices, and the movement of data between researchers was because it seemed to perform ‘collaborative research’. The practices and processes involved in collecting, producing, processing, sharing and using data was the way in which the different research groups were able to ‘do’ multi-disciplinarity. This meant that by following data I was able to trace the ways in which multi-disciplinary scientific research was done in practice.

The internal meetings held for only single disciplinary research groups were harder to access and I came to realise that it was at these sites where data were shaped, moulded and processed in new ways, which enabled their subsequent movement between research groups. By focusing on data practices, but also the movement of data between data practices, I gained a better understanding of the life of data and the ways in which data were made to ‘live together’. Data were also considered as legitimate objects of research for me to study as an anthropologist. Researchers were often keen to show me the outputs of their work, usually in their visual forms as elaborate maps or graphs. Data were accessible as material things and they were objects that could, after the right kind of processes had been performed on them, move between research spaces aiding the explication of complex theoretical workings.

Having a material form to follow and zoom in on was useful when feeling slightly lost in a rather fluid and unfixed field site. Furthermore, by sharing my interest in data I was taking seriously the everyday interests and concerns of researchers on WHAP. This led me to appreciate the reasons why data played such a fundamental role for researchers, which I am going to detail further, but primarily as not simply the arbiters of knowledge but as the means by which scientific and non-scientific relations were mobilised and sustained.

Data were also a way in which relations were, at times, de-mobilised, and through which the boundaries between research groups were re-drawn. As I have highlighted, there was the liaison meeting which was an explicit ‘multi-disciplinary space’ but also research groups’ internal meetings, which were private and harder to access. The organisation of these different spaces where WHAP research took place was materialised by particular actions with and on data. For example, in internal meetings early data sets were discussed and particular research hypotheses tried and tested. The tentative results were often shared with other team members at the liaison meeting, but before this was done research groups had their own discussions and nearly always settled their own internal disagreements first. This allowed particular perspectives and controversies to be shared and stabilised, before a different disciplines perspective was drawn into the debate over data's meaning and subsequent use. It also highlights the kind of boundary work that gets done in collaborative research, as well as the different modes of doing science which become visible when one studies everyday scientific practice. I found that 'being collaborative' worked alongside, rather than in opposition, to the maintenance of single disciplinary research spaces and their concomitant ways of knowing.

WHAP was part of a much larger infrastructure of people, institutions and technologies, which enable the production of data on air pollution at a local and national scale. Data, and how to combine and translate data across different sites of practice, was a key concern for scientists on WHAP. Although I focus on the different disciplinary and multi-disciplinary knowledge production practices on the project, my ethnography is of data practices as an ethnographic object of concern. It was the different translations and transformations that data underwent that proved the most insightful to my understanding of the different knowledge practices of the scientists on WHAP.

Refining my research questions: three empirical puzzles

I introduced myself to researchers over email within the first week of starting my PhD, during which time I applied, successfully, for ethical approval from the London School of Hygiene and Tropical Medicine's ethics committee. My joining the project was not a surprise to participants because my PhD position was written into the funding bid and my supervisor had introduced my role at the first collaborator's meeting during the summer of 2011 (before I started that September). Of course, what ethnographic research meant in practice was difficult to anticipate for researchers, as it was for me. For this reason, and as way of a less formal introduction, I suggested meeting each researcher for an introductory interview. This worked well because it allowed me to introduce some of my initial thoughts and ideas, whilst opening up a space for researchers to ask me questions and voice concerns. The transcription and analysis of these early interviews were also fruitful as a starting point for thinking about and beginning to conceptualise some shared concerns around the multi-disciplinary nature, shape and form of 'collaboration'.

As I proceeded with my fieldwork, moving between different sites of practice and joining in on the weekly liaison meetings, three principle observations emerged which are a helpful as an introduction to how I organise this thesis. The first, which I outlined above, concerned the question 'what is air pollution?' What is interesting for scientists in WHAP is not what air pollution is or what it means as a research subject, but what it does as a research object. This is an epistemological finding and one that emerged because of my own practices of knowledge production. I assumed that I would be finding out what air pollution does, as a research object, and the scientists finding out what it is. Instead this was inverted, something I develop throughout the chapters to follow by tracing the role data play in knowledge production. For scientists on WHAP, air pollution exists and, although distinctions are made between types of air pollution (for example between anthropogenic and non-anthropogenic air pollution), that air is made up of particles which are harmful is a fact. When asking team members to define what air pollution is, the question was considered almost irrelevant and I received a range of rather ambiguous responses, as illustrated in chapter one: 'what is a weed, a plant in the wrong place, what is dirt, matter in the wrong place. Pollution is, gases and particles in the wrong place' (Peter, Interview October 2011).

That there was no shared understanding of air pollution initiated my second research question: how does work get done when the object of research is so ambiguous? Air pollution could be considered a boundary object, as something that brings together different disciplines and fields of practice around a shared object of concern. Yet, there was no consensus around what air pollution is, nor a clear research object defined by researchers, indeed there was no shared air pollution. It wasn't simply that air pollution had a degree of plasticity to allow for consensus, it was not even a question which was asked. That air pollution could not easily be categorised as a boundary object was an ethnographic puzzle which was imbued with another seeming paradox; that team members had quite stringent confines in terms of what counts as ‘real work’. The everyday setting of the team meeting was not considered real work but making data were. My second methodological and theoretical finding is that what counts as ‘real work’ has implications for the carrying out of my ethnography - for how do I follow science if science isn’t apparently happening? In terms of the latter, I began to consider what counts as ‘science’ and ‘non-science’ as particular ways of doing science, and therefore as boundaries and cuts made by researchers with material and non-material effects and affects.

The initial demarcation between ‘real work’ and ‘non work’ also led me to the sites at which data were being produced - ‘data production’ was considered ‘real work’ by team members. I noticed that time at the weekly liaison meetings was dedicated to updates on data production processes, and as the project progressed these focused more on the processing, use and re-use of this data, ensuring it was useable across the different sites of practice within WHAP. However, data was not only an everyday concern, but the material form of scientific research, through which the team communicated both with myself and the wider scientific and policy community. For example, the visualisation of data, as maps of air pollutant concentrations across the UK, or time series graphs of observational and modelled results, produced a shared work space, where different articulations of air pollution within data could be considered together. This is because the ‘real work’ which materialised air pollution data, was attached to specific scientific groups and sites of production. ‘Non work’, in contrast, was about working out ways to engage with these different sites of production - ‘the logistics’ - and data became a way to transgress these divisions, both for myself and for the researchers on WHAP. In terms of methodological implication, data became a way to observe and participate in both ‘real’ and ‘non-real’ work, and the nuances between these conceptually different practices led to my own theorising on ‘science’ as a form, affect and practice.

During the first few months I spent with WHAP I was repeatedly told that the weekly liaison meetings I attended along with other researchers was not where ‘real work’ took place. Indeed, the considered administrative discussions were dedicated to the logistics of co-ordinating a multi-institutional collaboration. However, these discussions and actions taking shape through ‘logistical work’ proved fundamental to the knowledge production process. They demonstrated the practical ways in which a multi-disciplinary project functioned on a day to day level. The weekly liaison meetings, regular emails and co-ordination between the senior researchers on the project developed through the building on relationships established through previous research were, it seemed, fundamental to the success of ‘real work’. Indeed, as I will go on to argue, this ‘ad-hoc’ work is constitutive of, rather than contributing to, the production of scientific knowledge.

Real work was the work of collecting and generating data. This appeal to authenticity led me to trace the trajectories of data. Data are both an ethnographic thing of interest and a thing of interest for my informants (Bowker, 2010; Hilgartner and Brandt-Rauf2, 1994; Hine, 2007; Walford, 2013). As Henare et al. state (2007: 11), to take others seriously, and thereby not reduce their articulations to mere perspectives and beliefs, requires a point of cultural convergence. I entered the social, technical and material spaces of the researchers on WHAP by thinking through data as an object, and as a point where different worlds met: my own, but also those of the different scientific groups in WHAP. Data carved out spaces of shared practice which were generative of new articulations and potential repertoires of air pollution. Accordingly, I was able to attend to the empirical questions which emerged for my informants, who did not seek to explain why data stand for air pollution but rather, if this is the case, then what air pollution are they are talking about? (Henare et al., 2007: 12). This is because in articulating air pollution through data, air pollution was both experienced - in the act of working in data, and conceived - by thinking with it.

Orientating my subsequent steps I began to look at the material practices of scientists in their endeavours to produce knowledge on air pollution, both because it was considered ‘scientific’ by researchers and because it suggests an act of making information on air pollution material. As I have already described, what counted as real work was the production of, and the playing with, data. I therefore traced the different work practices which produce data on air pollution. I was struck by the multiple ways in which air pollution data could be produced, the diverse technical instruments used, and the embodied, sensory expertise mobilised in the process of making data. My third finding, then, is that there are multiple data and multiple air pollutions configured by particular scientist-data-air pollution relations. This third finding relates to the first, air pollution is an ambiguous research object, but its materialisation through data complicates this conceptualisation because data is also heterogeneous and multiple. Indeed, what 'data stand for air pollution and what air pollution stands for data?' emerged as an empirical question for WHAP researchers (Chapter Four and Five in this thesis).

I am wary of making the case for a general ontology, but what I am engaging with could, perhaps, be called 'actor-ontologies' of air pollution: particular data practices conceive particular air pollutions, which are different things in the WHAP project. Although I have drawn upon the radical constructivism of Henare et al., I have used this as an ethnographic tool in my encounter with different air pollutions through different data, which are conceived in different scientist-instrument-data relations. Particular air pollutions are embedded within data and these multiplicities were mobilised within the WHAP project. The subsequent practice of what I call 'data-in-negotiation', during which data were considered in their movement between different sites of practice, involved conceiving of air pollution differently. Difference enabled a space for sharing data whereby research groups (who conceive air pollution differently through data) came to see, appreciate and engage with different air pollutions (and thereby participate in re-creating them). This third finding attends to this subsequent collapse between 'data' and 'air pollution', which, as researcher's ambiguity around what counts as air pollution suggests, is not simply a division between 'representation' and 'reality'. Instead, data practices often work in-between representations, and were therefore a way to appreciate ethnographically the mutual configuration of data and air pollution in practice.

Further, that data are materially heterogonous generates analytical problems for scientists on WHAP, which meant that data became an analytical tool for researchers too, used within the project as a way to reflect on how data can be used and re-used to 'say something' about air pollution (or equally for air pollution to say something about data). This suggests that data were not merely a way of representing air pollution but functioned according to the principles underlying data, constitutive of multi-disciplinary knowledge practice and 'collaborative' relations of knowledge production. That data were both the means by which relations were established and maintained, and also the epistemological barrier for communicating across difference puts data in a very interesting positon: data were both the means and the limits of collaborative practice. My ethnographic challenge here can be framed as: how do data practices engage with multiple kinds of data? Or, in the process, how do they modify data in ways which enable data to maintain and sustain collaborative relations? And further, when do data function by obstructing, rather than enabling, a shared trajectory of data-as-air pollution? In other words, how can data play this dual role of being both a sticking point but also the very objects through which collaboration takes place?

I have traced three empirical puzzles here. Starting with the slipperiness of the object of research, I suggest that there is no shared air pollution but multiple air pollutions. I then move on to how this ambiguity around the object of research gets managed in day to day practice. This led on to my claim that the different spaces of science are demarcated in order for differences to be contained and therefore managed. My third finding was that data constitute disciplinary identities whilst also helping to create interstitial spaces of practice. It was this in-between and liminal state that data-in-negotiation materialised that I found was productive to the crossing of different socio-material boundaries. Moreover, data enabled the emergence of practices which were uncertain and experimental, so that where particular boundaries should lie could be iteratively worked out by different disciplines and researchers. Thus, data took on a useful role, both for myself and researchers on WHAP, making visible the inter-relations between sites of practice, disciplinary identities, epistemologies, cultures of knowing and their particular material arrangements.

Studying knowledge production and being ‘collaborative’

One area of considerable running debate in the project provided a good case study for studying the relationship between data and air pollution was the ‘modelled and monitored data tension’. The story of modelled and monitored data became a way to trace a number of different processes and associations which worked to diversify my fieldwork process. It led me to different sites of data practices, the multiplicity of which extended beyond what I was capable of following. In WHAP, the employment of analytics such as ‘scientific communities’ and ‘different languages’ by team members made explicit reference to the different ways in which researchers approach and talk about air pollution as a relative research object. As I have already highlighted, the particular configurations of data, and how these were negotiated between sites of practice, suggest that different data can be epistemologically different things, relating to a particular kinds of air pollution.

WHAP was a shared space of knowledge production, mobilising different epistemic cultures (Knorr-Cetina, 1999), skills and expertise in the production, processing and movement of data between different research practices. The specific arrangements of 'people-in-places' affected how I came to consider the practice of multi-disciplinary, collaborative research. On entering the sites of knowledge practice of the multi-institutional WHAP team I soon realised that in everyday practice research required quite insular, disciplinary based data practices. At the same time, these situated practices mobilised a wider set of associations: with funding agencies, research groups, technologies and policy relations, which seemed to expand into an expansive map of connections and associations. These connections between multiple scales meant my field site was constantly shifting. I traced the movement of data within the WHAP project but also the external connections these data made at each stage of its trajectory: from the validation practices which connect data between sites of practice and scales of analysis, to the transformation of data into evidence in the science policy setting of the 'stakeholder engagement'.

My access was limited to particular research sites, or moments of research more specifically, for a number of reasons. Primarily this was because research groups working on particular components of the project (work packages) were generally located at the same institution and therefore meetings were not always planned and discussed with other team members. Secondly, these sometimes remained internal to the research group and because I was a social scientist, my presence was not considered appropriate. This was legitimated by reference to the importance of having private spaces where scientists could 'thrash out preliminary analyses' without making these public (to myself and other disciplinary groups within WHAP). However, this was reflective of a more general demarcation of working sites as sites for only certain research groups, so that at times, who was to be present or who to share a document or data set with was made explicit. Nonetheless, the carving out of private spaces did pose a problem of access. For it was these internal working group meetings where the ‘real work’ was considered as taking place, and the theoretical questions and practical problems ‘thrashed out’ and worked through - in other words where ‘science in action’ emerged.

The challenges involved in carrying out ethnographic field work alongside researchers and the work of negotiating access to the messy realities of everyday scientific practice also raises the issue of group consent. Most members of WHAP were very open to my presence, generous with their time and showed continued patience. However, the few occasions when team members were not happy with my fieldwork (of course I am only aware of the times this was made explicit to myself), did raise a number of problems. This is because the team members who were unhappy with my presence had to state this overtly, against the seeming wider consensus of those agreeing to my role. The PI was also in a tricky position in that my position as ethnographer had already been agreed in the writing of the funding bid, and therefore only by those scientists in senior positions. The arrangement was set before many of the more junior researchers had even joined the team. As a result, I was continually negotiating access with scientists who joined the project over the period I carried out my field work[9].

WHAP was a shared space of knowledge production because it mobilised different epistemic communities, skills and expertise, and the production and sharing of data between associated sites of practice. The importance of co-ordinating different sites of practice necessitated the construction of a fixed site for the project too, which came in the form of the weekly liaison meeting, through which much of my field work took shape. Web-conferencing also worked as a symbol of collaboration in its own right, and was often used by team members as a way to talk about the different limitations - institutional, disciplinary, spatial, and personal - of the project. In considering what a collaborative relation may mean for team members also re-orientated my own field work practice. For data enacted collaborative work, but also led to the continual reconfiguration of the carrying out of research at particular sites

[9] A process akin to Candea’s ethical-analytical modalities (2007: 248) as a means to generate both good data and good relations.

of practice. The different kinds of disciplinary work (real work) and collaborative work (logistical, non-work) led me to consider the different sensibilities which facilitate collaboration[10].

As I have already stated, email was a method of multi-disciplinary practice for researchers. However, I also used it to generate my own fieldwork opportunities. This was particularly useful when writing up my observations of data practices, such as the running of computer simulations, or the construction of a GIS map. Following a thorough reading of my fieldnotes, and attempts at formulating a sensible account of these complex technical processes, I frequently shared my write up with the researcher who I observed. On several occasions, I also emailed individuals with specific questions on things that were not clear to me when I got back to my desk. This seemed to work quite effectively, and I found that many scientists offered useful and insightful additions, and indeed elaborated on aspects of their work which remained silent when I was physically present. This suggests that email is a useful tool for explicating complex parts of scientific work and their reflective dimensions, which may not emerge in a physical research site context. In addition, this helped me understand why emails were such a popular choice for communicating between team members, for very often my ability to understand what was going on in 'real time', meant the questions I wanted to ask emerged after the face-to-face encounters. Emails therefore enabled the formulation and gathering of my own thoughts, something which I then picked up on as taking place within the team, where researchers often ended complex discussions with a note 'to put "that" down in an email'.

[10] Like Riles' use of the network form, by bringing informants conceptualisation of work into view I began to consider the two-fold character of collaboration, and the ways of 'doing together', 'joint thinking' and 'information sharing' that interdisciplinary research implies. Collaboration, then, is a relational and epistemic mode (Marcus, 2013). It is a theoretical and methodological frame which is drawn upon in Adolfo Estalella's (2014) call to consider collaboration as an experiment, a socio-material craft of devices (cf.Rheinberger, 1997), which enables a rethinking of the modes of engagement we deem as collaborative. This is productive, and as Marcus (2013) highlights, collaboration with our informants allows us to pose new questions, ones we may have not considered before.

Defining the field: a multi-disciplinary endeavour

The introduction of the term ‘multi-sited fieldwork’ by George Marcus (1995) in response to the perceived changing contexts of anthropological research was an attempt to reinvent the aesthetic and culture of method. This has meant focusing on how the field site materialises in research as a process of co-construction with one’s informants: a sensibility that reflects my own ethnographic experience. In anthropology ‘the field site’ has been the subject of much reflexive concern, which serves as insight into the contemporary status of anthropology and the epistemic value of its work. This has recently given rise to the interesting concept ‘para-ethnography’, used by Marcus (2013) to conceptualise ethnographic practices which are analogous to the ethnographer’s informants’ knowledge practices. Indeed, the different epistemological concerns of my researchers on WHAP were often similar to the internal debates within anthropology and the social sciences, around the objectivity, partiality and perspective of knowledge practices.

I initially considered my field as quite restricted, in the sense that I joined an already established project, made up of participants with quite strong social ties. Yet within the first few weeks of my fieldwork starting, this assumed total unit came under question. There was no shared definition of air pollution and scientific practices were bound by discipline and institutional site. Knowledge practices were distributed and team members identified with different research interests and wider scientific communities. Comprised as part of this matrix of difference and non-coherence, the subsequent chapters examine what it means to collaborate in multi-disciplinary research, both as an in vivo practice and anthropological endeavour.

The way in which this ‘siting process’ took place is interesting because it raises a number of ethical and practical constraints of carrying out ethnography in, what appeared to be, a ‘ready-made’ field site. Matei Candea suggests paying attention to the self-imposed limitations of ‘boundedness’, critiquing the unboundedness of the multi-sited imaginary relayed by Marcus, and the ethnographic freedom that it implies (2007: 167). Candea redefines ‘the field’ as an ‘arbitrary location’, as a site which has no overarching meaning or consistency but is rather ‘a contingent window into complexity’. For, any field site is always intrinsically ‘multi-sited’ (Ibid: 175) and thus, the bounding of a field site is a 'cut' necessary for ethnographic analyses to take place. Candea's insight led me to reconsider my own field site making process, as both expansive and restrictive in the explicitly multi-sited research of WHAP. 'The cut' was already made for me as a member of the WHAP project. However, rather than the problem of 'too many invitations to interesting field site spaces', as relayed by Candea, marking out my field site was more of a tentative process of co-constructing legitimate spaces to carry out my fieldwork. Indeed, at times my presence was considered inappropriate, and work practices which took place within disciplinary and institutional groups could not be accessed. As Corsin-Jimenez describes (2003), this can lead to a more confined field site than Candea suggests, proposing a more circumscribed notion of 'the field' as a shared space of knowledge production, highlighting the specific constellations and arrangements of people-in-places affect the routes we take, and how we come to think about such arrangements.

My ethnographic research was explicitly shaped by the marking of boundaries between work practices, what counts as science and stable data and thereby what was considered appropriate for sharing, or not. However, the difficulty of carving out a field site on the project of which I was officially a member made me consider the meaning and affect of 'collaboration' as a scientific relation. As George Marcus' (1995) concept of 'multi sited fieldwork' suggests, there is a need to reinvent the aesthetic and culture of method if anthropologists are to effectively respond to the changing contexts of contemporary fieldwork. This means focusing on how the field site materialises in research as a process of co-construction with one's informants, where negotiating and re-negotiating boundaries, identities and personal commitments is part of the fieldwork encounter. The notion of multi-sitedness and the level of awareness and sensitivity that the tracing of associations between different actors across different sites of practice requires, are sensibilities which reflect my own ethnographic experience, and are, I would argue, also familiar to researchers on WHAP taking part in multi-disciplinary research.

Scientists on WHAP discuss, materialise and implement a number of different ways of ensuring they effectively translate their knowledge (data, methods and technologies) on air pollution between different scientific disciplines. This process of translation and exchange between epistemic communities seemed to be what it means 'to collaborate' on WHAP, a process which implies an act of comparison and a transformation of information. However, this process of translation was the subject of much discussion and negotiation in WHAP, suggesting that collaboration was constructed in the movement of scientific work. ‘Incompleteness’ and ‘emergence’ (Marcus, 1998) characterised the process of producing knowledge on air pollution more generally. Thus, the partial nature of doing multi-sited work shapes both the field site encounter and research design and therefore my argument and findings throughout the chapters that follow. As Marcus suggests, the ‘chains, paths, threads, conjunctions, or juxtapositions of locations’ (Ibid: 90), in which the ethnographer establishes some form of literal, physical presence, with an explicit, posited logic of association or connection among sites, in fact defines the argument of the ethnography.

I used data as an ethnographic tool in my encounter with different air pollutions through different data, which are conceived in different scientist-instrument-data relations. The hyphen works here to collapse the distinction between the ‘perceiver’, ‘representation’ and ‘reality’, and to capture the *becoming with* of relations in the making of objects of research, as well as the objects themselves (See McLean and Evans, 2014). My interest in the multiple relations of scientific research is shaped by research in Science and Technology Studies (STS) and anthropology of science and its elucidation of the ways in which new forms of subjects and objects are formed in assemblages (Jensen and Rödje, 2010). I found that by attending to the movement, emergence and transformation of material work, the socio-material constellations which are made in practice, but also the forms of governance, narratives and infrastructures within which they are entwined, became part of my analytical remit. As Jensen (2010) argues, these assemblages include peoples ‘thoughts’ but equally the technologies and other materials with which they continuously engage.

Collaborative ethnography

As recent work has highlighted, new forms of ethnographic engagement have intensified the involvement of anthropologists with their counterparts (Faubion and Marcus, 2009) and ‘collaboration’ has been one of the figures invoked by anthropology to describe this situation (Estallela and Sanchez Criado, 2013). In light of Stathern's insight that 'a world obsessed with ones and the multiplications and divisions of ones creates problems for the conceptualisation of relationships' (2004: 53), I consider collaboration as a kind of field work relation that engages with difference and the partial nature of knowledge. By examining how the scientists on WHAP engage with similarity, difference and tensions between ways of knowing and making knowledge, I explore some of the ways in which these multiplicities and partialities are accounted for by researchers in practice. This offers an account of multi-disciplinary research in action. As an inherently multi-sited ethnography, I highlight tensions, nuances and transformations, rather than appealing to the implicit singularity and uniformity that notions of collaboration and multi-disciplinarity incite.

Annelise Riles (2001) exemplifies this trend of 'para-ethnography', and the shared ethnographic knowledge practices of late-modern society in her research on transnational issue networks and global finance infrastructures. In 'Networks Inside Out' she both documents the dissemination of information in international spaces, but also brings the concept 'networking', and its attendant knowledge practices, to bear on characteristics of anthropological analysis. In discussing the borrowing of her method from her ethnographic material, Riles argues that the method becomes far more contingent:

> Contrary to an ethnographic imagination of methods as universal and data as particular, I understand the "method" to be no more general or particular than the "data" to which it is applied. To state the same point another way, the contribution of this work is its challenge to the distance between data and method in the ethnographic information of information (Ibid: 191).

Building on Strathern's work in 'Partial Connections' (Strathern, 1991) Riles suggests doing the opposite of putting the ethnographer in the picture, but rather make the ethnographic state of mind a frame or form (Riles, 2001). Accordingly, 'ethnographic description must become demonstration', borrowing from informants own forms and designs and using them as tools - such as the forms that organise work practices and the modes by which communication takes place, and bringing what is already known into view for ethnographic analysis. In a similar way, ethnographic method can be characterised by an awareness of being a co-knowledge producer.

My role as ethnographer formed a part of the funding bid and the social science component of the project. I was therefore both a data producer and field site enabler. Demarcating a space to carry out my field work in a pre-defined site may, initially, seem paradoxical. However, the difficulty of carving out a field site on the project of which I was officially a member made me consider the meaning and affect of ‘collaboration’ as a scientific relation. Of course, being a part of a study of multi-disciplinary research is a reflexive process and throughout the empirical chapters which follow I make explicit how my field site engagement enabled each particular research encounter and analysis. Researchers also reflected on their own role as co-collaborators, considering the ways in which they communicated with other researchers on WHAP. There was a shared incentive to *do* multi-disciplinarity better, and researchers often reflected on how particular actions they had taken were received by others and with what effect.

I engaged in this process of sharing knowledge as an ethnographer on WHAP, presenting my findings, learning from and learning to respond to researchers’ feedback, suggestions and queries. Being complicit in the field site was a pedagogic practice too, through which I observed researchers adapting to other researchers’ suggestions by modifying the ways in which they presented and shared their work. For example, I noticed that researchers presented complex problems in visual ways and took on ‘other perspectives’ as a way to reflect on how their research interests could be co-ordinated with those held by other research groups. I also traced semantic shifts where the kind of language used to frame issues was adapted and altered in particular instances. Language and meaning were considered carefully by researchers, becoming a creative tool with the power and capacity to work around seemingly incommensurable epistemological discrepancies.

The effect of these reflexive actions by researchers led me to consider the visual and non-verbal ways in which complex theoretical problems were shared and communicated with those who did not share the same kinds of knowledge and technical skill. Indeed, it encouraged me to reflect on the predominantly textual ways in which I presented my own research and presented my arguments, which, I learned, did not necessarily lend themselves to a collaborative form of engagement. One of the occasions when I did attempt to collaborate as an ‘equal’ knowledge producer, was during a multi-disciplinary tension explicitly framed as resulting from different epistemologies. Since I produced data on the emergence of the tension, and its solution, chronologically, my fieldnotes could make visible research in process and formation for WHAP researchers. Researchers who read my ‘writing in progress’ on this topic were responsive, giving up time to talk to me about what kind of ‘debate’, ‘tension’ or ‘difference’ this really was. My data became a space for reflecting on research practices. Interestingly, they often lessened the problematic dimensions of these difficulties, viewing them as an inevitable and a productive effect of being a ‘trail blazing’ kind of research project. This suggests that reflecting on research practices shapes the ways in which tension and difference may get framed as a result.

In addition, attending the liaison teams meeting was an implicit way of sharing knowledge. Although the majority of discussions were led by other team members, my presence and, sometimes, minor contributions, worked to demarcate a space for anthropological knowledge. I was considered as contributing to the reflexive practices of team members, but also as helping to translate science to non-scientists and in their engagement with the outside world. As the PI told me during a brief chat about an issue I hadn’t quite followed in the liaison meeting, ‘if you don’t understand it then that is a problem because others won’t understand it’ (PI, Interview, June 2014). I stood then, sometimes, as a benchmark for the successful translation of science between researcher and to other non-scientific audiences.

The different roles I took on, and which were at times imposed on me, captures the creative and iterative nature of carrying out fieldwork on knowledge practices. As both knowledge producer and co-collaborator, the role of anthropologist and scientist were often inverted. Although often disconcerting, this crossing of roles enabled me to observe, experience and thereby flesh-out the affective dimensions of scientific research in action. Moreover, I was able to capture the in-between states of knowledge, but also reflect on how the particular roles collaborative science encourages foster an experimental mode of ‘doing together’.

Managing expectations

On joining the team, I took two MSc courses on statistical methods and environmental epidemiology, of which two members of WHAP were course convenors and tutors. This was useful in terms of getting to grips with some of the technical terms, and theoretical and methodological approaches employed in WHAP. However, it was also an experience that proved insightful ethnographically. It enabled me to stand outside the project and look in, in a sense. Indeed, on one occasion I was struggling to visualise and understand a 'GIS map'[11] of a hypothetical industrial leak during a practical seminar. The seminar leader and PI of WHAP came over to see how I was getting on. I explained that it was quite enjoyable and something very new to me. I asked if GIS mapping was going to be used in WHAP. It was. However, following this brief chat, Tim posed a new problem, a 'social problem', for the seminar practical, and one that also faced WHAP. He explained that although the industrial leak had negative health effects, those that live nearby may prefer for the site to remain because it could be a source of employment, emphasising that 'health' is not our only criteria when making decisions. Much to my alarm Tim then insisted that 'this is where you come in, to tell us what the public want', for 'it is all very well doing this kind of research but it is not always what people want?'

This took place early on in my fieldwork. I didn't know how to respond. Indeed, I hadn't even formed my central research questions, although I was certain I was not researching 'what the public thinks of air pollution'. As a result, I made a conscious effort to share some of my research ideas, and even findings, and team members' expectations became more in tune with my own, although the managing of these sorts of expectations remained a constant feature throughout my fieldwork. Furthermore, I found the managing of other research participants' expectations a feature of multi-disciplinary research more generally, rather than simply a product of the ethnographic encounter. Maintaining realistic expectations was a constant source

[11] In a general sense, the term describes any information system that integrates, stores, edits, analyses, shares, and displays geographic information for informing decision making. GIS applications are tools that allow users to analyse spatial information, edit data in maps, and present the results of all these operations.

of discussion during the weekly liaison meetings, with statements such as ‘what you are asking to do is not possible in our model’ or, ‘we are interested in the patterns not the specifics’. In a multi-sited and multi-disciplinary team, different research interests and expectations are constantly being reworked, thus, providing a complex mesh of disciplines does not simply make it possible to answer complex questions, as the rhetoric of multi-disciplinary often suggests in scientific research funding calls.

During one liaison meeting I was asked to contribute ideas for organising a showcase on WHAP for a related social science symposium, an activity which would fulfil the ‘public outreach’ component obliged by the funders. Here, again, my role was considered as a translator between the scientists as experts and the public as laypersons. The shift here suggests my role is considered not as a translator from ‘the public’ to ‘science’ but the other way round. Similarly, at one of the handful of stakeholder meetings the project held, my role was defined by the team as having a more critical charge. In the first stakeholder meeting I attended in late 2011, I was referred to, although in jest, as a spy who was observing ‘what we are all doing’. Indeed, in a liaison meeting (Fieldnotes December 2011), following what was considered a slightly controversial comment, there was a joke that we would find this sprawled on the front page of The Sun. Here my role was considered as an insider whose loyalties lay outside of the team.

Shifting affiliations of the ethnographer, as well as their alienations, with those with whom research interacts at different sites, constitutes a distinctively different sense of ‘doing research’. I was at once a PhD student, a peer, a translator, a collaborator, a social scientist and a spy and these identities were contingent, constituted in particular research moments. At times then, that I served as a reminder of the risks of sharing too much highlights the levels of trust and sentiment of membership that a collaborative project builds and relies on. However, I was also considered a member of the team. For example, when I visited scientists in their ‘home’ institution participants would introduce me as a PhD student working on their ‘WHAP project’, and great effort would be made to make me feel welcome with social events planned and working amenities provided. Indeed, because I was also a researcher and PhD student meant I shared the wider academic culture, but also held a junior position within its hierarchy. I was often given PhD 'tips' and even empathy, because it was all something researchers had been through. There were times, then, that I was one of the others, seen as more than an 'other' but as a mediator because I shifted between sites of research, unlike other researchers on WHAP. This movement enabled me to experience the geographic and disciplinary distance between sites of practices. There were times when informants would share some of their frustrations with doing multi-sited, multi-disciplinary research. A frequent example of this was the problems of communication, asking me to suggest to 'The City lot' improved ways of formatting the weekly liaison meeting. I was even requested to attend the weekly web-conferencing meeting as an 'online participant,' to experience it first-hand.

Summary of thesis

In this chapter I have presented a few snippets of the different roles I took on as an ethnographer and co-collaborator on WHAP, illustrating the interactive shifts my research took. These shifts were shaped by the different spaces of scientific work which emerged as the project progressed, and through the role data played in the carrying out of research across different sites of practice. In tracing the movement of data, I examine the practices and processes which came to use, re-use and reconfigure data between research groups. Data were both methodologically and theoretically productive, providing me 'a way in' to access the dynamic and generative movement of 'real scientific work', and the contingency of the epistemological cuts made in the process.

The discussions in WHAP were data centric, and this ethnography traces the way in which data were produced, used, re-used, contested and negotiated between sites of practice. In the chapters that follow I am going to describe the different technologies used to measure, model, materialise and visualise air pollution as a research object, and the way in which these knowledge generating tools carry their own effects and capacity to generate claims about air pollution. Over the course of my fieldwork it became clear that data were considered crucial both within WHAP and the scientific community at large. External discussions with policy makers revolved around what data is available, what data represent, what questions need answering, and thereby what kinds of claims are to be made about air pollution. What I also examine, therefore, are the ways in which these data practices compose and comprise the social and cultural relations of air pollution politics.

I have split this thesis into three parts. This first part, including this chapter, is about constructing a conceptual and practical frame for researching air pollution. In Part I, I introduce the problem of air pollution in contemporary science and policy, and describe my own role as ethnographer within a multi-disciplinary research setting. In the subsequent chapter, Chapter Three, I will discuss some of the key developments in social and cultural studies of science, to provide a theoretical background for the empirical chapters which follow.

In Part II, I focus on the multiple ways in which measurement data of air pollution are made and how these different data relate to one another in practice. In Chapter Four, the empirical focus is *multiplicity*. Multiplicity, as I have described so far, is a defining feature of WHAP, yet also a practical problem. I am going to examine the different ways in which data of air pollution are made and the different kinds of data practices these comprise. From tracing the different kinds of research objects these data practices produce, I move on to the ways in which these different research objects become problematic for the epidemiologists, who intend to re-use measurement data in their own data practices. Chapter Five develops this tension around multiple data and multiple research objects, and I will examine two of the ways in which these different kinds of data and data practices are negotiated and *co-ordinated* by researchers on WHAP.

In Part III, I am interested in the ways in which data travel between different research spaces and data practices, and to non-scientific spaces. In Chapter Six I will focus on the statistical data practices of the epidemiologists, primarily on the production, interpretation and re-production of a series of graphs of air pollution data and health data. I make these images active in my discussion to come to understand the ways in which they link different kinds of scientific phenomena together. From *linkage* I move onto *selection* by examining ways in which data relations extend to 'non-scientific' spaces and in doing so the particular kinds of data relations which become salient. In Chapter Seven, then, I will show the ways in which external relations are brought in to scientific discussions, which shift the kinds of considerations which have orientated data practices so far, and thereby the questions asked about air pollution with this data. I point to the tension between science and policy, but also the relations between these different spheres of knowledge and practice.

In the discussion chapter I summarise the different analytical concepts I have used in each empirical chapter. Starting out with the finding that air pollution and how to come to know air pollution, through data practices, is *multiple,* I will look at how these different kinds of data are *co-ordinated* in practice. I then discuss the ways in which data are made meaningful, which endow it with the capacity to do things and through which data relations are extended. The first way in which this is done is through *linking* air pollution data with health data. The second method of extension involves sharing data with stakeholders, a process of engagement which moves the responsibility away from the scientists to the considered end-users of scientific knowledge. The invited stakeholders have to *select* which data are to be used and which air pollution relations are salient. In this way, the object of research, air pollution, is configured within particular local scientific practices but also through the extension of the network of science-policy relations.

Each chapter will contribute to a particular juncture in the process of carrying out multi-disciplinary research in practice, from the collecting, producing, processing and re-using of multiple kinds of data. In doing so I attend to data as an informational and material form, and to the relational capacities data grants. I therefore also consider data as a sociable form, one which can be used as a way to ethnographically explore scientific relations and the playing out of scientific practices across different geographical, epistemological and cultural spaces. In this way, I am going to not only examine the ways in which air pollution is made visible as an object of scientific research and as a policy issue, but make visible the often imperceptible material and non-material affective engagements which make up *doing* ‘science’, ‘collaboration’ and ‘multi-disciplinarity’ in practice. I begin, however, by introducing some of the ways in which scholarship within the diverse field of social and cultural studies of science have examined science in practice, and in the chapter which follows, trace a series of terms, concepts and material workings which shape and structure my empirical arguments.

Chapter 3: Science as a social and cultural practice

Introduction: classifications and distinctions

> Formal classification systems are, in part, an attempt to regularise the movement of information from one context to another; to provide a means of access to information in time and space. The ICD, for example, moves information across the globe, over decades, and across multiple conflicting medical belief and practice systems (Bowker and Star, 1999: 290).

Science is a system of knowing. For those studying science as a social and cultural phenomenon, what scientists do and the material work this involves is a way to get 'inside' science. This has often meant attending to the historical, spatial and temporal dimensions of knowledge making, and the technical and practical circumstances within which it is enmeshed. In the opening quote, I reference Bowker and Star's renowned study of classification practices, which describes what classifications do and enable. Using the example of the World Health Organisation's International Classification of Diseases (ICD), the authors show that this classificatory mechanism facilitates the production of global and universal knowledge, despite internal difference and complexity. These mechanisms are often invisible, they argue, particularly in contexts where classifications are taken for granted categories to work with. Therefore, by focusing on the practices which construct classifications, the underpinning ideals and concepts which shape, structure and arrange knowledge become tangible, as components of everyday practice.

Practices are a rich site for sociological and anthropological analysis, articulated through material interactions and entangled with normative social and cultural values. Bowker and Star's work also suggests that classifications not only organise and structure knowledge, but they constitute the very things they seek to classify. In other words, classifications are also technologies which construct knowledge, making classifications an epistemological and political process with social and material effects (See also Hacking, 2007). Indeed, through the example of the ICD, the authors argue that classifications can also mobilise knowledge, enabling it to travel between different kinds of knowledge practices, and therefore to be potentially used to achieve other kinds of results.

In scientific practice, nomenclature of phenomena are parts of the system of knowing itself. As I highlighted in the introductory chapter, air pollution is categorised according to different chemical species, and these are further classified according to their characteristics and behaviour with other pollutants, constituting the atmospheric relations of which they are a part. The ways in which phenomena are organised and arranged as entities to be known, shapes the material arrangements of scientific practices, and therefore the nature of the data collected, made and used to make knowledge claims. Knorr-Cetina refers to these arrangements as ‘machineries of knowing’ (1999: 117), to highlight the mechanic process by which different material and conceptual components of practices articulate with one another:

> Practice, of course, can also refer in a more generic way to just those patterned, dynamic sequences which are the ingredients of such machineries. This notion of practice shifts the focus away from mental objects such as the interests or intentions that inform concepts of action, and toward the reordered conditions and dynamic of the chains of action of collective life (Ibid: 9-10).

In this way, classification practices, and indeed other ways of organising and arranging objects of knowledge, configure the ways in which scientific phenomena materialise and come into being.

The materialisation of classifications, specifically the unpacking of binary divisions, has been a starting point for many scholars in science studies, who have sought to demonstrate the intricate and messy entanglements of science and technology with social, cultural and political processes. One of the central tropes has been that of dissolving the bifurcation of nature and culture, a distinction, Latour explains, that results from of our ‘modernist predicament’[12] (1993). Andrew Webster extends this notion of what a classification is by suggesting that it is also ‘a form of governance

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[12] Latour (1998) describes the modernist predicament as seeking purity over hybridity and fact over fabrication, yet, he writes: ‘the whole theory of society is enmeshed into a much more complex struggle to define a psychology -an isolated subjectivity still able to comprehend the word out there; an epistemological question about what the world is like outside without human intervention; a political theory of how to keep the crowds in order without them intervening with passions and ruining social order; and finally a rather repressed but very present theology which is the only way to guarantee the differences and the connections between those domains of reality. It is this whole package that is in question’.

inasmuch as it provides one of the bases on which the regulation of life can occur, establishing boundaries of responsibility, inclusion and exclusion, and accountability' (2007: 4). In this way, classifications are both a practice and an effect, in the sense that they instantiate epistemological boundaries, help construct the institutional structures of science but also configure the socio-material relations which enact these.

Since classifications can be both at once universal and particular to local systems of knowledge and values, they are particularly powerful modes of organising and distributing knowledge across different social and material worlds. In the following chapter, I am going to examine some of the ways in which knowledge systems generate expertise, make claims and shape actions, and the ways in which these have been studied as practices and processes by those in science studies. Indeed, sociology, anthropology and STS have developed a number of concepts which shift discussions about science from its considered 'objective gaze' to the more situated knowledges (Haraway, 1988) and the social shaping of technologies (Mackenzie and Wajcman, 1985), which one finds in practice. Studying everyday material work and interactions is a way to reveal the messy realities of practice. Part of this 'mess' (Law, 2004) is the discovery that human and non-human actors are entangled in complex ways. Haraway's concept of 'the cyborg'[13] (1991) and Latour's notion of 'hybrids' seek to capture the amalgamation of human and non-human forces in techno-scientific knowledge practices and processes. For example, the introduction of different kinds of technologies which interact and intervene with the conceived 'natural order' enact new kinds of realities: as modified organisms in genetics and bioengineering (Rabinow, 2005) or to the status of the human, in terms of the role of reproductive technologies on the re-configuration of kinship relations (Franklin, 2001). The conceptual ways of describing hybridity, and relations between human

[13] Haraway (1991) writes: 'A cyborg is a cybernetic organism, a hybrid of machine and organism, a creature of social reality as well as a creature of fiction[…] Social reality is lived social relations, our most important political construction, a world-changing fiction contemporary science fiction is full of cyborgs - creatures simultaneously animal and machine, who populate worlds ambiguously natural and crafted. Modern medicine is also full of cyborgs, of couplings between organism and machine, each conceived as coded devices, in an intimacy and with a power that was not generated in the history of sexuality',

and non-human forms, foregrounds the agential role of tools, instruments and material arrangements of knowledge making.

Studying science as a practice which instantiates a set of divisions and distinctions, is an epistemological and ideological endeavour, revealing of the social, historical and cultural complexity of scientific fact making (Edwards et al., 2007: 3). Starting with the practice turn in the social and cultural studies of science, I introduce the role of data practices as particular kinds of socio-material practices and processes. I will then go on to introduce some key concepts from these studies, which orientate my own theoretical and approach in the empirical chapters which follow. Beginning with the ways in which measurement and observational practices have been theorised, I move on to data as particular kinds of material objects and the related practices which produce, use, process, interpret and re-use these. I end by examining the capacities data practices open up for the social and cultural theorising of science.

The 'practice-turn': from relational networks to material enactments

Over the past few decades, social and cultural studies of science have focused on the multiple practical and material dimensions of everyday scientific life (Knorr-Cetina, 1999; Latour and Woolgar, 1986; Pickering, 2008; Traweek, 1988). The focus on practice is entwined with a notion of 'generalised symmetry', an approach which, Bloor (1976) argued, requires the analyst to study knowledge practices which relate to both 'true' and 'false' claims in the same way. Previous studies, it was claimed, had focused only on the successful practices of science, thereby not challenging the epistemological commitments which a given claim upholds (e.g. Merton, 1973). For example, Harry Collins and Trevor Pinch have pointed to the role of non-scientific practices in achieving consensus scientific controversies (1982), which laid the groundwork for Collins' later work on 'the experimenter's regress' (1985). Related empirical inquiries have since challenged ways of theorising science more broadly, focusing on what scientists do rather than say, thereby prompting not only epistemological, but also ontological debate.

Knorr-Cetina's concept of 'machineries of knowing' encourages a material and relational approach to scientific objects of knowledge. This primarily means studying the spatial and temporal conditions and arrangements of scientific work, such as ‘the laboratory’ (e.g Latour and Woolgar, 1986; Knorr-Cetina, 1999) and ‘the experiment’ (Shapin, 1985), and also in spaces and places ‘beyond the laboratory’ (Bijker and Law, 1992). These studies illustrate the assembling of things, technologies and people, which articulate phenomena in particular ways, as a set of human, non-human, material and non-material relations. Moreover, they show how situated practices and the objects they produce, travel and extend scientific networks and therefore ways of knowing and materialising scientific phenomena ‘outside’ of science.

The material-semiotic approach of Actor Network Theory (ANT) has played a leading role in the formation and development of science studies since the 1990s. Although famously claimed as neither theory nor method (Law and Hassard, 1999), John Law later reflected that ANT was an empirical version of Foucault’s post-structuralism:

> [ANT] can also be understood as an empirical version of poststructuralism. For instance, “actor networks” can be seen as scaled-down versions of Michel Foucault’s discourses or epistemes […]The actor network approach asks us to explore the strategic, relational, and productive character of particular, smaller-scale, heterogeneous actor networks (2008: 145).

Working across different ways of knowing and particular kinds of classification systems involves understanding the way we organise knowledge and the material world as particular articulations of power. This flattened topology of social relations leads to questions around how we instantiate dualisms such as epistemology and ontology, the individual and society and, consequently, the micro and the macro. The primary claim of ANT is that knowledge is the outcome of successful relational networks, which co-construct the context and content of science. A key concept in ANT has been that of ‘translation’, understood as a movement from one set of heterogeneous relations to a new set (Law, 2008). Because networks are not necessarily stable entities, translation is the moment when an actor responds to an action carried out by a different actor(s), and through which a network is mobilised. In this way, each actor (human and non-human) mutually shape one another in an on-going process of distributed agency. This means that both what counts as the social, cultural, scientific, technical and indeed human and non-human, is decided upon in stabilisation of a network of relations. This symmetrical approach is ultimately about power because, as Michel Callon explains, it brings to the fore the different kind of actors involved in the process of knowledge production, and therefore 'how a few obtain the right to express and to represent the many silent actors of the social and natural worlds they have mobilised' (1986: 225).

A second key concept for understanding scientific practice is that of 'inscription', a term coined by Latour to describe the material marks which are produced through human-instrument interactions, and which transform matter into written documents. Developed through his ethnographic field work at the Salk Institute, Latour observes the process of inscription making as one which ultimately leads scientists from particularity to universality:

> Going from the paper to the laboratory is going from an array of rhetorical resources to a set of new resources devised in such a ways to provide the literature with its most powerful tool: the visual display […] This move through the looking glass of the paper allows me to define an instrument, a definition which will give us bearing when entering any laboratory. I will call an instrument (or inscription device) any set-up, no matter what its size, nature and cost, that provides a visual display of any sort in a scientific text (1987: 67-68).

The emergent laboratory studies of the 1980s and 1990s (Traweek, 1988; Knorr-Cetina, 1999) describe the intricate workings which bring together of ideas, skills, instruments and material arrangements for the making of inscriptions. In this way, scientific practices become local processes, and the object of research, rather than discovered emerge in relation to the particular socio-material practices which materialise them.

As such, the instrumental interventions of practices, as both ideological and material, requires taking seriously the relations between the different components of a given practice, between related practices and the visual tools which intervene in and connect these. These working objects of research, as the things which make up the human and non-human relations configured in practices, are one of the ways in which both the doing of science and the cultural and social values shaping science as a knowledge making process have been traced.

Hans-Jörg Rheinberger, a philosopher of science, defines his approach to the study of scientific phenomena, as one undertaking a 'shift in perspective from the actors' minds and interests to their "objects of manipulation"' (1994: 7). Indeed, Rheinberger proposes the notion of the 'epistemic thing' as the material entities or processes - physical structures, chemical reactions, biological functions - which constitute objects of inquiry. Working with objects of inquiry is an uncertain process, inhabited by vagueness because, paradoxically, epistemic things embody what one does not yet know:

> [T]he activity of scientific representation is to be conceived as a process without 'referent' and without 'origins'. As paradoxical as it may sound, this is precisely the condition of the often touted objectivity of science, and of it peculiar historicity as well. If we accept this statement, any possibility of a deterministic account of science, be it socially or technically motivated, is excluded (Rheinberger, 1995: 51-52).

In highlighting the processual nature of studying epistemic things, Rheinberger points out that any representation should be conceived of as a 'chain of representations', which involves crossing boundaries and classifications, between scientific techniques, experimental systems, established academic disciplines and institutionalised projects. Objects of scientific investigations are, then, often in a process of being materially defined in non-representational and open ways.

Karin Knorrr-Cetina's work (1999) develops the concept of 'epistemic cultures' as a way to capture both the making of epistemic things, but also 'subjects' and their 'cultures of knowing'. She argues that scientists are also shaped by the objects they study, and just as objects are transformed into images, extractions and a multitude of other things, so are scientists reconfigured to become epistemic subjects (Ibid: 32). This process is nicely demonstrated in her description of 'the experiment' as relating to particular scientific ontologies:

> […] the experiment becomes constituted as a distinctive and powerful structure in its own right […] it is the work of rearranging the social order, of breaking components out of other ontologies and of configuring, with them, a new structural form. The repackaging of efforts accomplished during the birth of a new experiment is also the repackaging of social composting and the creation of a new form of life (Ibid: 214).

Knorr-Cetina examines the ways in which scientific reality itself is also constructed by selective and contextual scientific laboratory practices. She complicates the relationship between a representation of science, the output of a particular practice and the thing (the reality), of which it is supposed to refer (Knorr-Cetina, 2001). This is in not a relativist position where what counts as a reality is always particular, but rather that what is 'out there' is the consequence of scientific work rather than its cause.

Such practical redefinitions of scientific reality and the relations which constitute it demonstrates ways in which scientists not only represent the world, but engage *with* the world. In this way, studying practice therefore becomes a dynamic process of multiple agencies emerging and interacting (or, as Karen Barad (2003) describes, as a process of 'intra-acting'). Indeed, Pickering suggests starting off with the idea that the world is doing things, one in which science is 'a field of power capacities, and performances, situated in machinic captures of material agency' (2008: 7). Pickering's vision of the multiple agencies which are mobilised in scientific practices, as one in which the human and the non-human is always in emergence and becoming, offers a kind of 'de-centred ontology'. With reference to the field of cybernetics, he argues that particular practices can have world-changing effects:

> Cybernetics' distinctive ontology fed into distinctive approaches in areas of human endeavour as various as brain science and artificial intelligence, robotics, information theory and theoretical biology, on the one side, and psychiatry, management, politics, the arts and spirituality on the other. Cybernetics thus showed that bringing to consciousness a decentred and temporalised ontology can make a big difference in the world, restructuring and reconfiguring great swathes of culture and practice (Ibid: 12).

Building on this notion of 'particular ontologies' and the intertwinement of human and non-human actors has shifted notions of epistemology, where acts of observing or representing are at once moments of intervention and construction. As Jensen describes, this means that science becomes a practical activity:

> In this view epistemology collapses into ontology and the science are reformulated as practical activities aimed at (re) building the world by adding new elements with new capabilities and new relationships to it. Knowing (and thinking about knowing) are turned into particular styles and methods for connecting and

> cooperating with specific actors (humans and otherwise) – thus shaping reality or doing practical ontology (2004a: 248).

The emergent and dynamic modes of doing science, which materialise through studies of detailed everyday practices, are significant to those studying science because they make visible the patterns and disjuncture's which practices open up. This is the focal point of Annemarie Mol's research, where she attends to the multiplicities which ensue when one takes seriously the empirical study of practices (2002; Mol and Law, 2004). In her research on atherosclerosis, she argues that the disease is not simply interpreted by doctors and patients, but enacted as different versions of the disease. This distinction between interpretation and enactments shifts the nature of her research and analysis, because the empirical problem is not that there are different understandings of the disease but different diseases altogether. For Mol, then, by detailing and following practices, not only can knowledge in the making be traced but so can a world in formation:

> If we no longer presume "disease" to be a universal object hidden under the body's skin, but make the praxiographic shift to studying bodies and diseases while they are being enacted in daily hospital practices, multiplication follows. In practice a disease, atherosclerosis, is no longer one […] atherosclerosis multiplies – for practices are many. But the ontology that comes with equating what is with what is done is not of a pluralist kind. The manyfoldedness of objects enacted does not imply their fragmentation. Although atherosclerosis in the hospital comes in different versions, these somehow hang together (2002: 83-84).

This emphasis on practice as material and relational has meant brining to the fore 'the practical and material terms of engagements'. As Mol highlights, practices do not take place in closed systems of knowing but most often 'hang together' and intervene with other kinds of practices.

Tiago Moreira's (2006) examination of the co-ordination of different ways of measuring blood pressure traces the ways in which difference is managed in surgical practice. In doing so, he highlights the standardisation of the conceived epistemological hierarchies around different knowledge practices. He shows that in practice the traditional 'finger method' and the superior 'sphygmomanometer method' are both peripheral and interdependent to one another, so that each are configured by the other. In this way, difference is not necessarily a state of tension, but a starting point for the study of how different knowledge practices are at once possibilities and strategies which are partially connected (Law and Mol, 2002: 17).

'Seeing and knowing': making scientific objects visible

Much of scientific work involves making phenomena visible as objects of research and action (Latour and Woolgar, 1986). These practices are contingent on particular ways of 'seeing' and 'doing', relating to particular 'rendering practices' (Lynch, 1985b) and 'professional vision' (Goodwin, 1994). As I have described, the more recent shift in science studies from a focus on representation to mediation and enactment (Coopmans et al., 2014), orientates us towards the different kinds of devices used to make scientific claims, such as images, graphs and models. The socio-technical arrangements of scientific work require what Daston and Galison (2007) call, 'practices of seeing', but also the theoretical discourse which shape 'epistemologies of vision'. For example, Michelle Murphy (2006) uses the encompassing frame of 'regimes of perception' to take into account the playing out of micro and macro processes of power through 'vision', where the politics of knowledge production and the process of materialisation involve obscuring awareness of certain things in order to make others more pronounced, known and controlled.

Fixing scientific objects is an unstable and indeterminate process. Social studies of science have shown that authority does not reign 'outside', in nature, but is constituted in the network of relations of which it forms a part. The craft of science becomes the holding together of these heterogeneous elements in ways that enable phenomena to express themselves. This means that following the material objects of science and the ways in which they are visualised through particular techniques and 'tinkerings' (Knorr-Cetina, 1981) is also a way to see the situated contingencies through which phenomena are stabilised. In Daston and Galison's study of objectivity, they argue that objectivity has a history which can be traced through scientific atlases, as the 'working objects' which embody the emergence and development of epistemic virtues which guide and sustain science over time:

> [T]here is no atlas in any field that does not pique itself on its accuracy, on its fidelity to fact. But in order to decide whether an atlas picture is an accurate rendering nature, the atlas maker but must first decide what nature is…Atlas makers committed to mechanical objectivity resisted interpretation; their predecessors, committed solely to truth to nature, relished it (Daston and Galison, 2007: 86-88).

By studying objects, and the arrangements which make and sustain these, 'objectivity' can be seen as historically mutable, evolving and contingent, and therefore more than an epistemological concern. With reference to what they call the emergence of 'mechanical objectivity' in the nineteenth century, the authors show the historical contingency of scientific ideals and how these shape and configure what is seen and known.

Developments in the ways in which science is practiced, both in terms of technical innovations and in the institutional and structural arrangements of scientific research, has required new conceptual tools for thinking about and studying scientific practices and therefore practices of representing, intervening and visualising phenomena (e.g. Coopmans et al., 2014). The multiplicity and heterogeneity of material domains of scientific practice means paying attention to the particular and concrete arrangements of bodies' textual surfaces, lines of sight, and fields of technical action as, simultaneously digital, representational, technical, embodied, mathematical and epistemological (Ibid). One of the central technological developments has been, of course, computer technologies, as devices which configure the collecting of measurements, the interpretation of these and also the production of new kinds of data. These developments also shape the kind of orderings which emerge in scientific practice, and as a result influence contingent ways of seeing and knowing, because not only do these afford new contexts of study, but they also require unsettling notions such as 'representation', 'visualisation' and 'perception'.

Simon Cohn has argued that knowing and seeing are part of the same activity, and that these are achieved through often cultural and rather mundane methods (2007: 92), in which the skill and experience is gained through purposeful, pedagogic practices, in the form of an 'apprenticeship' (Grasseni, 2007). Based on fieldwork with medical practitioners using digital surgical intervention, Cohn examines the ways in which new technologies, as particular techniques for seeing, allow the traditional values underpinning medical expertise to co-exist, a technological development which could be misconceived as an individual activity: ‘the technology continues to allow for mutual interpretations, decision making and evaluation emerging from the social rather than strictly technical nature of their work’ (Cohn, 2007: 101). The ethnographic comparison of surgical intervention by hand shows that the process of revelation can be circular rather than linear, and actively pursued rather than simply tacitly conceived:

> In both procedures of the scalpel or computer keyboard, there is continual oscillation between decisions and procedures thought to ‘reveal’, ‘unmask’ or clarify structure and regions and others that actively seek and pursue them, catching sight of them in the chaos and complexity of the data (Cohn, 2007: 104).

Digital techniques of visualisation are also embodied relations, with their own forms of practical efficacy (Lynch, 1985a). Burri and Dumit’s account of scientific studies of images and visualisation, for example, show that visualisations are also comprised of scientific practices which are always in process and indeterminate:

> These visualisations - alternatively called models, hypotheses, maps, and simulations - are provisional and interactive. The researchers constantly tweak them, altering parameters, changing colour scales, substituting different algorithms or statistical analyses. These visualizations are part of making the data meaningful. They are interstitial, facilitated modes of seeing and intervening (Burri and Dumit, 2008: 303).

Similarly, in a discussion on the visual documentation of objects in the life sciences, Lynch argues that representational practices are more than a matter of reducing information to manageable dimensions, because ‘visual representation involves adding visual features which clarify, complete, extend and identify conformations latent in the incomplete state of the original specimen’ (Ibid: 229). Tracing the use of visual conventions by scientists, Lynch argues that the additive role of visual models enable new theoretical relations to be represented as though they are in the depicted objects:

> Instead of reducing what is visibly available in the original, a sequence of reproductions progressively modifies the object’s visibility in the direction of a generic pedagogy and abstract theorizing… the object becomes more vivid; we can picture it as though it were “naturally” present for our inspection (Ibid 229).

It is these ‘modifications’ that make visual forms such a rich starting point for exploring the contexts and ways in which scientific objects emerge, stabilise and come in to being. Digital technologies do also offer purchase on what ‘intervening’[14] (Hacking, 1983) really means in contemporary scientific practice. As Sismondo states, models are complex social objects which afford complex representational claims (2000: 239) and, as Hacking claims, models are representations of both theory and phenomena (1983). In a similar vein, Morgan and Morrison’s philosophical study of computer models demonstrates the ways in which computer models are instrumental in making a measurement, but also work as investigative devices for learning something about the things they seek to represent:

> We can think of a thermometer representing in a way that includes not simply the measurement of temperature but the representation of the rise and fall in temperature through the rise and fall of the mercury in the column. Although the thermometer is not a model, the model as an instrument can also incorporate a representational capacity (1999: 25).

This shift in the conceptual capacities of knowing and sensing phenomena through new, digital technologies can be demonstrated in the development of climate science modelling, where the object of science are complex ‘environmental systems’. In Paul Edward’s (2010) acclaimed historical account of climate science he argues that studying the natural environment as a system requires modelling because without models there are no data, for everything we know about the world’s climate is known through models. Computer models both extend and constrain what and how knowledge is made. Indeed, the very notion of ‘simulating the atmosphere’ reconfigures knowledge on phenomena because of the informational capacity this grants.

Computer models, then, join with measuring instruments, experiments, theories and data as one of the essential ingredients in the practice of science (Morgan and Morrison, 1999: 36). New technologies assist new ways of thinking and intervening

[14] Philosopher of science Ian Hacking argues that acting and thinking are not separate actions, where theorising, calculating, modelling, approximating are all real articulated representations of how the world is. What this means in terms of the notion that ‘representations are interventions’ is a focus on how representations are used and interpreted in ways to alter the world in some way, what he calls ‘entity realism’.

with phenomena. Indeed, Stefan Helmreich claims that computer simulation practices may shape the very meaning of ‘life itself’ (1998). Based on his ethnographic field work with computer scientists, biologists and engineers at the Massachusetts Institute of Technology, Helmreich explores the emergent field of ‘artificial life’ dedicated to the computer simulation of biological systems. He calls the substance and space of the researchers he studies, ‘silicon second nature’. This is for a number of reasons: firstly, they are ‘second natures’ in the sense that they are rule-ordered human constructions which are meant to mirror nature; secondly, because they are also set to succeed nature as a resource for scientific knowledge; and finally, since they are likely to become increasingly common among humans inhabiting a world in which computers are haunted by ‘life’ (Ibid: 12). What Helmreich illuminates is the ways in which computer simulations re-configure both how scientists come to perceive the world and the re-making of this world through these embodied and distributed practices.

Seeing changes in an instrument, or observing phenomena in their ‘natural setting’, are often delineated as ‘experimenting’ and ‘observing’, yet computer models cross this neat division. As Mikaela Sundeberg (2006) claims, the relationship between theories, models and observation are important, but so is the relationship between the working practices that underlie theories, models and observations. If we consider again Helmreich’s tension between ‘reality’ and ‘simulated reality’, then Sunderberg’s research on the relationship between modellers and field workers offers purchase on this tension. She shows that what counts as ‘model output’ and ‘field data’ is worked out relationally, meaning that neither types of data are stable entities but delineated through a process of mutual interpretation.

> The different way experimentalists and modellers work with and understand simulation models and data shapes what it means to be a “good” simulation model or “good” data. The contents of these qualities depend on whether you measure or simulate. From a social world viewpoint, it is the practice-based understandings rather than any abstract criteria that are of importance (Sunderberg, 2006: 64).

In this way, engaging with different models of phenomena, both technologically and conceptually, is an opportunity not only to re-articulate realities, but to also enable interaction between different ‘models’ of these.

The practical dimensions of making digital data shows that digital spaces of practices are no less material and embodied than laboratory work. Moreover, considering 'seeing' as 'intervening' offers purchase on the different kinds of scientific identities and realities which emerge as technologies develop. Natasha Myers' research on modelling practices illustrates the ways in three dimensional modelling practices make explicit scientists' creative and embodied contributions to 'visualising life' (2007: 33). By highlighting the animate, dynamic practices of protein modelling Myers traces the ways in which these refigure ways of seeing and enacting proteins, and thereby the reciprocal relations between the culture of science and the ways in which objects of knowledge are made possible and brought into being. For example, the protein structures which populate life since laboratories and databases are now digital, which make objects of molecular biology tangible and workable in new ways. Describing the ways in which protein modellers develop an 'embodied imagination' of molecular forms dissolves, she argues, any distinction between material and conceptual models:

> Once embodied, these models come alive in the performative gestures of researchers used to communicate protein forms and mechanisms in conversation within and outside of the laboratory, and in conference presentations and classroom lectures (Ibid: 166).

Drawing on Donna Haraway, Myers' work, along with scholars such as Rachel Prentice (2005; 2014), draw into focus the embodied, tacit and sensory craft of scientific work, which generates new conceptual demands and insights for the study of technologies of representing and manipulating scientific objects.

Latour argues that we need to understand how a representation is produced or manufactured through many stages of inscription (1987). I have traced some of the multiple and heterogeneous ways in which inscriptions have been studied, from photographing and experimenting to modelling and the running of computer simulations. Such studies show that digital and statistical data practices produce and work with phenomena in news ways, where new ways of seeing, feeling and therefore establishing authoritative scientific claims arise. As a result, these emerging complexities pose a changing set of inquiries for the social and cultural study of science.

The social and material relations of data practices

Technological developments enabled through computer models not only generate new kinds of knowledge but also shape the very organisation and meaning of scientific terms, practices and standards. The seemingly routine, administrative work of, for example, data management, curation and care, have also generated curiosity, for their playing a part in establishing stable representations (Daston, 2008; Hine, 2006). Paul Edwards' account of making global data through global climate models, details the multiple processes that go into making data, from infrastructural developments to everyday practices and processes. Data is not, he argues, simply collected, 'but checked, filtered, interpreted and integrated within computer models' (Edwards, 2010). The craft-work of data practices, then, involve automatic and manual processes which transform data into manageable entities, but also work with data and the devices used to make the data in order to gain confidence in the methods used. Some of the transformations data undergo have been explored as a way to highlight the kinds of interpretive work data practices involve, such as 'quality control' and the 'checking for error' (Sunderberg, 2006). Indeed, these practices and processes have, in the case of the atmospheric sciences, transformed the word 'data' altogether, because data are not the end-point of a practice but make-up and connect in an on-going process of further data generation[15] (Edwards, 2010: 253).

In this thesis I define data practices as the social, practical, material and conceptual dimensions of working with data; spanning the collection, production, processing, interpretation and re-use of data. In this way, I consider data like other scientific objects, as material entities and the result of a network of heterogeneous relations, which are in process and emergent rather than fixed and abstract. Gitelman writes that data are 'evolving assemblages rather than discrete entities', which need to be understood as 'framed and framing' (2013). By this Gitelman highlights that data cannot exist on their own. Despite being seemingly abstract 'data ironically require

[15] As Edwards eloquently states, the associated 'techniques' for working with these large amounts of data make global data possible, but also converged with making data global (Ibid.: 187). It is this two way process that demonstrates the capacities of data practices, as a set of heterogeneous relations, to shift, shape and re-configure not only particular practices but the relations between these, as inter-related systems of knowing.

material expression', so that just as we use data, data also 'needs us' (Ibid: 5-6). Part of what distinguishes data from the more general category of information, argues Gitelman, is their subtle ability to shift between epistemological and ontological domains, which means that the 'imagination of data is in some measurable ways always an act of classification' (Ibid: 8-9).

> When phenomena are variously reduced to data, they are divided and classified, processes that work to obscure – or *as if* to obscure – ambiguity, conflict and contradiction […] Data by definition are "that which is given prior to argument", given in order to provide a rhetorical base […] Yet precisely because data stand as given, they can be taken to construct a model sufficient unto itself: given certain data, certain conclusions may be proven or argued to follow. Given other data, one would come to different arguments and conclusions (Ibid: 7).

Data can be both representational and informational, but also material and conceptual. As such, data therefore slip between objects and things, as both temporarily stable entities and fluctuating things[16] in process and emergence Furthermore, as Hilgartner and Brauf's (1994) study of data access shows, ethnographically analysing the process of scientific production and the creation, packaging, and exchange of data has implications for how we understand science and policy. Using the concept of 'data streams', Hilgartner shows that studying data as material practice is also a way to socially and culturally frame scientific practice, not as isolated objects but as entities that are embedded in evolving courses of scientific production:

[16] The difference between objects and things is a distinction which has recently been re-animated in anthropology and science studies. Drawing on Martin Heidegger, Latour suggests taking scientific objects and considering them as things: 'A thing is, in one sense, an object out there and, in another sense, an issue very much in there, at any rate, a gathering. To use the term I introduced earlier now more precisely, the same word thing designates matters of fact and matters of concern' (Latour, 2004b: 233). Ingold, in a not dissimilar way, suggests that things are made up of flows and transformations of materials rather than form, like the notion of object implies: 'My ultimate aim, however, is to overthrow the model itself, and to replace it with an ontology that assigns primacy to processes of formation as against their final products, and to flows and transformations of materials as against states of matter[…] My purpose, in short, is to restore to life a world that has been effectively killed off in the pronouncements of theorists for whom, in the words of one of their more prominent spokespersons, the road to understanding and empathy lies in 'what people do with objects' (2007: 3).

> We define it [data streams] inclusively as the many different entities that scientists produce and use during the process of research. In this usage, data include a wide variety of materials, instruments, techniques, and written inscriptions. Such a broad definition is needed because scientists in every subfield have their own specialised conceptual categories for classifying the resources that they use and produce (1997: 30).

Considering data *as process*, in, what the author calls, 'data streams', expands our ways of understanding and studying data ethnographically. Indeed, I return to classification here, specifically in terms of the ways in which data need to relate to other data or kinds of knowledge in order for them to accomplish knowledge production. That is why considering data as relational assemblages is useful, because it shows that data are not detached outputs of scientific work, but embedded in science-policy relations, material resources and made-up by particular skills and techniques.

Data practices are a particularly good way of tracing both the combining of practices and articulation of objects of research in different ways, spanning different scientific settings, from 'the field, 'the lab' to the 'computer model' and 'multi-disciplinary dialogue'. Indeed, data, and harnessing the potential of data, has become a topic of shared concern between academia and the government (e.g. the Care.data programme)[17], with data becoming the subject and focus of several large research groups crossing the social and natural sciences (e.g. ESRC funded collaboration 'Socialising Big Data: identifying the risks and vulnerabilities of digital data-objects')[18]. These emerging collaborations on the theme of data, and especially 'big

[17] The Care.data programme is a controversial NHS programme which enables the sharing of data from patients' medical records by external bodies. The result of the Health and Social Care Act 2012, it provides the Health and Social Care Information Centre with the powers to collect previously anonymous patient data from primary care (the process already exists for secondary care). The hope is that collected data will be able to be used by researchers to look at population health issues, i.e. being able to make linkages of certain health issues in a specific area of the country or by looking at linked morbidities that people have. The contentious aspect is to do with data safety, how is it stored, is it anonymised/identifiable?

[18] 'Data has the potential to transform public and private sector organisations, drive research and development, and increase productivity and innovation. The enormous volume and complexity of data that is now being collected by government departments, businesses and other organisations represents a significant resource

data', exemplify contemporary concerns around data and data practices which transgress traditionally conceived scientific, social, economic and political domains.

Terms such as 'big data' and 'the data deluge' (The Economist, 2010) have generated interest within social and cultural studies of science and the social sciences more broadly. Attention to what has been called 'the social lives of data' has orientated sociological and anthropological curiosity, with studies looking at the construction of scientific relations with data and data as particular kinds of research objects (Helgesson, 2010; Hine, 2006; Leonelli, 2009; 2010; Levin, 2013; Michael, 2004; Walford, 2013; Zimmerman, 2003). Ethnographic engagement with such practices have offered new methodological and theoretical insights into scientific work (Hilgartner and Brandt-Rauf, 1994), in terms of the socio-material technical relations and processes of construction, care and exchange which data practices comprise (Borgman, 2010; Edwards et al., 2011; Leonelli, 2014; Sunderberg, 2006). These developments highlight the materiality of data and data practices, inclusive of both the concrete and physical with the digital and virtual kinds of engagement with phenomena.

Data are inherently mutable, they are both fleeting and substantial; a taken for granted starting point for research and also the outcome of scientific practice. It is for this reason that until recently data have very often been black-boxed. Yet, like all scientific objects, these data are comprised of situated meanings and material relations. As Suchmann states, culturally and historically constituted, social and material conditions of particular practices, both provide and constrain the conditions of these, so that 'what is happening here and now' must be 'actively re-enacted in ways not fully specified in advance or in any strongly determinate ways' (Suchmann, 2007: 52). Suchmann argues that studying scientific objects means focusing on the actions which animate them. This movement of data between different practices implies a shift in their meanings and relations. How, and in what material ways, data are enacted is both of interest to those studying science and to those practitioners

within the UK which can be used to the mutual benefit of academic research, organisations and society as a whole' (ESRC, 2014) using ‘big data’, which, as I have pointed out, is taking place in complex and distributed ways.

A material-semiotic approach is one that highlights not only the process and contingency of practices, but the ways in which their objects are actively performed through the craft-work of these practices (Myers, 2007). As I have shown, data practices have been empirically studied, as material and semiotic processes comprising human and non-human relations and the beliefs and meanings imbricated within these. Studying data practices empirically is an opportunity to examine the informational and material dimensions of scientific practice, and also the ways in which data sharing makes up and mobilises research, scientific lives and socialities across different scientific settings. A multi-disciplinary project highlights not only current initiatives for the harnessing of the informational potential from data, but also the combining of different kinds of data in order to frame and study environmental health in unprecedented ways.

Making and crossing boundaries

So, in what ways are data enacted as research objects in material practices? How do these travel and move between different kinds of research settings? What kind of translations do these processes entail? How is air pollution at once multiple, insubstantial and a ‘shared’ research object?

Data sharing involves epistemological inconsistencies because data are the outcome of human interaction with the world (Leonelli, 2010: 28), and therefore the way in which multiple data are understood and brought to bear on each other may be contested. Some of the complexities that emerge in the sharing of data between sites of practice and epistemic cultures have been termed ‘data friction’ (Edwards et al., 2011), which include both technical hurdles, like incompatible hardware, software, and data structures, and also the very organisation of science with issues of authorship and ownership of data and claims arising (Zimmerman, 2008).

Sabina Leonelli’s analysis of the ways in which data get enrolled in order to make scientific claims (2009) shows how data remain very much attached to their site of locution. Through the following of data, and their movement between different research practices, specifically data-driven biology and the biomedical sciences, she develops the concept of 'baggage'. Baggage is the information required in order for data to be re-used in other practices and data settings, such as information on how the data are collected and produced. Sharing data, and facilitating data to travel between research practices, requires particular material formations and reflexive practices by those making and using data. Ann Zimmerman's PhD thesis (2003), and subsequent paper (2008), also examines the experiences of scientists (ecologists) who used data they did not collect themselves. By focusing on the ways in which ecologists understand and assess the quality of the data they reuse, she highlights the role that standard methods of data collection play in these processes. Standardisation is one means by which scientific knowledge is transported from local to public spheres, but Zimmerman also argues that standards are not always sufficient for the transportation of data from one context to another, stating that 'while standards can be helpful, my results show that knowledge of the local context is critical to ecologists reuse of data' (2008: 631).

So, although data sharing is often considered productive, in practice this process can be rather problematic. In principle, data collected in widely varying fields can now be assembled and brought to bear upon each other, leading to entirely new perspectives on complex natural, environmental and ecological systems (Edwards et al., 2011). The research of Edwards et al. focuses on the work practices of research scientists, software developers, data managers, and others involved in distributed 'e-science' projects. They argue, however, that 'it is time for science studies to investigate how data traverse personal, institutional, and disciplinary divides' (Ibid: 669). Using the empirical example of developing 'meta-data', the authors develop the notion of 'meta-data as process' to emphasise the working culture and practice of scientists who design and implement standards (where meta-data are the standardised descriptions of a set of data). In doing so, they conceive of the concept of 'science friction', as a way to come to understand and consider the issues which emerge when data travel among different disciplines and fields of practice:

> To put the point another way, consider the following analogy. Engineers reduce friction with precision – making interacting parts mesh better - and with lubricants. Typical discussions of metadata see them as contributing to precision, making it possible to join one part (dataset) more perfectly to another one. This may involve

> considerable effort at shaping and polishing a part - refining its metadata - to reduce its coefficient of friction. By contrast, the process view we explore here looks primarily at lubrication: the practices through which people overcome friction without precise solutions or the need to modify components. Does interdisciplinary data sharing work more like a fine Swiss watch, with dozens of gears and jewelled pivots so precisely engineered that they never need lubrication? Or does it work (as we believe) more like a car engine, running fast and hot, bathed constantly in motor oil to keep the parts from burning up? (Ibid: 670).

Edwards et al.(2011), Leonelli (2009; 2010) and Zimmerman's (2003; 2008) research all suggest that, although data may be conceived of as abstract and free-flowing, in practice data are no different from other kinds of scientific objects, in the sense that they are embedded within a complex of locally situated boundaries properties and meanings. Data, then, can be studied not simply as outcomes of research but as processes and practices of objects in the making. It is this 'ad-hoc' work which becomes important because, as Edwards et al. (2011) argue, this enables data to emerge, adapt and move between different fields of practice, which shape how problems are formulated, managed and responded to.

More traditional science studies also offer a way into the study of incoherence and complexity of scientific work carried out between different social and cultural ways of knowing. Star and Griesemer's (1989) concept of the 'boundary object', is a figure for both following and theorising 'difference' and 'co-ordination' in practices and processes of knowledge production. Boundary objects are defined as objects which both contain and constrain practices:

> [O]bjects which are both plastic enough to adapt to local needs and constraints of the several parties employing them, yet robust enough to maintain a common identity across sites (Ibid: 393).

Star and Griesemer examine how epistemic boundaries make processes of translation and standardisation difficult, and in doing so provide a model of practice through which tensions between different ways of knowing, and the values embedded within these, are made to relate in productive ways. At the same time, these acts are not necessarily ones of displacement or addition, but ones of co-ordination, which enable both researchers and those studying scientific practice to keep the heterogeneity and complexity of knowledge objects in focus.

Indeed, the construction and mobilisation of standards which transgress scientific and medical localities has received much attention in science studies. Timmermans and Berg agree with Latour, arguing that relational networks are required in order for 'universality' to be achieved. However, by focusing on the distributed relations of scientific and medical settings, and the taking seriously of difference and partiality within these, the authors develop a trajectory of the translation of interests where standardisation is achieved at the intersection of different practices, rather than disciplining of these practices in any singular kind of way. By studying standards they claim that every universality is 'local universality', but that this does not imply centralised scientific control, because standards are a distributed activity aided by a 'loose network'. As such 'universality' is an ambiguous and rather precarious state involving 'real-time work' and 'localised processes of negotiation', rather than a 'rupture with the local' it transforms and emerges through it (Timmermans and Berg, 1997: 275):

> Local universality, then, is about being in several locales at the same time, yet being always located as a product of contingent negotiations and pre-existing institutional and material relations […]In sum, local universality depends on how standards manage the tensions among transforming work practices while simultaneously being grounded in those practices (Ibid: 297-298).

Star and Griesemer and Timmerman and Bergs' empirical investigations into boundary objects and the standardisation of practices challenges the, what Star and Griesemer call, 'Callon- Latour-Law model' of translations where this is only one 'point of passage' between social worlds. Instead, boundary objects and notion of shared standards brings to the fore the multiple passage points and translations which take place in settings where a diverse number of human and non-human actors are at stake. As Timmerman and Berg show, 'looseness 'of the network of relations and ambiguity around, say, objects of research is productive of co-ordinating and performing objects across different locales.

Star and Griesemer's model of translation has been developed in a number of ways, used to trace and understand the ways in which science is translated across different domains of practice and their principles and values. For example, Fujimura's concept of 'standardised packages' (1992) highlights the craft-work and particular modes of doing that get mobilised when heterogeneity and difference emerge. By highlighting both standardisation and translation Fujimura connects particularities of practices with wider scientific relations, but also the formation of spaces through which translation can take place. Fujimura talks about interfaces between research practices to emphasise the productive connections these enable, as the places where different social worlds intersect, and the means by which interaction or communication is effected (Ibid: 170). As such, Fujimura shows that the crafting of homologies between laboratories and communities and between inscriptions and laboratories enabled the emergence of a material, technical but also conceptual work space for researchers, and which resulted in 'a new and highly privileged genetic representation of cancer' (Ibid: 170).

In a different way, Galison's notion of the 'trading zone' (1996) adds a spatial, temporal and cumulative dimension to the boundary object, emphasising the transformative, to and fro process of interdisciplinary knowledge production. Using the analogy of trade, Galison shows how a collaboration of researchers from discrete scientific fields researching the Atomic Bomb were brought together despite differences in language and culture. Stressing the 'goods' from which both sides of the transaction benefited, Galison demonstrates that, over time, a shared language developed, which led, ultimately, to the emergence of a new field site. He argues that differences are often productive, enabling the emergence of new ways of talking about and conceptualising scientific and technical processes. In the case of his field work, difference led to the development of a new kind of 'pidgin language', and ultimately to the extension of the boundaries of practices and the formation of new spaces of practice.

The very rationale for sharing data is that they enable different kinds of information to be used together in new and creative ways. Data are both the output and the everyday objects of research practices and therefore of science in the making. Moreover, data are both representations which embody ways of knowing whilst also prescriptive of particular kinds of engagements with the world: as models of phenomena, but also as models which shape the material arrangements of scientific settings. Indeed, Geoffrey Bowker (2010) has suggested that databases are a performative infrastructure. Starting off with the nature of classification in biodiversity policy, he argues that 'data management' is an important area of research for science studies because they are sites where models of the world are created. He is interested in what does and does not get represented in databases, because what is not classified gets rendered invisible:

> If certain kinds of entities and certain kinds of context are being excluded from entering into the databases we are creating, and those entities and contexts share the features that they are singular in space and time, then we are producing a set of models of the world which…is constraining us generally to converge on descriptions of the world in terms of repeatable entities (Ibid: 655).

Bowker suggests that data enfold as many organisational and social decisions as scientific texts or the other material and informational inscriptions that scientists produce. He also points to the multiple disciplines involved and who seek to draw on databases for theorising biodiversity as a useful mirror for our current modes of interaction with 'nature'. So, rather than only looking at what and how people use data to make knowledge claims, Bowker suggests examining modes of data production and organisation of data before it is used.

Considering Bowker's findings in the context of the WHAP project, as multi-disciplinary research using multiple kinds of data to study entities, where the boundaries of classification and measurement are rather uncertain, is useful. His work suggests that the ways in which air pollution come to be known can also be studied through the various ways in which data are combined and structured across multiple research practices. Part of the problem that Bowker points to is the role of standards and the impossibility of stabilising these in contexts, practices and organisational systems of data, because of data's inherent varying temporalities, spatialities and materialities, which all require flexibility. Thus, working with data is wrapped up in ontological questions around what kind of thing 'biodiversity' is. Attending to the local ordering and alignment of data across multiple disciplines requires creativity, because it is not simply about finding a commonly accepted set of spatial and temporal units, or as Bowker calls it 'a naming convention', but one which involves the manipulation of ontologically diverse data.

As much as I am going to be focusing on the situated practice of knowledge production, what is significant to the WHAP project is its disciplinary and geographically distributed nature. In this way, the ways in which different practices are 'held together' is significant to the kinds of enactments multiple data make possible, both within science and policy. Indeed, the appeal of sharing large data sets and engaging in multi-disciplinary, multi-institutional research as a result of the extent and global dimensions of data is part of what Gitelman and Jackson call 'the imagination of data':

> At a certain level the collection and management of data may be said to presuppose interpretation. "Data [do] not exist", Lev Manovich explains, "they have to be generated". Data need to be imagined as data to exist and function as such, and the imagination of data entails an interpretive base (2013: 3).

The capacities of data to be used in ways which will offer solutions to complex contemporary environmental health problems presuppose a particular kind of solution. Further, for those studying data practice, the assumptions and discernment of data suggest that we should not be looking (or studying) *at data* but *under data* to consider their root assumptions. Studying data in the making is, then, an opportunity to study the social and cultural workings of data in process, before they are black-boxed and difficult to unpack, but also the ways in which the world is being imagined and made visible through research practices.

The politics of 'the environment' and 'health'

In Chapter One I provided a brief history of air pollution, highlighting the socio-technical networks of relations which make-up air pollution as a science and policy problem. What I emphasised was the ways in which air pollution is co-produced through scientific knowledge production, practices of measurement and qualitative scales of risk. More recently, the social and cultural entanglements of science and policy have been re-framed by Latour as orientated around 'matters of concern' (Latour, 2004b; Latour, 2004a). 'Matters of concern' is a term he uses in contrast to 'matters of fact' and as shorthand for refusing the distinction between what can be disputed, such as values, and what can't, such as observational data (Whatmore, 2006):

> To indicate the direction of the argument, I want to show that while the Enlightenment profited largely from the disposition of a very powerful descriptive tool, that of matters of fact, which were excellent for debunking quite a lot of beliefs, powers, and illusions, it found itself totally disarmed once matters of fact,

> in turn, were eaten up by the same debunking impetus. After that, the lights of the Enlightenment were slowly turned off, and some sort of darkness appears to have fallen on campuses. My question is thus: Can we devise another powerful descriptive tool that deals this time with matters of concern and whose import then will no longer be to debunk but to protect and to care, as Donna Haraway would put it? Is it really possible to transform the critical urge in the ethos of someone who adds reality to matters of fact and not subtract reality? To put it another way, what's the difference between deconstruction and constructivism? (Latour, 2004b: 278).

This re-conceptualisation of scientific issues means that science and politics can be analysed as partial and uncertain in ways that does not beckon a deconstructivist mode. This shift paves a way for science studies to intervene and interact with science, whilst taking seriously their claims about the world.

The politics of scientific knowledge has been examined by anthropologists in terms of science's 'generative capacities' (Fischer, 2007), and how particular scientific actions relate to wider infrastructures of power and action and 'global assemblages' (Ong and Collier, 2005). For example, anthropologists Kim and Mike Fortun's 'trans-disciplinary' research on asthma illustrates the importance of involving different kinds of actors in coming to understand and respond to asthma - 'embracing the real-world complexity of social epidemiology of asthma' (Fortun et al., 2014). By studying asthma as an embodied experience and in terms of the 'new reflexive social institutions for decision making' which emerge in spaces of environmental, social, medical and political convergence, the authors draw upon the scientific and political nature of asthma as both a public health problem and scientific knowledge problem.

As Sarah Whatmore, a geographer, argues, materialist concerns have profound ethical and political, as well as analytical, consequences (2006). The explicit focus on the co-fabrication of socio-material worlds, rather than say the more epistemological discourse of co-construction (Jasanoff, 2004), results from understanding agency as distributed across human and non-human things. The human and non-human relations mobilised in scientific practices can, of course, be extended to the political domain, as Lezaun and Marres' special issue of 'Economy and Society' on material participation demonstrates (2011). The foregrounding of 'the material', as, for example, a 'participatory device' (Marres, 2012b) has meant highlighting the political capacities of materials in constituting politics and publics. Although I am focusing primarily on science in action, the WHAP project has a strong policy stance, which means not only am I studying the making and materialising of scientific research objects but also how these are configured and translated into policy settings. A material and, specifically, data-centric approach to environmental health concerns like air pollution is significant because, as Fortun has shown, data and information perform particular kinds of environments (Fortun, 2004). Furthermore, as Mol's empirical philosophy illustrates, particular practices and 'the cuts' these make are always political because they enact contingent realities (Mol, 2002; 2001).

The process of making, producing and re-using data is an opportunity to examine the process and emergence of 'environments in the making' and how human actions are politicised as part of air pollutions materialisation. The politics of air is a good place to start examining human non-human entanglements because, as Peter Adey points out, 'we inhabit air' (2013), which means it 'makes real' the abstract representations of places, spaces and temporalities which constitute knowledge making. One of these abstractions, I think, is information, and data is one of the ways in which information is materialised, and made to travel and do things. Understanding 'the environment' has always required large amount of information to be collected and that this is now taking place on an unprecedented scale and in digital and often automated form, has consequences on how we come to know and participate in environments (Fortun, 2012: 321). As Fortun's study of environmental information systems suggests, information is playing an increasingly performative role:

> […] setting up comparison or connecting bits of information previously unrelated performs cultural work. So do click throughs. Zooming in and out, learning to consider the implications of scale involves what Antonio Gramsci termed 'elaboration', the labour of working out common sense. This kind of labour can't be reproductive. It involves a play of signs and systems that is always unsettling (Ibid: 322).

The process of 'informatting' 'the environment' points to the possibilities for its constant reordering and revisualisation. In light of the non-humanist materialities emphasised in recent science studies however, I argue, that Fortun's focus of 'informatting technologies' also highlights the performance of natures-cultures which make up ‘the environment(s)’ and the interfaces between phenomena of research, science and politics. Indeed, new technologies and ways of collecting and using information means considering the ways in which the material qualities (Ingold, 2007) of phenomena play out in different actions and interactions. For example, environmental data practices are often oriented toward collecting large amounts of data as part of ongoing practices of collection and storage, but are also used as a way to trace patterns and relationships between environments and environmental action. The framing of environmental problems is also frequently the starting point for the generation of more data.

The relations between data constitute new capacities for coming to know and experiencing environments in knowledge practices. As Jennifer Gabrys’ research on environmental sensing shows, new instruments, such as air pollution sensing devices, transform how we sense, know and take action as parts of environments:

> Data may be correlated across sensor types, or sensors may trigger other sensors to capture phenomena or trigger actuators to collect samples for later study. Inferences can be made about phenomena through sensors and actuators, and sensors can be arranged through flexible, multiscalar platforms that investigate particular sensing relationships (2012).

‘Environmental sense data’ can be used in present time or collected for storage and later use, they can also cross scales and be re-arranged in ways which make new relations visible. What Gabrys’ work emphasises is the way in which collecting and making data is an experience which emerges across human and more-than-human subjects. This extension of what participation means in ‘sensing environments’, as varied forms of techno-scientific practices, is revealing as to the shaping and transforming capacities of collective practice. Environmental data makes visible the different kinds of environments and different ways of measuring and producing data on these. As Bowker suggests, ways of handling these multiple contingencies by practitioners themselves is a way to understand and trace how worlds are being made, not simply in how data are used but how they come into being in the first place are organised, processed and re-constituted.

Summary

In this chapter I have traced the emergence of data as an object of study in social and cultural studies of science, presenting some of the ways in which I have come to understand and thereby frame my ethnographic study of data practices. In a context where multiple data practices and diverse kinds of data are being made, processed and shared, studying data practices in a multi-disciplinary, multi-sited collaborative project like WHAP is an opportunity to consider the kind of interactions and interventions which emerge and are made possible.

The empirical case studies I presented exemplify a theoretical and methodological approach which emphasises the material, embodied and performative dimensions of knowledge practices. Classifications become more than nomenclature, or simplification, because they frame particular ways of seeing, knowing and acting, and therefore always have social, material and political implications. But so too, Mol and Law remind us, can whatever escapes the paradigm, the episteme, consciousness (2002: 5), because we live in worlds which overlap and co-exist. Indeed rather than a flow from one data practice to another, in a linear fashion (as outlined by Leonelli and Zimmerman) the WHAP project suggests a more circular movement - between different sites of practices, disciplinary commitments, objects of research, standards and protocols – motivated by a rhetoric of *doing together*.

I suggest studying data practices in process and combination is an opportunity to consider the ways in which data are forever contextualised (Gitelman and Jackson, 2013: 6), the different ways in which data are interpreted along a trajectory of collection to implementation; the material and relational capacities and constraints of multiple kinds of data; and the related articulations and imaginations which data in combination generate. A multi-disciplinary project like WHAP is a particularly good case study for understanding some of the everyday practices and constraints of doing science when there are multiple ways of coming to know and materialise objects of research. Moreover, the sharing and translating of data across practices is one of the defining features of WHAP and its approach to studying air pollution. Tracing the multiple trajectories of data is a way into examine the material and relational ways in which data contain and constrain knowledge practices across different sites of practice, but also the social role data play in establishing and maintaining professional relationships.

I suggest that data practices are not just particular kinds of scientific practice but are also the means by which to re-consider the social and cultural theorising of science itself. By focusing on scholarship studying data and data practices I have highlighted some of their contributions to science studies more broadly, particularly in relation to the movement and transformation of knowledge across different social, technical and material realms of practice. I am now going to show, however, that data practices also open up new kinds of ethnographic engagement with science. Data, then, are not the end point but starting point for further iterations, transformations and new modes of engagement with objects of research, both in science and the social sciences more broadly. In the following section, Part II, I am going to examine the particular kinds of data practices which produce data on air pollution. I study in detail two particular practices and processes which make data, before tracing the capacities and constraints of data as they move between different research practices.

Part II - Internal difference: doing *multiplicity* and negotiating *co-ordination*

Chapter 4 Multiplicity in practice: making data, enacting air pollution

Introduction: Measuring air pollution

In this chapter, I am going to examine two different measurement practices which work to produce particular scientific objects in practice. The two measurement practices are called 'modelling' and 'monitoring' by researchers, each providing numerical information on the amount of particular air pollutants in the air. These measurements are processed as data, forming what became colloquially referred to in WHAP as 'modelled data' and 'monitored data'. I trace the ways in which modelling and monitoring data practices are similar and different, and how they are made through particular scientific engagements with the world, to create different versions of air pollution as research objects in practice (Mol, 2002). In doing so I draw upon interest within anthropological and STS scholarship on the multiplicity of objects which emerge when one focuses on what people do rather than what people say, and on the processes of science in the making rather than the products of these practices.

As I have shown in the chapters so far, there is no single order of air pollution, and the very premise of the WHAP project was based on air pollution's heterogeneous and complex nature. What I also examine through the study of two different measurement practices, then, is the kinds of research objects they produce and the episteme these inhabit. My analysis of multiplicity is, therefore, also an analysis of difference, and I pay attention to the ways in which difference gets made and re-produced through two particular data practices.

The simulation model and the monitor both function as 'technological objects', which embody and materialise a set of heterogeneous relations, by participating in the bringing together of different human and non-human actors (Akrich, 1992: 205/6). A computer model and a monitor are different kinds of technological objects. Computer models are considered as approximate imitations of natural systems, built through complex mathematical equations and theoretical knowledge of the atmosphere. These are used to simulate the dynamic movement, fluxes and flows of the atmosphere in order to produce measurements of the concentration of air pollutants within the atmosphere. In contrast, monitors are placed in particular locations in order to capture air and measure the amount of air pollution within it. Both these technological objects produce data and participate in the construction of air pollution as a research object, mediating and (re)constructing the boundary between phenomena and their representation in particular instances.

Although modelling and monitoring are different types of techno-scientific practices, each use an instrument which describes the amount (a concentration) of a particular air pollutant in an air sample under certain conditions. What is of interest for each practice is, in the first place, air rather than air pollution: how to capture it (monitoring) and how to re-produce it (modelling). It is the measurement contexts, including temperature, time of day, season, location, for example, that make the measurement meaningful. These contexts are classified as types of air or atmospheric conditions. I found that, in both measurement practices, the measure of an air pollutant was made sense of in terms of its given context, and therefore the type of air that it is entangled with. As such, air was understood as a relational entity, and something that could not be separated from the material relations of which it is a part.

By focusing on how technological objects are used and what they enable, a more democratic study of knowledge practices is possible. In doing so, the agencies of non-human participants are brought to the fore and, consequently, a shift from scientists' cognitive ability to the material practices and processes of manipulation of which they form a part. As Alkrich writes, to examine science and the articulation of research objects in practice and process one needs 'to find circumstances in which the inside and outside of objects are not well matched' (Akrich, 1992: 207). It is the unstable relation between a measurement and the phenomena being measured that I examine in this chapter. Accordingly, my analytical focus attends to the relationship between different statuses of measurement, as data or error, and how these are collectively[19] managed in two different practices. Checking for error was a formalised procedure in measurement practices, which enable the subsequent data to carry meaning and authenticity beyond their context of production. Working out error in measurement practices is, then, a key part of stabilising the relationship between a measurement (a representation) and that which is being measured (air pollution as a research object).

Case study 1: Monitoring instruments

> We start by climbing up the outdoor stairway to the roof of the school. On top of the roof I see a grey porta-cabin. Phil opens the triple locked door and we enter a small, square room, which is about four by four meters in size. I am told the reason for visiting this monitoring site is the size, usually only one person can fit into a station. On entering the room, I am greeted by a set of what looks like four large rectangular boxes stacked on top of each other supported by a shelving unit. To the right of the stacked boxes is, what looks like, an electrics cabinet, which requires a key to open it. Inside are two tubes, one attached to an outlet in the roof and the other connecting to the four stacked boxes. To the left of the shelving unit are two gas canisters [which later I learnt host the differed certified gases] (Fieldnotes 25th October 2012).

The monitoring station is situated on the roof of a primary school. It is a 'background'[20] monitoring station and has been used to collect measurements for seventeen years. The area has four other sites, and this is known as 'number one', which relates to its relatively long history and considered good setting. Phil visits the site every two weeks to test the calibration equipment. Calibration is a process whereby the measurement made is compared with another, stable measurement in order to test the measurement made for error. This is one part of a much more elaborate process of testing data for error. Indeed, monitoring sites are also visited by

[19]By 'collectively', I refer to the different human and non-human associations which are gathered in the assembling of measurements as data (see Latour (2004a) on 'the collective' as an extension of human and non-humans)

[20] Where monitors are placed relates to the character of the surrounding environment. Monitor locations are then classified according to these surroundings. The classifications 'urban background' and 'rural background' signify that the monitor are measuring the lowest levels of air pollution in that surrounding area. In contrast, 'road-side' denotes a point which is considered as having high levels of air pollution, but which is not considered as 'representative' of the wider area.

engineers and auditors, so the site check I attended was one among many others ‘to ensure quality data’ (Phil, Fieldnotes 25th October 2012).

The location of monitoring stations is significant to the making of measurements, and the siting process an important aspect of the process of monitoring air pollution. There are both practical and epistemological issues to consider. For example, a DEFRA manual (2006) for local authorities installing monitoring stations outlines some of the considerations that need to be taken into account when setting up a monitoring station; they must not be ‘enclosed by surrounding buildings or covered by overhanging vegetation, or placed at a sampling height of between two and five metres, not close to local or point source emissions unless these have been specifically targeted for investigation’ (DEFRA, 2009: 3-3). The actual siting of the monitoring station shapes the kind of air that is being measured. These air settings are classified as ‘urban’, ‘rural’, or ‘background’. DEFRA’s guidelines, which I reference above, refer to the ‘background’ type of air, which is why the third point states that the monitoring station should not be located close to emission sources, such as a busy road. These classifications of spaces of air, help analysts and scientists interpret the meaning of the measurements made at monitoring stations and subsequently the kind of data they become.

The site of the monitoring station seemed to be significant to Phil too. Within the first few minutes of our meeting he provided me with a brief description of the vicinity:

> The site is located on top of a school, in a densely populated, low income area, with a high proportion of social housing. All these factors, he explains, mean air pollution is disproportionality affecting the health of people with a low socio-economic status (Ibid).

The site, then, bore more meaning to Phil than the protocol makes reference to. It is not only practical and epistemic, but social and political. The making of measures and the meaning these have appeals to ideas and values which underpin the more standardised procedures of ‘testing the instrument’. Indeed, the data relate to the monitoring of the air that more vulnerable populations breathe, and are a way to make visible the disproportional effects of the social and natural environment on people.

Air pollution is monitored across the UK, initiated by central government and often carried out by local councils. Measurements are collected at different monitoring sites, and these are organised into different networks according to the location of the site. For example, London has one accredited ‘air quality network’ managed by government departments, local councils, university research groups and environmental agencies. The aim of these quite elaborate networks is the production and maintenance of ‘continuous data’. This means that past and present data on air pollution are sustained through the network, from production to storage, making up what is called a ‘data archive’. The purpose of these data are stated as two fold, as providing the public and authorities ‘real-time’ information on current air pollution levels, and to enable short term and long term responses to air pollution as a public health concern (DEFRA, 2012a).

Using the data produced at monitoring stations, ‘air quality’ is then reported on a scale which intends to make the actual concentrations of air pollution meaningful for the public, inciting recommendations and actions, such as advising asthma sufferers to stay indoors. Types of air and the types of pollutants which monitors produce information on are organised according to these different classification systems: the classification of types of air (background, rural, road side etc.); the classification of air quality as a numerical scale of bad to good air; and a classification of air pollution into different pollutants. At the site I visited four monitors measured four different pollutants: Ozone (O_3), Oxides of Nitrogen (NO_x), Sulphur Dioxide (SO_2) and Particulate Matter (PM).

Zero air and the calibration test

‘Error’ is a term which, in our case here, denotes a measurement that is not considered as representing air pollution properly. There are multiple reasons for error and the aim of the manual calibration check is to control and account for some of these. In terms of technical error, the calibration test is a process of ‘tuning’ the instrument to ensure the measurements it makes are accurate. To make a measurement it is essential that the instrument used does not influence the measurement being taken. The main cause of error which calibration checks for, is 'the drift' of the instrument from 'zero'. Zero is not a null reading, but a term used to denote a baseline from which a measurement can be made, implying an unmediated and therefore 'scientific' setting. If the baseline is not zero, then the instrument is drifting above or below this baseline, which means the subsequent measurement will be erroneous by the amount the instrument has drifted. Drift is a measurement too, and used to account for how much the measurement made is erroneous by.

Zero air also signifies 'pure air', which means air with no air pollutants in it. This can't be found or discovered but has to be made using something called a scrubber, a name which quite literally describes its role in getting rid of - scrubbing away - parts of the air which are not being measured for. The notion of pure air was referred to by Phil as an external reference material, made either through these technical processes in the instrument or at a laboratory, as a certified standard which can be physically introduced into the measurement setting. Without this fabricated material reference point, the measurement made on-site cannot be stabilised as data.

In a paper on the making of legal reference materials, Javier Lezaun argues that reference materials for phenomena involve a fundamental tension in our understandings about the relationship between the authority of an official substance and its material nature (2012: 23)[21]. In our case study here, the pure air reference which gets used in the calibration practice works as an 'objective version' of air with no air pollution. In practice, Phil mediates the standard and the actual measurement in his manual calibration test, where the standard is used to get purchase on the

[21] Lezaun writes: 'The original condition of the material can never be preserved. It can, however, be reconstituted. This is why the attempt to produce increasingly commutable references, material standards that in some important way are truer to their world form, is fundamentally an artistic pursuit. Naturalness must be recreated, a process that far exceeds in sophistication and skill the mere preservation of form the goal is not the maintenance of the source material and its unadulterated state, but rather the production of a particular kind of verisimilitude' (2012: 31).

authenticity of the measurement made. In this way, pure air is created as a way to produce a kind of truth through which a measurement can be made.

The measurement

Each monitor has unique components to measure specific pollutants, through which they take an air sample and, with a sensor, measure the concentration of an air pollutant within it. The metric is micrograms (one-millionth of a gram) per cubic meter air (µg/m3). The air samples are drawn into tubes by a pump unit connecting the outside of the station with the indoor instruments (See Figure 3). The sensor I discuss here functions by the passing of a UV light beam through the tube where the air sample is taken. The sensors reveal the amount of a pollutant in a given air sample. Exactly how this is done is specific for each pollutant. The tubes are called single reaction cells, which are fitted with pneumatic valves. These enable the tube to switch between zero and ambient (the air sampled) air paths and the absorption is then measured using a UV detector.

> […] alternately measuring the absorption of the air path with no Ozone present (zero air) and the absorption in the ambient sample. Gases pass through these UV beams and absorb some of the transmitted energy, which appear in the measured absorbance data (Fieldnotes 25th October 2012).

The reading is the level of absorption of the pollutant in the UV beam. The switching between different air paths is a calibration test, which compares these two measurements. This is carried out within the monitor for every measurement made, and without this comparative baseline no measurement can be made.

Figure 3: Inside an air pollution monitor (Personal photo).

The measurements I focus on here are those made by measuring UV light absorbance of a particular pollutant in an air sample. Each gas, O_3, NOx and SO_4, is measured slightly differently by the UV light. For O_3, the measurement is made by measuring the absorbance of UV light by the pollutant, the absorbance measure being analogous to the concentration of the air pollutant. The proportion of the UV light absorbed is equal to the proportion of gas in the air. However, for SO_4, the ambient air sample drawn into the monitor from outside is exposed to UV light, which ‘excites molecules to higher but unstable excited states’, these ‘excited states decay, giving rise to the emission of secondary fluorescent radiation’ (Principles of Operation, AURN manual). It is this reaction, which is detected within the tube, and which causes an output voltage proportional to SO_4 concentrations. NOx are measured in a similar way to SO_4, where the intensity of the chemiluminiscent radiation is measured using a photomultiplier tube, and the detector output voltage is proportional to the concentration. The pollutant of interest, NO_2, is calculated from the difference between mono-nitrogen oxides of Nitrogen (NOx) and Oxides of Nitrogen (NO), and the ambient air sample is divided into two streams; in one, ambient NO_2 is reduced to NO using a molybdenum catalyst before reaction. The reaction of the instrument, which for each pollutant works in slightly different ways, is captured as a measurement of the pollutant. Within each process of measurement, then, the air sample undergoes a series of transformations, involving chemical reactions and the different ‘states’ these produce, as the air sample moves through the different parts of the measurement apparatus.

Seeing error

At the monitoring site I visited I observed an on-site calibration test. This is a standardised procedure which accounts for technical error in the monitoring instrument. This is similar to an internal calibration test, which is carried out automatically by the monitor, but the zero air and air pollutant are input externally not internally from standardised measures held in gas canisters within the monitor, rather than the making of zero air within the instrument and taking an air sample from outside. The purpose of ‘zeroing’ air is to check that the internal calibration equipment of the monitor is working. The zero gas canister is laboratory tested so that ‘everything inside is known’, emphasised Phil. The polluted air, in contrast to the zero air, is similar in that it is a known measure, which is also laboratory accredited, and because these measures are known to be ‘true’ they work as a way to test the instrument. In a calibration test the two airs are measured and compared. Ideally, the readings on the front of the monitor should be the same as the measure in the gas canisters.

Looking for this span and drift in a calibration test means waiting for the reaction to take place in order for a stable measurement to be taken, both for the zero air and calibration gas. Phil has to wait at least ten minutes in order for the analyser (the name used to refer to the piece of equipment that makes the measurement) to achieve a stable reading and therefore to make a measurement. When carrying out the manual calibration test, Phil explains that by using a baseline zero you can measure the effectiveness of the system: ‘I am looking for the readings to stabilise […] so to stay at around the same number to check all is functioning ok’ (Phil, Fieldnotes 25th October 2012).

The flux that results from the manual calibration test is visible to an expert eye. Whilst I sat next to Phil during my site visit, a series of numbers appeared very quickly on the small screen at the front of the monitoring boxes. Indeed, numbers were continuously shown on the front of the box as air is continuously pumped through the tube and measured. The fluctuation of numbers on the front of the box is a result of the introduction of span calibration gases. I was told that during normal operation, when automatic calibration takes place, the display alternates between the average pollutant concentration and the real-time pollutant concentration. The current pressure and temperature are also displayed as a way to perform the stable setting from which the measurements are made.

> As the numbers on the front of the monitor start to slow down, Phil tells me that the display on the front of the monitor box is the 'zero reading'. Phil types this into the spreadsheet under the table 'data acquisition response' (Phil, field note diary 25th October 2012).

The spread-sheet is a formal procedure of record keeping, but also one which submits the numerical reading to a series of further transformations: 'because Excel is also a calculating tool'. The readings of 'zero' and that of the calibration gas are compared by a mathematical equation, which then provides a measure of error[22]. The same process of pressing zero and running the monitor is carried out on each monitor. However, the process of reading and interpreting the sequence of numbers, which stabilise as a measurement is, explained Phil, contingent on the pollutant being measured:

> It is different for different pollutants [...] for calibrating Ozone you would check another monitoring station to see if it is similar, as Ozone is stable over a regional area... You wouldn't give a standard, like for PM_{10} [because] it stays around the same amount each time (Ibid).

This suggests that the process of carrying out a calibration test is not simply a technical procedure, but draws upon in situ experience informing the interpretation of these different readings. For example, in contrast to NO_2, the instrument used to measure O_3 is not carried out on site, but regularly recalibrated under laboratory conditions. This is because O_3 is an unstable form of Oxygen and will readily combine with other atoms, which means it is not possible to have a gas cylinder with

[22] In the journey of data from the monitoring sites to the central database where measures of uncertainty are worked out, the results of the calibration test can be used so that in order for error to be accounted for in the 'final data'.

a known concentration for calibration purposes. Phil explains that for monitoring O_3, the internal analyser equipment has an on-board generator that produces O_3 so that when carrying out a manual calibration Phil can look for consistency between these readings (Personal Correspondence 30th October 2012). This specific practice of calibration is shaped by O_3 being 'a regionally stable pollutant', and therefore comparisons with other monitor readings of O_3 work well as a reference material. However, for something like PM, this is not the case, as particles are unstable in space and time. This comparative case of measuring different pollutants suggests that, 'to read the numbers on the screen' involves knowledge of the relation between the 'properties of air pollution', and 'how they relate to the instrument' being used to take the measurement.

> I ask Phil how he knows what the numbers mean, and he responds by stating that meaning comes from 'experience [...] you need an eye to know what to look for'. Expanding on this notion of experience, Phil highlights the embodied aspect of doing this kind of work, through which someone can develop 'a good eye' (Fiednotes 25th October 2012).

This means that experience of carrying out calibration tests enables one to 'know what to look for', drawing upon the age old distinction between seeing and knowing[23], and exemplifying the symbiotic relationship between seeing and knowing in practice. Indeed, Phil went on to compare his own experience and knowledge of seeing and thereby getting 'good data' with 'non-data analysers' (specifically local authorities and inexperienced technicians) as people who 'don't know what to look for' and that, therefore, 'their data is not of a high enough quality' (Ibid).

These descriptions are all recorded and input into the spreadsheet by Phil, which contribute to the archive of air pollution monitoring data. Indeed, the maintenance of this record of measurements and calibration results was the second major task of visiting the site, during which Phil explicitly referred to his inputting of data in the spreadsheet as 'a record keeping exercise'. Whilst running the calibration test, Phil balanced his laptop on his knees and opened up the spread-sheet ready to input the

[23] See Lynch and Woolgar (1990).

recordings he made. The spread sheet is a table which structures the measurements, with a list of variables including the name of the monitoring site, the date, time, temperature in the cabin, and the calibration results. This record keeping forms part of the process of checking for error and the cleaning of data, which highlights the ways in which these efforts are made traceable and visible for his colleagues and for others who will attend the monitoring site in the future or re-use the data at a later date:

> [These] records go straight into a data base. Constant data is the aim and records of calibration results are kept and put into the data base for the time period […] and you scale it [the data from the monitor] until the next time someone comes to the site [according to the calibration results of this visit] (Fieldnotes 25th October 2012; technicalities confirmed via email, 30th October 2012).

The calibration results then become attached to the measurements made by the monitor, and in the future data analysis carried out offsite, these results can be drawn upon to check and explain the measurements made and, accordingly, make any adjustments required.

Cleaning data

Some of the problems that may arise in the instruments over time, and which calibration tests check for, are categorised as 'instrument performance' and 'instrument interference'. Like all the different parts of checking for error, these processes of checking for interferences in the measurement process are important because interference, like drift (error produced within the instrument), stops the monitor from making continuous, stable and representative measurements. In Claes-Frederik Helgesson's (2010) study of hospital data he described the process of 'cleaning data' as a series of practices which worked to rid 'dirty data' from errors and omittances, in order to make data more 'clean' and therefore credible. What is interesting in his account of cleaning practices of hospital data is the way in which the references used to clean data did not often come from the actual process of data capture:

> Re-visiting the initial act of capture is rare, even when a data point is found to need correction. Instead other referents are used, both for identifying peculiarities and for informing what corrections to make (Helgesson, 2010: 61).

Similarly, in the calibration of the monitor instruments, I found that checking the measurement for error was rather recursive, in the sense that the references used were previous measurements, and the standards used as an external reference point produced within the experimental setting.

In air pollution monitoring, cleaning procedures are governed by standardised protocols and related thresholds of validity according to UK and EU legislation, and like Helgesson's study, cleaning meant the data become further shaped and formatted by influences from outside the situation of initial capture (2010: 61). However, unlike the cleaning practices of Helgesson's hospital data, in air pollution monitoring the cleaning practices are recorded, maintained and sustained. Cleaning data is part of the history of air pollution monitoring. Indeed, during the site visit, Phil emphasised the importance of maintaining the records for 'data capture'. Phil played a role in the production of data, tracing in his account of this description process the proceeding journey of these 'captures' to their stabilisation as data, a state referred to as 'ratified data' and which is carried out off-site.

The aim of producing monitored data is also highlighted in an extract from DEFRA's 'quality analysis/quality control' manual, which states that results must be comparable, consistent and representative (DEFRA, 2012b). Like Phil's insistence on maintaining data capture, the manual sets out the ideals which underpin this maintenance. The link made between continuous data and its being comparable, reproducible and representative points to the wider social and political context within which this data practice is immersed. Indeed, it is this 'public' for which data is intended, and also the research bodies which go on to re-use this data in scientific studies, that means cleaning is made so explicit. I suggest that cleaning is a technical, scientific process here, which contrasts to Helgesson's findings relating to the cleaning of hospital data as administrative rather than real science, because the practices of checking for error are part of the making of continuous measurements.

It was at these in-between stages of data acquisition and stabilisation where the practices and processes which contribute to the making of scientific objects are visible, before they are black-boxed[24] as data. The flux and flow of air pollution in time and space means that measurements need to be 'continuously' made, so that the spatial and temporal patterns of air pollution can be rendered visible as patterns in data. Yet, each measurement is discrete and therefore discontinuous, and much work goes into constructing 'continuity' through the consecutive measurements made, primarily in terms of the form data takes as part of an evolving, continuous data set which is claimed to capture air pollution in 'real time'. Of course, there are gaps between each measurement (every fifteen seconds), and continuity is instead constructed by these sorts of cleaning practices.

Continuity is a useful metaphor to think analytically about this process of getting 'good data' too. Air pollution was conceptualised by Phil as something which is always in emergence and therefore continuous. However, continuity is difficult to measure in practice and one of the ways in which continuity was constructed in practice was through maintaining the material context of measurement which remained identifiable and attached to measurements in their journey to becoming data. Continuity was also constructed through the maintenance of a history and record keeping of measurement. Yet, continuity between context and measurement is also a way of maintaining continuity between the phenomena being measured and the representative tool by which it is rendered into a numerical form. The notion of 'continuous measurement' is perhaps a way to re-construct air pollution, understood as inherently fluid and as something which is difficult to capture through the production of discrete measurements. Here, the logic is, the more continuous the data the 'more real' it becomes, yet, in order to do this, a series of interferences are required to construct and fabricate this sense of 'the real'. The real, then, is constructed through maintaining continuous data capture.

[24] Black-boxing refers to the way in which scientific and technical work is made invisible by its own success. As Latour writes, '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, 1999: 304).

Stable data

I end my examination of the production of monitoring data by providing an overview of the journey of data following calibration because it sheds light on what a manual calibration test does, in terms of the measurement-data continuum maintained and sustained through the material organisation of monitoring practices. Indeed, the calibration test carried out by Phil was only one step of a larger, more elaborate process of validation. At this stage of the calibration test, these 'provisional data' are made public on the DEFRA website, yet these are ultimately unstable until they are measured for 'overall uncertainty' off site, which is determined by combining the 'type approval results' obtained from the relevant tests in the lab and in the field. These standardised procedures, by which monitored data become more stable through reducing and accounting for error, is simultaneously a movement of the measurement away from the site of production. The measurement becomes data, and as a result a series of further relations are enabled, such as the movement of data into research practices, or as information for public health advice. In this movement from measurement practices to the practices which work with and re-use these data, data become an objectified form, from which claims can be made and further epistemic inquiries made.

The in-between state of data-in-process is illustrative of one dimension of measurement practice, where flexibility and potential are paramount. What is of interest in the data, shapes the subsequent form data takes, and vice-versa. The forms data take change according to the interested party and those who use it. For example, the EU requires 'the mean daily level' of particular pollutants in their recording of the UK's air quality, but a scientist trying to understand and explain changing concentrations of a pollutant in a day may need to know the minimum and maximum measure of a particular pollutant. By providing discrete measurements every fifteen seconds, monitors enable further scientific relations and analytical patterns of air pollution to be made. For example, making continuous information publicly available means it can function potentially as a political tool for local communities. The way in which measurements are made allows these data to be re-used in flexible ways[25]. Indeed, one way in which monitored data were used was as comparative data for modelled data, the subject of case study two.

Case study 2: Running a simulation and checking for error

Modelling practices involve a different kind of measurement practice and measurement setting, yet there were also resonances with monitoring practices, in terms of the processes and transformation that practices make possible. Both practices attend to the construction and control of the setting from which a measurement can be made. For modelling, the measurement setting was built with a computer, so the complexities which make up a controlled environment, such as temperature, weather conditions, time, were also pre-conceived and constructed within the model structure. This contrasts with monitoring, where the complexities in taking a measurement influenced the setting in which a monitor was initially located.

The actual practice of doing a simulation run was talked about by researchers in WHAP because how data are produced became of interest to the epidemiologists who were intending to use it. Interestingly, however, monitoring measurements were not. This was because these were carried out outside of the project, collected according to standardised practices and because they were ‘real time measurement’ rather than ‘modelled outputs’. This distinction of modelled and monitored data will be one that I draw upon a lot in this chapter. Furthermore, in contrast to the monitoring station, where what types of pollutants are measured was fixed, in modelling, what pollutants were to be studied was also flexible. Indeed the process of deciding which interactions to study was the subject of continuing debate between the modellers and epidemiologists[26]. In the weekly liaison meetings the modelling

[25] By emancipating it from the messy realities of the measurement setting (air pollution in context), the data also becomes, what Antonia Walford calls ‘socialised’ (2013: 143). By socialisation I refer here to the effects data has on scientific relations, as something that carries meaningful information to other spaces of practice: as validated data for subsequent scientific work, as a way to ‘push policy’ by those advocating for the health of those adversely affected by air pollution and, closely linked, to be used as evidence in policy practice (some of the processes of which I will discuss in the subsequent practices).
[26] See Chapter Five

process and data sets the model produced were generally talked about by a senior modeller, Elizabeth. However, she did not carry out the simulation runs herself. During my stay with the atmospheric chemists in City B I shared an office with two of the junior modellers who were responsible for carrying out the simulation runs, called Craig and Tom. They also joined in on the weekly meetings with other researchers and were therefore aware of the epidemiologists' interest in the actual process of producing modelled data.

The modellers talked about the model as 'three dimensional' when discussing their data with other team members. This reference was often made in contrast to monitored data, which was described as two dimensional, and only representing air pollution in space and time. Rather, modelled data represented air pollution in space, time and as a volume. The model represents the 'fluxes', 'flows' and 'the transport and dispersion of' air pollution in a given spatio-temporal unit. So, for example, the spatio- temporal scale used by the modellers in WHAP was hourly and spatially defined by a hypothetical 5x5 km gridded space (and the UK is measured as multiple 5x5 km grid squares). However, a monitor makes a measurement at one point in time and space, and were therefore considered as representing air pollution within a 5x5 km grid square.

Craig told me that, as modellers, they are interested in the chemical processes and relationships between 'source-receptors'[27]. This was often talked about in terms of air quality rather than air pollution. The concept of air quality is inclusive of the relational nature of atmospheric processes, which has implications on the way in data should be produced. Terms such as, 'volume mixing ratio' or 'pollutant depositions' were used to describe the processes of air pollution in time and space. So, even though the model can simulate a number of different atmospheric pollutants, including NO, SO_4, O_3 and PM, these were considered as related rather than distinct in reality. The different pollutants were referred to by the modellers as *relations in*

[27] A source-receptor relationship describes the effect of placing a source of given strength in one region on the air quality at the receptor in a different region, therefore acknowledging the multiplicity of, and transformations air pollution relations undergo, in time and space.

the atmosphere, for example, as ‘Nitrogen and Sulphur deposition’ or ‘surface Ozone’. This differs from the monitor measurements, where the air pollutant is made pure by, quite literally, scrubbing the other parts of air away. Deposition is the effect of excess Nitrogen Oxides or Sulphur (the source of this pollutant is generally considered to be from the burning of fossil fuels and agricultural fertilisers), and is recognised as having negative environmental effects when an excess of the pollutant accumulates within the atmosphere. As an accumulated form, it is both a concentration and a flux, and exemplifies what the modellers refer to, when they claim that modelled data is a three dimensional form (rather than monitored data which is a two dimensional form).

The construction of a measurement setting was described by the modellers as the model’s ‘parameters’. A parametisation is made from observational measurements or theoretical knowledge, which get translated into mathematical equations to construct a model of atmospheric relations. Parametisations are then performed in a simulation run, in the case of WHAP through the writing of code in a computer language called FORTRAN. Adapting these parametisations is the practice I focus on here, in terms of the unstable moment of making a modelled measurement, and the ensuing interpretive process of working out data from error.

For the atmospheric chemistry modellers, the movement and fluctuation in concentrations of air pollutants is influenced by meteorology. The simulation model is a theoretical representation of the atmosphere and the theoretical assumptions which underpin the model are described through mathematical equations. The combined model simulates the atmosphere, then by reducing the atmosphere to a number of physical laws, which are converted into mathematical equations. The model also consists of measurement data as input variables, which function as parameters and boundaries required in order for the simulation of particular types of interactions in the atmosphere. These differential equations represent an ‘exact determination of how the system will evolve through time’ (Winsburg, 1999: 5), and the actual simulation process is internal to the computer model, where the equations which form the model calculate changes in the atmosphere, and therefore air pollution concentrations.

From what I observed, this process of running a simulation and adjusting the computer code seemed to make up a central component of the modelling work. This was described as a process of tuning by Craig, where his adjustments to the code make sure the simulation run on the model is more effective. This process of 'tuning' is a useful one because it exemplifies, and takes into account, the agency of both simulation model and modeller in interaction, contributing to the forward and backward process of checking the modelled output is 'sensible'. Although fundamentally different to the practice of calibrating an instrument, the process of working out data and error in a simulation run was a means by which the modelled concentration of air pollution turned into stable data. Tuning, as a mutual modification between the scientist making the measurement and the technological object used in practice, could also be understood as a process of 'attunement'[28], involving a mutual modifying between those making the measurement, the instrument used and achieving stable and clean data.

In the running of a computer simulation, the act of producing a number which stands for the amount of a given air pollutant in the air is referred to as an output before it becomes data. The simulation model in WHAP was a combined chemistry transport model (CM) and meteorological model (WM). This combined chemistry transport model (CM-WM)[29], was used by the atmospheric chemists to simulate the

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[28] Kathleen Stewart describes attunement as 'what attunes us to the sentience of a situation […] attunements can also affirm difference and be receptive to non-human qualities, rhythms, forces, relations and movements. The senses sharpen on the surfaces of things taking form' (2011: 452). See also Miele and Latimer (2014).

[29] The CM-WM is a pseudonym for the model used by the atmospheric chemists on WHAP. It is a model used for European scale atmospheric chemistry modelling at a scale of 50x50 km grid squares. For the WHAP project the model is altered to model the UK at a 5x5 km resolution. The resolution of the model is significant because it is the grid squares which define the spatial contours of the values made in a simulation. The grid squares can be visualised on a map of a geographic area, but in the construction of the model the grid squares appear in the form of computer code. As a three dimensional model, the space has to be constructed horizontally and vertically, to form a volume of the atmosphere. For the CM-MW model, horizontal means that the grid squares are influenced by a 5x5 km horizontal area. These different resolutions are 'nested' within the model domain, which means that in the construction of the model the 5x5 km grids are treated as inseparable components within the atmosphere. The vertical resolution refers to the distance between the

concentration and movement of air pollutants in the atmosphere, generating three hourly description (by mathematical equation) of the evolution of the dependent variables (the parameters and boundary values) of the model (Project Protocol).

A simulation run

> I am sharing an office space with Craig and Tom, and observing them running a simulation. I am surprised to find a simple and rather ordinary setting, an office very much like my own, considering the global remit of the CM-MW model. It seems to be time-consuming. I see that modelling takes place through an inter-connected network, relying on access to external expertise and technical resources. For example, the model interface of the PC is connected to a super computer [30](which communicates with a standard PC from an office computer desk), which Craig can communicate with. On the screen are lines of code, and below a box which Craig begins to type commands for the model into (Fieldnotes 8th November 2012).

A simulation has been described by Dowling (1999) as the performance of an experiment upon a theory, because simulation models are materialised mathematical models: The abstract model is manipulated through a digital machine, 'which inextricably entwines the epistemic and the technical together to form a simulation' (Ibid: 271). This combination of theory and experiment is often discussed within philosophical and social studies of science, with attention focused on the dual role simulation modelling enables: as a 'question-generating scientific object' (Sundberg, 2005: 156), and 'an exploratory tool' (Morgan and Morrison, 1999). Dowling emphasises the flexibility of computer simulations and the different roles they can carry out, according to the requirements of a particular narrative (1999: 263), which, again, emphasises the flexibility of the model in terms of the kinds of measurements that the model makes.

In WHAP, the role of the modelling was to produce data which carries information of the daily air pollution concentrations for the years 2000-2010. The Project

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Earth's surface, the ground, and the upward limits of the model, and the grid marks the limit of the information that is in the data (Edwards, 2010: 50).

[30] Super Computers are able to carry out a very high amount of computation, and are used for working with very large data sets. The super computer used by the modellers at The University was based in a research institute close by, but every year it moves between prestigious scientific institutions.

Protocol stated that these data were to be used by the epidemiologists in their statistical analysis of the relationship between air pollution and health. The computer simulation is an instrument of data production in the context of the wider project, much like monitoring, however, at the same time, for the modellers running the simulation was an experimental and theoretical practice. This meant that the checking of data for error was not only an issue for the users of that data (such as, the epidemiologists), but was also tied up with modellers' own notions of representational veracity and data quality.

The process of working out error in a simulation run was a key component of the practice of running a simulation, where there was an alteration between the computer screen as an interface and the mathematical model exemplified in the computer code. A simulation run is conducted through the model-simulation interface of the computer screen. The computer screen becomes the material way in which the researcher engages with air pollution as a digital abstraction. By typing out particular commands in the command box visualised on a computer screen, Craig manipulates the modelled atmosphere to produce a measurement of an air pollutant. This is done, Craig explains, by communicating with the model through computer code, which can be understood as a kind of language (FORTRAN). Knowing this language enabled him to communicate with the model, an engagement which makes the model do things:

> The core model code provided by the model developers is modified for the specific needs of the project [WHAP] by manually editing via the keyboard. This human readable 'source code' is then 'compiled' via a standard software tool (compiler) into a set of binary instructions which can be understood and executed by the computer (Craig, Personal correspondence 19th December 2013).

This is a process called 'compiling', where the line of code is translated by the model into a series of actions. It is this process that differentiates the model and the modeller, highlighting the agential role of each, because the model and the modeller understand the same commands in different forms[31]. This translation then performs a

[31] Here, Rheinberger's notion of 'epistemic object' (1997) is apt, because the model becomes both the representation of air pollution but also the means by which new discoveries about air pollution can be made. Both the model and the measurement

simulation according to the specific parameters written in the line of code: ‘to execute the sequence of instructions created by the compile stage’.

The compile stage refers to the work of the model because compiling, described by Craig as literally the material performance of the instructions in the code, refers to a process where the designated variables of interest are ordered by the model in a way that means the output files to represent the atmosphere according to the desired frequency (usually hourly or daily). The output files are structured and stored under the details of the simulation run, and Craig was then able to choose what kind of output data will be written out in the different output data file, so that what information the data will represent is shaped by the organisation of output files (see Figure 4). The arrangements of the output data into files results from a successful simulation run. However, the majority of runs involve error and Craig told me that error is just a part of the process of simulating.

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are in mutual emergence, and this process of switching between the interface and the output the model produces is not black-boxed at this stage, but engaged with in a process of getting a sense of both the model run and the output made.

rojects/emep/OpenSource/200802/User_Guide_Unified_EMEP_rv3.pdf

Output data files	Short description	Format
Base_day.nc	Gridded daily values of a selection of compounds.	netCDF
Base_hour.nc	Gridded hourly values of a selection of compounds.	netCDF
Base_inst.nc	Gridded instantaneous values of a selection of compounds.	netCDF
Base_month.nc	Gridded monthly values of a selection of compounds.	netCDF
Base_fullrun.nc	Gridded yearly values of a selection of compounds.	netCDF
sites.MMYY	Surface daily values of a selection of stations and compounds per month.	ASCII
Base.sites	All data from "sites.MMYY" files in one file.	ASCII
sondes.MMYY	Vertical daily values of a selection of stations and compounds per month.	ASCII
Base.sondes	All data from "sondes.MMYY" files in one file.	ASCII

Additional files	Short description	Format
Base_RunLog	Total emissions of different air pollutants per country	ASCII
eulmod.res	Mass budget of different compounds	ASCII
INPUT.PARA	List of input parameters	ASCII
Remove.sh	Removes the links to the input data at the end of the run	ASCII
Timing.out	Timing log file	ASCII

Figure 4: 'Organising the input and output data in a simulation run' (Table from CM-MW User-manual).

Achieving 'balance'

Error can be both a material and informational form. Indeed, Craig's demonstration of a simulation run showed me what error means in theory, but also how it appears and was worked out through the computer interface:

> Having pressed 'run' on the computer interface, we wait about ten seconds for a series of lines of code, similar visually to my untrained eye, as the code that was input into the model. This is because the model output is also in code. The model presents a result in the response box above the command box on the interface which Craig sits in front of (Fieldnotes 28th September 2012).

There is error if the code does not produce a legible output (although, even if it shows a non-erroneous output this doesn't necessarily mean it is data, error can still be there) and therefore error is materialised in the modelled output, appearing as a line of script within which error lies. Indeed, the line of script becomes the object of interest:

> The modeller states "see, there is an error" pointing to the computer screen where, after several seconds, a series of numbers appear. However, I can't see the error.

> Following this apparent visualisation of error the modeller describes how they then seek to understand this error, explaining that the compilation of code is tricky because if it is compiled on one computer then it won't necessarily work on another, so by re-running the model you start to work out where the error lies (Fieldnotes 28th September 2012).

'De-bugging' is an exercise, Craig explains, that seeks to understand the error in the result, which involves going back to the typed in code commands. There are a number of recognised sources of error. There may be error in the performance of the code in a compilation; there may be error in the assumptions within the code, for example, the approximated measurement by those who wrote the code not co-ordinating with the approximation of measurement being produced in the simulation; or, there may be 'human error' in the process of re-writing the code for the particular compilation for the simulation run. These different kinds of sources of error are found, understood and controlled for through a series of interface interactions. One such problem could be that the averaging of a concentration is done differently by the observational measurement than the modeller's averages in the code for the simulation run. A second common source of error is the emissions data, as measurements of the amount of different air pollutant emitted by particular sources. These are also subject to error and often drawn upon by the researchers in City B at the weekly liaison meeting, often as a disclaimer when discussing their output with the epidemiologists, who will go on to use their data, stating that 'their model is only as good as their emissions data'.

In the process of working out error, Craig clarified that he was trying to achieve and maintain a 'balance between things'. Balance is a component of the tuning practices of modelling, where error leads to adapting and manipulating the model to measure phenomena more accurately. Achieving balance and successfully tuning the code and the model in the simulation run is 'an art', which Craig elaborates on by describing the different 'things', including computer code used and commands made, the kind of information being sought, but also the very structure of the model itself. When the output showed error, the first thing Phil did was work out what kind of error it was and where it came from. In order to do this he needed to know how to use the model, and to have a good understanding of the theoretical laws which underpin the model.

Yet, there were also a number of practical concerns to keep in mind too, which one learns through the practice and experience of modelling:

> The more you do the faster it compilates, but then this is counter-balanced by the fact that length of time in queues depends on amount of processes you have... so you choose do you want huge amount of processes fast? But it takes time to get it to beginning of the queue, it is a value judgement (Craig, Fieldnotes 28th September 2012).

Craig explains that the practice of modelling involves value-judgments, involving decisions like whether one wants a quick output or a more complicated set of instructions. This illustrates the role of the modeller in the shaping of the kind of data made. 'The queue' Craig refers to in the quote above is a queue for using the super computer. Even though Craig used a desktop computer this was connected to a much more powerful and complex computer based off site (at a research centre close by but accessed and used by researchers from all over Europe), highlighting how local practices are connected to larger networks made up of technologies, institutions and people. The concept of 'balance' symbolises the inter-relationship of multiple, heterogeneous elements of doing modelling, which are carefully honed through practice and repetition and which perfect this 'art'. This is a combination of seeing and sensing (what kind of error, where that error may lie) as visualised in mapped concentrations (see Figure 5), in combination with the practicalities of making data (how quickly, how much or how complex?) and craft-skills of making the model work (modifying and adapting the code). In the map below, the error is ' obvious' because 'the SO_2 concentrations around the west coast of Ireland are too high compared with mainland UK due to reading an incorrect input file' (Craig, Personal Correspondence 27th February 2015).

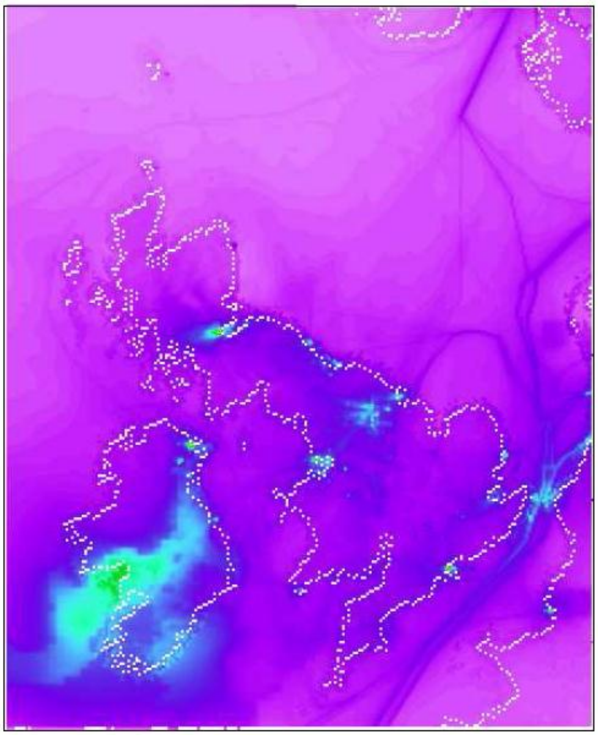

Figure 5: The visualisation of error: a map of SO_2 concentrations (Craig, Personal Correspondence 15th January 2015).

One of the central ways in which error was seen and understood by the modellers was through the plotting of graphs and making of mapped concentrations. The codes are read using open source software which helps analyse the data output by visualising data as mapped concentrations. Practices and vision are entwined because 'checking for error' and its related 'adjustment' following a failed run was part of the very act of running a simulation. Getting a balance was achieved through the practice of modelling and accounting for the different interacting components which need to be held together in order for a model run to work. Like Phil checking for error at the monitoring site, Craig emphasised the act of generating data, as a human-technical-object of research interaction, emphasising the looking and sensing aspect of modelling, which can only be successfully honed through the experience of producing numbers and results which perfect this 'art'. The concept of tuning also works well to re-describe Craig's portrayal of modelling as an art - the art of producing data which is both representative of the modelled atmosphere and air pollution in the real world.

It was important to understand the extent to which data is dependent on the modelling context, on the very production of data itself. In Craig's descriptions of the type of error that can occur in a simulation run, it was the balance of the model as a good representation of the atmosphere, the code as the means by which the computer and modeller can communicate and the assumptions behind the data used as inputs into the model that were considered in the production of modelled data.

> Errors can occur at all stages of the modelling process; some errors will cause the model simulation to fail to start or to terminate prematurely, but others may only be detected by careful analysis of the model output. Syntax (language) errors in the model code will prevent 'compilation' (translation of the human-readable code into binary instructions understood by the computer); logical errors in the code may allow compilation, but cause the model simulation to fail during running, for instance due to an attempt to divide by zero. However, even when the simulation apparently successfully completes, 'non-fatal' errors may have invalidated the results. These errors may manifest very clearly in the output chemical fields or may have more subtle effects only apparent to an experienced model user. The graph above shows an example of a relatively obvious error in the results where the SO_2 concentrations around the west coast of Ireland are too high compared with mainland UK due to reading an incorrect input file (Craig, Personal Correspondence 27th February 2015).

On asking Craig what the eventual output would look like, he presented a hypothetical comparison of modelled data and monitored data (Figure 6), explaining that all modelled data are compared with the observational, measurement data (from monitoring stations). In the graphs below, blue stands for modelled data and red for monitored data. This visualisation was another way in which the modellers compared the output of their simulation runs.

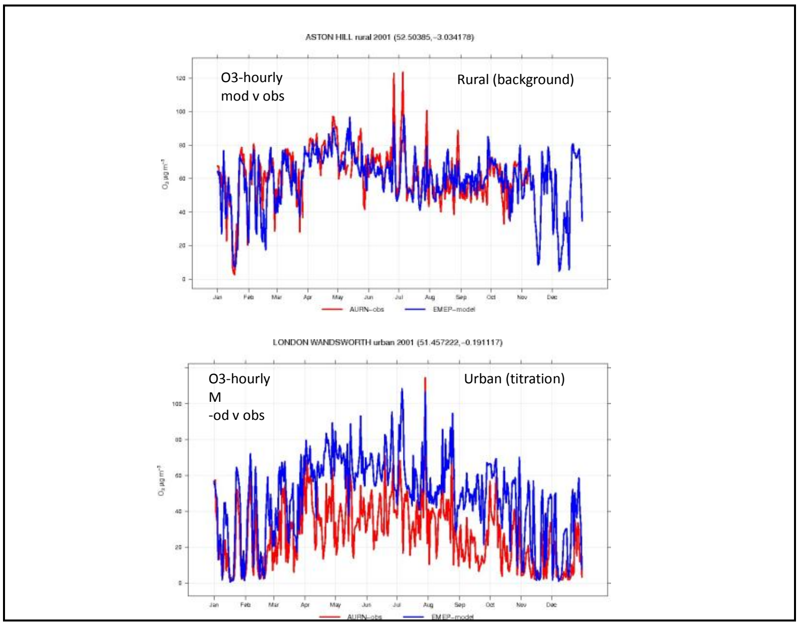

Figure 6: A comparative graph of modelled and monitored data (Craig, Personal Correspondence October 2012).

Like in the calibration test, this shift back to the technical arrangements of the measurement setting is where the ‘tinkering’ (Knorr-Cetina, 1981) and tuning were done. The subsequent interpretation of error involved the re-running of the simulation run by adjusting the code, primarily through the interface of ‘the compiler’. Craig described this as the most laborious part of modelling, an exercise referred to as ‘de-bugging’. During team liaison meetings’ ‘re-running the model’ was frequently referred to as the work that is currently taking place at City B. Indeed it often felt like the model was always running, constantly in motion, with researchers waiting to see what emerged.

Tuning

Tuning, then, relates to the theoretical and physical principles of the model, but also to the observational data which make up the model and a simulation run. For example, error can be caused by a problem with the numerical code and the parametisations upon which the model is built. For both Phil and Craig, gaining the ‘best estimate of air’ requires knowledge and experience of the technical equipment used in the process of producing data. From the careful balance of technical success, the agency of individual pollutants and to the interaction of both, in a modelled hypothetical space and time, required Craig to understand the technical equipment being used as well as theoretical knowledge of the chemistry of pollutants and the physical workings of the atmosphere. What renders these distinct practices similar was this act of *feeling for*, which both Craig and Phil elaborated on in their articulation of getting a sense of good data, which highlights the tacit and embodied craft of getting 'good data'.

The coupled nature of seeing and feeling was developed by Natasha Myers in her PhD thesis on the crafting of scientific knowledge, which demonstrated the physical and virtual nature of getting to grips with, and making sense of phenomena (Myers, 2007). She used these forms of tacit knowledge to foreground 'the body work' of protein crystallography, tracing the ways in which scientists confer this kind of knowledge through the training of new structural biologists. Although my topic is not pedagogy, the crafting of phenomena through the body work of scientific practice and the difficulty of recording and relating this knowledge via verbal and textual means is significant for the study of multi-disciplinary research in practice, as I will go on to show. I demonstrate that distinguishing a good measurement from error is part of a collective, standardised practice of making data. Yet, I also show that these measurement practices are unpredictable and precarious, where achieving *a sense* of data is an embodied act of making, influencing and interpreting. For Craig and Phil, stability, and thereby 'data', was achieved through carefully balancing the context of measurement, the phenomena under study and the scientists' role in effecting and affecting their ability to sense what counts as data or error.

Different ways of *doing* air pollution

> If, however, there is such a thing as ontology wrought by observation, this is where it is taking place […] This schooling of the senses is probably not qualitatively different from that undergone by the fledgling musician, cook, or weaver—as Aristotle noted, the paths to skill, on the one hand, and to understanding, on the other, pass through the same stations of perception, memory, and experience. But the scientific path is greatly straitened by the demands of collective empiricism, which require a degree of co-ordination seldom achieved (or desired) in the traditional arts and crafts. Just because it is an ontology, not only a standard of connoisseurship, that scientific training must impart, convergence is indispensable (Daston, 2008: 107).

Simulation modelling and monitoring data practices are situated ways of knowing, which take place within particular constellations of researchers, technicians, instruments and technologies. Indeed, my fieldwork shows that measurement is not unmediated or discrete, but is rather a process that unfolds temporally through interaction between the phenomena in question, technical objects and other scientific values. In this way, producing good data is a practice of increasing fabrication. The dichotomy of pollution versus purity was inverted in the act of measuring air pollution, where the measurement of air pollution becomes more pure through the calibration practices which clean data. One can apply the same metaphor of pollution and purity for modelling, where the practice of checking for error means the measurement of air pollution becomes more pure when a simulation run has been de-bugged. A pure measurement can only emerge with interference from outside the measurement, with the practices of checking for error, which paradoxically seek to rid the measurement from external contamination.

For Craig and Phil, 'feeling for error' was one part of both getting a sense for, and making sense of, air pollution as data, a practice that didn't simply represent but materialised air pollution into a tangible numerical, digital form. In this process of *feeling for* error, instruments were tuned to capture air pollutants 'realistically', but so were scientific bodies, and it was researchers' embodied experience and skill which also helped tune and align the multiple agencies mobilised in measurement practices. Checking a measurement for error was routine practice, yet it was an inherently uncertain and ambiguous process, and here I highlighted the ways in which a sense of data was achieved in modelling and monitoring. Although the checking for error was distinct for each kind of technological practice, in both, data's subsequent status as 'data' relied on these processes. This is because the making of a measurement of air pollution involved differentiating air that had got the pollutant of interest in and air that did not contain the pollutant of interest. This division had to be artificially created through a series of practices which constituted what Daston in the quote above describes as 'collective empiricism'.

At the same time, modelling and monitoring practices were not simply different kinds of scientific work, but different material engagements with air pollution. As

Evelyn Fox-Keller describes, scientific models are cognitive representations of something else, but they are also embodiments of action and practice that constitute the kind of scientific questions asked and how these can be answered (2002). The work of what was understood as capturing and rendering visible air pollution (in a tube or in a simulated atmosphere) as a tangible, material form involves embodied craft-work, but also the application of standards of measurement and practice in their articulation as data. In other words, sensing air pollution through data practices involves tacit knowledge, craft skill and technical expertise, where seeing and knowing is local and particular, whilst simultaneously connected to a chain of further relations and articulations.

I have shown how air pollution was made visible as different kinds of data. In making a measurement of air pollution, air pollution was enacted in different ways in order to achieve different ends. Through the production of monitoring data, the storing and collecting of these large amounts of data enact a history of air pollution, a materialised, tangible and continuous (well, at least a dis-jointed kind of linearity), movement of air over time. These data were spatially stamped, characterising local places and air pollution hot spots. Getting a sense of monitored data was also an articulation of a particular kind of sense-making, framed by a logic of knowing and acting, where data is made instantly accessible and re-usable for publics, governments and community activists. I call this air pollution, *air pollution-on-the-ground*.

In monitoring, the craftwork which made this distinction between data and error visible involved scrubbing away the wrong relations in the air so that the right relations could be measured. In order to do this, Phil drew upon the knowledge of the monitor in action, the construction of the environmental context of the phenomenon in question, and the heterogeneity of the pollutant being measured. Rather than taking away other parts of air, modelling adds relations to air pollution through the building and running of a 'simulation' of the atmosphere, so that air pollution is measured as part of other physical and chemical interactions. Air pollution becomes a non-local phenomenon here, rather than a situated object of research, rendered visible as part of 'the atmosphere'. Producing modelled data was framed by a logic of knowing not just air pollution, but air pollution in the atmosphere, in flux and process, and therefore one which articulates with a scale of global governance and regulation. I call this air pollution, *air pollution-in-the-atmosphere*.

The pros and cons of modelled and monitored data

That there were multiple ways of measuring air pollution was of significance to the WHAP scientists because the team have to investigate these different air pollutions. For, as I have shown, the different practices which produce data materialise air pollution in contingent ways. The construction of research spaces enact air pollution as having particular spatial characteristics. These different ways of spatially performing air pollution through different measurement practices became problematic for the epidemiologists who had to use this data in their own data practices.

In recognising that there were different ways of measuring air pollution researchers drew upon particular values of what 'being scientific' means. Accordingly, 'which representation is best?' became a relative question. For the modellers, that modelled and monitored data are different is not a problem. Each work in symbiosis with the other, and the output of simulation runs are always compared with monitored data at a similar spatial and temporal scale. Yet, they were considered as also representing different kinds of air pollution, which could be summarised according to the table in Figure 7. For the epidemiologists, however, this multiplicity was rather problematic.

Modelled data	Monitored data
Represent a 5x5 km grid square area.	Represent a single point in space.
Each measurement is connected to other atmospheric relations.	Each measurement is discrete.
Data is checked for error and validated by the model producers and through the training of modellers to use the CM-WM model	Data is checked for error and validated according to UK government accredited, standardised procedures.
Measurements are made by re-producing the atmospheric conditions within which air pollution is produced.	Measurements are made by taking a sample of air and measuring the amount of air pollutant within that sample.
Made using a computer model.	Made using a monitoring instrument.
To use the data you have to take into account the other relations which influence air pollution.	To use data you have to extrapolate outwards, in order to capture air pollution's movement in space.
Simulate reality	Are made in 'real time'.
Can be used to produce data on air pollution in the future by running 'future simulation'.	Can only be used in present time, but a historical record is kept so that air pollution through time can be studied.

Figure 7: My summary of the pros and cons of modelled and monitored data.

The modelled and monitored data tension

> As epis[32] what we trust is when we see measurements, because we see it and we know how it works and that is a version of reality, but you might say it doesn't represent all these different things, the epidemiologists don't trust models, but the modellers, you say you don't trust the single point measurements (Tim, Liaison meeting 18th May 2012).

In the quote above, Tim, an epidemiologist and the PI of the project, highlights both the differences that researchers on WHAP hold about what counts as 'good data', and an attentiveness to the sort of negotiations involved in the carrying out of collaborative, multi-disciplinary research. I've shown that data practices are the sites at which difference and multiplicity were (re)produced, both in terms of data's meaning and its material form. I am now going to explore the emergence of what became known within the team as 'the modelled and monitored data tension'[33] (M&M tension).

[32] The term 'epis' was the shorthand name used to refer to the epidemiologists on WHAP.

[33] The modelled and monitored data tension was often also called a debate, and indeed I began by referring to it as modelled *versus* monitored data. On sharing my

In STS and anthropology, it is now a well-rehearsed argument that no object or phenomenon is singular, and that material practices enact different versions of these objects, bringing them into being in multiple ways (Mol, 2002; De Laet and Mol, 2000; Law and Mol, 2002; Harvey et al., 2014; Jensen, 2004b). Accordingly, how to describe and manage the relations between different knowledge practices, requires paying attention to the local collaborative dynamics established between different methods and forms of practice (Moreira, 2006). Attending to the relations between data, and its resulting multiplicity, is a point of departure both for myself and for WHAP researchers, who, in finding that data practices produce and enact different kinds of research objects, instigate a series of attempts to co-ordinate and make commensurate these different data.

Philosopher of science Sabina Leonelli (2008; 2009; 2010) has shown that the material attributes of data can be used, or ‘re-packaged’, by scientists to achieve novel results. The movement of data from one scientific setting to a new scientific setting, she argues, is a process of translation, which transforms data’s boundaries, properties and meaning. In doing so, she illustrates how data are material things, embedded within complex set of heterogeneous relations. By following data in their movement between different research practices, specifically data-driven biology and the biomedical sciences, Leonelli argues that data need to have ‘baggage’ in order to travel successfully (2010: 9). By baggage, Leonelli refers not only to the material ways in which data are formatted and aesthetically presented, but also to the disciplinary based epistemological values which underpin local data practices.

The tensions which ensue when data move between different research practices has also been termed ‘data friction’ (Edwards et al., 2011). Edwards et al. argue that paying attention to data as process is of increasing significance, because of increased amounts of data and the movement these large data sets between scientists across the

research on this topic, however, some researchers resisted the idea that it was a debate, but was, instead, more like an on-going dialogue. I decided to call it a tension so as not to suggest a binary juxtaposition, whilst pointing to a difference which requires a solution, and at the same time allowing room for possible commensurability.

globe. Highlighting the informal, ad hoc, incomplete and contested processes of communicating about data, the authors suggest that these spaces of practice are imperative for the sociology of science agenda.

The sharing of data is the starting point of the M&M tension, because it was only because the epidemiologists wanted to re-use data of air pollution, produced in different research spaces, for their epidemiological analysis, that problems arose. The tension between these two types of data emerged in the early stages of the project and its apparent intractability struck me (Field note memo, April 2012). It also remained an interest and concern for team members (October 2011- June 2013), where at times it was side-lined and, at other times, took centre stage. As a result, a series of interesting, and often tentative ideas, suggestions and actions were proposed and materialised by team researchers, in an attempt to use data across these different epistemic practices. In this way, I examine just how data sharing takes place on a particular multi-disciplinary project.

The first reference to the M&M tension took place during a weekly liaison meeting, a couple of months into the project and my field work commencing. Peter, an epidemiologist and statistician, and one of the senior members of the team, had recently returned from an international conference on atmospheric chemistry and health where, he claimed, he found that 'people were very anti modelled data' (Peter, Liaison meeting 7th November 2011). During the liaison meeting Peter shares with the team his conference Power-Point presentation, in which he introduces the different work packages which form WHAP, but stating that the main thing he took away from the conference was their criticism of the epidemiologists' proposed re-use of modelled data in the epidemiology work package. Peter claimed that participants at the conference were 'suspicious of using modelled data for epidemiological analysis of air pollution', which had led him to re-consider what they, as epidemiologists, will do with the data from the models on WHAP.

These members of the conference audience were also later referred to as 'senior academics', whose challenging of the proposed use of modelled data for the epidemiological analysis had a number of consequences. Peter went on to explain to the team that it is important to consider these queries, because if 'the epidemiological world' do not accept modelled data then this could lead to problems later on, in terms of their academic reputations, their ability to publish research findings and ability to access future funding. However, I soon realised that it was not simply an issue outside of the project, but one very much embedded in the disciplinary practices differentiating the modellers from the epidemiologists.

The deep-seated epistemological, and indeed ontological, nature of these issues required some kind of local-level, practical solution. Implicit in the 'other senior academics' suspicion of modelled data is that they are not 'empirical' and also, like Tim's explanation in the opening quote in this section, is a distinction which conflates modelled data with 'untrustworthy' data. Trust is difficult to unpack here, but alongside these 'other academics' concerns around the use of modelled data for epidemiology, there seemed to be something inherently untrustworthy about modelled data in terms of the making credible epidemiological claims.

The intention was, initially, for the epidemiologists to use the data produced by the modellers with health data in the statistical analysis of correlations between particular pollutants and associated health effects (Project Protocol). However, the problematisation of modelled data meant that the intended re-use of modelled data came under scrutiny by the epidemiologists on WHAP, and in the process wider epidemiological concerns around conducting 'empirical research' were brought to the fore.

During the subsequent weekly liaison meetings the epidemiologists presented a united front, sharing Peter's suspicion of modelled data and advocating for what they call 'some real data' on pollutant concentrations (Liaison meeting 13th November 2011). As a result, modelled data was juxtaposed against 'real measurements' (from the monitors). However, at this stage, the epidemiologists avoided focusing on epistemological notions of 'truth' and 'reality', suggesting instead to think about what *we should do* as a practical solution to the problem of modelled data. Indeed, 'the problem' was situated 'outside' of the project, with Peter distancing himself from the 'epidemiological community' who were used instead as an external reference point for the making of a ‘practical compromise’ with the modellers on WHAP.

The problem with modelled data was further elaborated on by Peter and Tim in a later liaison meeting (Liaison meeting 21st November 2011), framed as being about ‘error’ in modelled *and* monitored data. That there is no ‘true measure’ of air pollution is problematic for the epidemiologists because it reduces the power of the epidemiological claims they make using air pollution data with health data. ‘The truth’ was recognised as something that cannot be accessed, yet the measurements produced by monitors come closer to the truth for the epidemiologists. Again, in the opening quote, Tim explains that measurements are only ‘a version of reality’, but they are still a version which can be understood. ‘How it works’ relates directly to the data practices which account for error in epidemiology, and how much data errs from the truth has to be statistically accounted to make credible statistical claims. It is less easy here, because in modelled data the epidemiologists ‘do not know how it works’, which makes it a problematic unknown unknown (rather than a known unknown, like error in monitored data).

As such, the epidemiologists suggested to the modellers that they could use both modelled and monitored data together, as a way to reduce the error in both these data:

> Error distributions of the model and monitor-based estimates should be estimable roughly [...] it should be possible to get a better estimate by assimilating the information from the monitors with that from the model (Peter, Liaison meeting, 21st November 2011).

A shift here in ways of talking and thinking about data can be seen here, from data as a form which carries informational potential, to one that can function as an investigative tool for studying error in both types of data.

The problem with data assimilation

In a team wide email, Peter attached some preliminary notes on a possible solution to 'improve modelled pollution estimates' for epidemiology, suggesting a process called 'data assimilation':

> I propose a process of post-modelling data assimilation which can be approached in several different ways, but the main idea is to use both modelled and monitored data together to produce more valid results [...] it is an idea which needs working on and the actual practicalities of how to carry this out should be explored by the team (Fieldnotes, Liaison meeting 14th November 2011).

The epidemiologists explain to the modellers that they would like to produce something out of modelled and monitored data (Fieldnotes, Liaison meeting 21st November 2011). Peter clarified further that, as an alternative to using only modelled data in the epidemiological analysis, data assimilation could be a practical means to using modelled data and monitored data together.

Here, the concept of data assimilation is taken up by the epidemiologists as a practice relevant to their own re-use of modelled and monitored data. The notion of combining two different types of data implies that data is considered 'raw' (Gitelman, 2013) and pre-theoretical by the epidemiologists. For the epidemiologists, then, data assimilation is a possible practical solution to a theoretical problem, contingent on a particular conceptualisation of 'good scientific method'. This is because each set of data is understood as carrying different kinds of information rather than representing different things. The assimilation process would, they claim, enable 'real measurements' to be used as well as the data produced by models. By adding modelled and monitored data together 'more valid' data would be possible:

> [W]ith two independent approximate estimates (model- and monitor- based) it should be possible to get a better estimate by assimilating the information from the monitors with that from the model [...] and that the epidemiological world seems sceptical of using modelled data on its own (at least for time series analyses) adds motivation to consider this (Team wide email, 13th November 2011).

In terms of being a process that 'needs working on', Peter referred to the literature as 'other studies' which the epidemiology group on WHAP have considered in terms of how to go about the process of data assimilation. As a result, Peter summed up the process rather vaguely, as ‘the basic idea of data assimilation is that two sorts of data are combined’, in order to produce more ‘valid data’ of air pollution and how to actually carry this out in practice remained elusive. Indeed, it required the modellers to contribute to the process of ‘combining’ modelled and monitored data.

Yet, for the modellers, data assimilation is a technical practice, which is arduous and time-consuming:

> Data assimilation requires a joint version [of the model], where a change in concentration requires the model to go forwards and backward in time, to go back to changing emission in a similar fashion… it would take a year to have a computer version (Tom, Liaison meeting 14th December 2011).

Furthermore, for the modellers, the actual process of re-using modelled data in combination with monitored data was also problematic because modelled and monitored data carry their own boundaries, properties and meanings. For the modellers, data assimilation evokes a rather different history and socio-technical network, and their CM-WM model would have to be re-built so that it can run forwards and backwards in time. Secondly, the modellers would have to change the emissions data which is input into the model to enable it to run a simulation. Both these actions require the involvement of other researchers, other data sets and indeed the transformation of the model itself (rather than simply the use of it). Data assimilation for the modellers was not simply ‘post-hoc’, or an additional process of data analysis and development, but a re-structuring of the very process of making data.

The spatial representation of air pollution

The starting point for tackling the idea of data assimilation took place in a liaison meeting a few weeks following Peter’s initial problematisation (Fieldnotes 14th November 2011), during which the modellers responded to the epidemiologists’ challenge to modelled data’s suitability for epidemiological analysis. The modellers claimed that data assimilation would require the modellers to re-write the whole model, thereby ‘upsetting’ the simulated atmospheric relations and the scales and correlations assembled in a model run (Field note diary - liaison meeting 21st

November 2011). Elizabeth explained that, if you add monitored data to modelled data then the combined data output becomes disconnected from the modelling context within which the modelled data was produced (and as I have shown modelled data is comprised of heterogeneous atmospheric relations). Using the example of Ozone (O_3), she explains that adding data together meant ‘it was not that temperature produced that Ozone’, which is crucial because atmospheric conditions are considered as constitutive of air pollution.

Elizabeth also further elaborated on these different kinds of air pollution research objects which data practices embody, arguing that the balance between air pollutants, emissions sources and weather data achieved in modelling are lost through the idea of ‘post-modelling assimilation’ (data are not raw or pre-theoretical). If these elements are not captured properly, according to the theoretical structure of the model, then the subsequent model output will not accurately simulate the atmosphere. Elizabeth’s concern relates to contingent notions of validity, which contrast with epidemiological notions of validity. As Oresekes et al. explicate, models can only be evaluated in relative terms because they represent environmental systems (1994), which means the practice of modelling is always in process and open, making validation through comparison with point measurements only ever partial. It is this point that Elizabeth refers to, in the sense that the modelled data is measuring a different kind of air pollution which can’t be combined with the monitored data.

In a conversation which took place whilst visiting the modellers in City B, and following my sitting in on a department seminar where an external speaker (an atmospheric chemistry modeller) leading a large atmospheric chemistry project funded by the EU gave a lunchtime presentation, Tom, a modeller, compared WHAP with these kinds of ‘large EU projects’, with ‘big ideas’ and ‘big sets of big scientists’. He explains that the project which was focused on during the presentation is interested in increasing the complexity of models, but that this was not the emphasis on WHAP:

> [The] epis are not so much interested in the science of it all but more so in making sure the grid square matches the measurements. This means that even if the model is a load of rubbish or not working properly, it doesn't matter…However, this causes problems in the future because if you want to use future scenarios, like we are in WHAP, then if the model has got problems then it won't work[...] (Tom, Fieldnotes 9th September 2012).

Here, Tom makes two important points. Firstly, that assimilating, and therefore the 'matching' of modelled and monitored data is not just, as he suggests the epidemiologists assume, a sure way to check the validity of the model, because correspondence may belie incoherence and inconsistencies within the model. Secondly, and related, the data produced by the model relates to the model itself, and cannot simply be detached from this relationship by integrating it with monitored data.

Tom helped build the version of the CM-WM model used in WHAP at a local but globally renowned research institute. During my visit to City B, he described the process of modelling in a bit more detail, in order to help me understand why combining the model output with monitored data is not possible:

> The model is 3D, a portion of volume not just surface [as measured by the monitoring stations, which measure ground level concentrations of air pollution][...each 3D volume box] calculates which pollutant emits it and the interaction, the secondary product, the chemical transport such as wind, and how much gets removed from the atmosphere [through the transport of pollutants]...the model calculates this at every step… the monitored data may be the truth and accurate, but it doesn't reflect the 5x5 km grid (Tom, Fieldnotes 13th December 2011).

In terms of space, the model simulates the 5x5 km grid square as an interactive and dynamic space. In contrast, monitors do not capture the interactions 'in the atmosphere' or in three dimensional terms, but rather collect measurements of air pollution 'on the ground' (two dimensional). For the atmospheric chemists, monitored data come into conflict with what modelled data stand for, both materially and conceptually: what the model offers as data doesn't relate to 'a representative measurement' of air pollution by the monitor, because representations are internal to the simulation process, in the sense that 'space' is reconstructed in the model. Thus, data assimilation as a solution to problem of modelled data was in fact the means by which the tension was re-instated, because, as Elizabeth also explains nicely, in the practice of assimilating, data start losing their correlation with one another:

> So, say hot days produce Ozone, if you start scaling your temperatures then it was not that temperature that produced that Ozone. That is my main worry that you lose the power [of the model] (Elizabeth, Interview 13th November 2011).

Scaling is a tangible process in modelling, where 'space' is materialised through a model run. The run which produces data is contingent on the construction of space in the simulation, and 'adding monitored data' to the modelled data disrupts the relationship of data with the modelled space.

So, for the epidemiologists, the idea of assimilating data is a way for them to counter the 'mis-match' between modelled and monitored data, because each data is understood as producing a gap between the measurement as a representation and the phenomena that it is representing. Yet, for the modellers the data produced by the model, as a kind of representation of the world, is a very different kind of representation to that made by monitors. The model re-creates the atmosphere and air pollution measurements are produced within this system, rather than actively collected, distinguished and taken away from the context of measuring, like in monitoring data practices.

Summary

In this chapter I have described two different ways of measuring air pollution and the different kinds of representation of air pollution they produce. I detail the practices and processes involved in achieving a stable measurement, highlighting the external references which are brought into a local measurement setting in order to do this. I then consider these different data and why they are different, suggesting that modelling and monitoring practices produces different kinds of research objects. The different ways of making data on air pollution was not only an empirical finding for myself, but a practical problem for the WHAP researchers because their object of research was not one but multiple.

The subsequent M&M tension revolved around the epidemiologists intended re-use of modelled data in their epidemiological analysis. However, in considering how to re-use modelled data, the modellers and the epidemiologists had to make explicit what data stood for, which brought to the fore the problem of sharing data between different socio-material practices. In the following chapter I will trace two ways in which the researchers on WHAP engage and intervene with the problem of 'what data stand for?' Accordingly, I attend to the local level practices which engage with multiplicity and difference as part of the practical, everyday constraints which doing multi-disciplinary research entails.

Chapter 5 Data-as-intervention: the modelled and monitored data tension in practice

Introduction: materially intervening with difference

> There was certain scepticism in the epi community of the use of modelled data and the question I think we are asking is […] clearly the modelled values are not going to replicate measured concentrations they are different from measurements, and the question is how big is 'slightly' [how different modelled data are from measurements] and what impact might that have on the estimation of health effect (PI, Liaison meeting18th May 2012).

In Chapter Four I described two of the multiple ways in which air pollution *gets done* (Mol and Law, 2004) as the result of data practices. The multiplicity of air pollution performed through different data practices was problematic for some researchers on WHAP because, in theory, they consider their research as contributing to knowledge on one air pollution. Indeed, the rationale that underpins the multi-disciplinary structure of WHAP was that, by adding different data together a more truthful rendering of air pollution would become possible. In this chapter, I examine the ways in which the WHAP team negotiated modelled and monitored data as a form of 'collaborative practice'. I trace a series of different material interventions initiated by researchers in their attempts to find a way for these different data, and their related configurations of air pollution, to 'hold together'. I call this process data-in-negotiation, where what data represent and what data can do became a key concern.

Starting off with the finding that multiplicity was a problem for researchers, I now focus on the productive relations that multiplicity enabled. As the opening quote illustrates, considering modelled data in light of epidemiological data practices requires determining both the gap produced in measurement practices, and the differences between the measurement made and that to which it refers, but also the new gaps which arise when measurements are re-used in the making of new kinds of measurements, such as that of the health effects of air pollution. In order to shed light on these differences between data and the air pollution they represent, new data were produced. These data ‘interventions’ were not used to produce knowledge on air pollution, but rather to measure the gap between data and air pollution - what was often referred to as ‘error’.

The first intervention I discuss is ‘the simulation study’, a statistical practice which compares the error in modelled data and monitored data of air pollution at the scale of the 5x5 km grid square. The aim of this was to work out which data are the best representation of air pollution. This was carried out by the epidemiology group. The second intervention was the introduction of DIY data. These data of air pollution were collected via small, hand-held devices, which produced measurements of air pollution at a finer scale than the 5x5 grid square. Both these additional data intervened in the M&M tension by producing data on the ability of modelled and monitored data to represent the grid square, by producing data to compare with, and consider the spatial veracity of, modelled and monitored data.

In tracing these new data and how they intervene in the M&M data tension, and in drawing upon insights within social and cultural studies of science which emphasise the material and performative dimensions of scientific practice (Pickering, 2008; Barad, 2003; Mol, 2002; Haraway, 1991), I pay attention to the representative dimensions of practice, but also the socio-material relations which multiple data practices in combination enact. The M&M tension began with the question: what data best represents air pollution? However, the subsequent material interventions through which negotiations took place meant that these practices also enacted data as air pollution in new and different ways.

The first material intervention: ‘The simulation study’

> Starting off with a concept piece on the issue of modelled and monitored data would be useful, largely to get our [the epidemiologists] own thoughts clear and to think about what we are defending against. I have written down a few things that have occurred to me, and Peter has helped me do this, on the kind of theoretical basis, from an epi point of view, as to what variations [air pollution] we are interested in, and I think Naomi has been away but when we last met, the four of us, she had already done some simulation work and was also going to have a look at what has been published in this area, and also the use of hybrid models (Tim, Liaison meeting 2nd April 2012).

The simulation study was a statistical exercise in which modelled and monitored data can be examined according to statistical and epidemiological conceptualisations of measurement error. The notion of assimilating modelled and monitored data together is based on the epidemiologists' assertion that they need to use 'real measurements' as input data for their epidemiological analyses. The modelled and monitored data tension revolves around the spatial representation of air pollution by different data practices. For the epidemiologists, the tension also relates to values around 'being scientific'. Since the idea of data assimilation was rejected by the modellers[34], Tim suggests a 'conceptual approach', as a way of working out which data to use in light of their own epidemiological data practices. This conceptual shift orientated discussions towards a new kind of data practice called 'the simulation study'. As the quote above illustrates, this move also involved new ways of describing the combining of data, as one which is hybrid, and therefore a mixture of data, rather than one of assimilation, which implies addition.

The epidemiologists explained to other team members that they are interested in showing why they are using modelled data rather than measurements (monitored data):

> One reason, they suggest, is that 'we only have measurements in a limited number of grid squares', but we need to do some simulations 'to show that modelled data is better than a limited number of measurements in a limited number of grid square'. The problem is, they go on to consider, that the modelled and monitored data 'aren't measuring quite the same thing', 'we are not going to have the gold standard' and 'we are not comparing like for like' and it is a struggle to try and address this (Fieldnotes, Liaison meeting 2nd April 2012).

The simulation study is one of the ways in which the epidemiologists try to address this problem of not having a gold standard. The simulation study concerns epidemiological notions of 'good data', and provides information on error, because, as I have highlighted, error relates to the power of the epidemiological claims made using this data. Epidemiological concepts of error are different from the instrument and technical error that I discussed in Chapter Four. There are also standardised ways of conceptualising error in epidemiology, which result from a practice of

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[34] See Chapter Four

feeling for error through statistical techniques. A series of terms are used to describe different types of error statistically, which inform how data is interpreted and re-used in epidemiological research more generally. As Peter, an experienced statistician, explains:

> Error causes loss in study power, the 'efficiency', 'correlation' between 'truth' and 'proxy'. 'Bias', depends on error type [and the] wrong confidence interval. If you can imagine it as an equation [x + y= xy] we are feeling for error on this side [the right side] of the equation (Peter, Team meeting 6th December 2012).

In the simulation study, the different data are re-represented in a statistical framework called 'time series regression', which is a temporal and spatial statistical space for the analytical comparison of modelled and monitored data. An extract from my fieldnotes observing a statistician going through the process of the simulation study with me is illustrative of the way in which data are statistically compared in practice:

> Using pencil and paper she begins by sketching out and explaining how she sorts out databases of real modelled and monitored data on an Excel spreadsheet. I asked where these data come from. The study uses NO_2 data from the City B group and Ozone from Peter's previous epidemiological study and also probably City B's data. There are only about ninety monitoring sites where there are monitoring stations so that you can compare data with modelled data [modelled data is produced for every 5x5km grid square and the comparison is with monitors within these grid squares]. Then in Excel the comparison is carried out to produce 3-4 time series of modelled and monitored data compared (Fieldnotes 12th September 2012).

There are not monitors in every 5x5 km grid square of the UK, so the statistician carrying out the study interpolates multiple monitored data to work out an average for every grid square to be used as the 'monitored data' for comparison with the modelled data in each grid square. The process of interpolation is one way in which error is produced, understood by the epidemiologists as reducing the capacity of data to represent the 5x5 km grid square. This is a spatial form of measurement error because it concerns the representation of a space within which people breathe air, and a space which is likely to be made up of fluctuating levels of air pollution.

By arranging the modelled and monitored data in Excel they can then be compared spatially, within the same constructed 5x5 km grid square. The simulation process builds on certain pre-conceptions about error, and as one statistician describes, 'you feel for error and simulate'. I am told by the epidemiologists that they are only simulating O_3 and NO_2 at the moment, because they have a feeling that these have very different behaviours (Fieldnotes 12th September 2012).

Feel is used in two ways here. First, as the sensing part of understanding and conceptualising error in data, described as a tangible, sensing practice rather than something as purely cognitive. Second, *feel* is used in an analytical and more abstract sense, as something which comes with experience and knowledge of how pollutants perform in statistical analyses. The statistical work is explained by the epidemiologists in terms of 'known' characteristics of the chemical relationship between O_3 and NO_2, which are widely recognised as inextricably linked (Clappa and Jenkin, 2001). This established relationship is one of the ways in which the simulation study is used to test the modelled and monitored data for error, because they can use them to test how well the different data represent the known characteristics of these pollutants.

As illustrated in Chapter Four, feeling for error is a practice which draws on embodied skills and experience of doing modelling or monitoring in the process of making data. Error in statistical practices is no less material, and testing for error involves both computer software and playing with epidemiological models to get a sense of the data in terms of statistical error. Despite Peter's attempts to explain verbally, he ended up showing me what feeling error was like by jotting down a simple equation, $x+y=xy$, adding that, 'you are probably not that familiar with numerical ways of working, but the best way of understanding this is that we are feeling for everything on this left side of this equation' (Fieldnotes 5th March 2013). The process of the simulation study made statistical analysis of data possible, thereby providing a material space within which feeling and meaning are undertaken.

The classification of error

The simulation study produced data on the kinds of error in the modelled and monitored data. The epidemiologists used two different terms to describe statistical error in modelled and monitored data: ‘classical error’ and ‘Berkson error’. The error in the monitored data (‘classical error’ in Figure 8) was described as ‘precise’ because it related to ‘area error’ and ‘instrument error’. The modelled data error (‘Berkson error’ in Figure 8) was described as ‘not precise’ which meant the error was larger due to this additional level of error complexity. Berkson error was not only about the spatial siting of the instrument, like monitors, but about the error within the model itself - in terms of the internal workings of the model as an abstract mathematical system. This can lead to an over-estimation of the health effects of air pollution in the epidemiological model. It is also the kind of error that can’t be accounted for easily because it is an unknown unknown. Classical error, however, means that error lies in the spatial representation of air pollution rather than the theoretical representation of air pollution, which can be measured and taken away by the epidemiological model through the introduction of relatively simple equations (as a known unknown).

What was significant for the epidemiologists was how error influences the meaning of the data produced. As the slide below depicts, different types of error relate to different formulae with particular consequences. In the equations on the slide in Figure 8 ‘the proxy’ represents the modelled or measured data, and ‘the truth’ is hypothetical. Here, ‘the truth’, is understood as constructed, produced by the simulations by taking away the error in modelled and monitored data in order to provide a workable ‘gold standard’. The equations only work if a ‘truth’ can be used, where E_b and E_c stands for error types - ‘Berkson’ or ‘Classical’ (see Figure 8). Through these equations the different types of error can be explained and understood by the epidemiologists. For example: ‘classical error is error dependent on truth, so a high proxy suggests high error’ (Peter, Team meeting 6th December 2012). Indeed, during a presentation of the simulation study results to other team members, Peter explained that, ‘with classical error it seems simpler as you can estimate what you need. There are reliability studies on classical error' (Peter, Team meeting 6th December 2012).

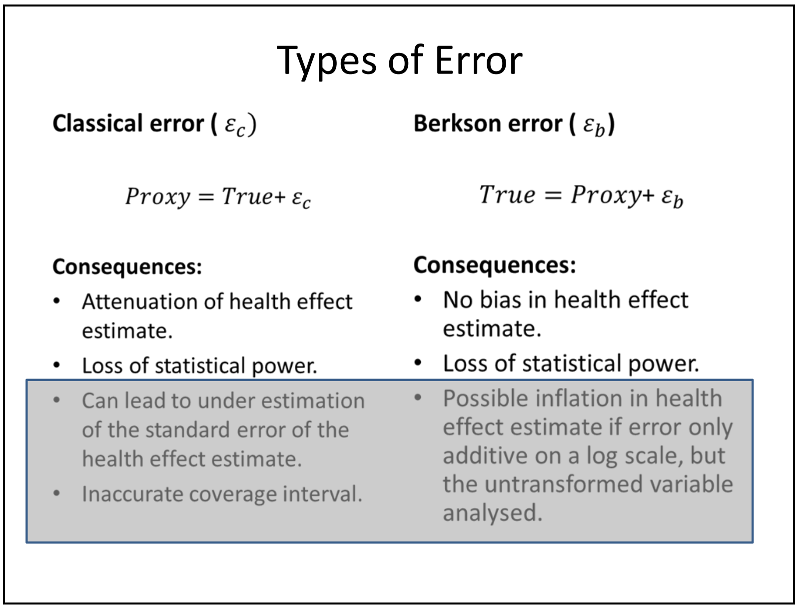


Figure 8: 'The classification of error' (Presentation slide by the epidemiologists at December 2012 WHAP project meeting).

The equations in Figure 8 not only represent error but, in the epidemiological model, are the means by which the epidemiologists come to anticipate and evidence error. As Peter tried to simplify for researchers during the collaborators meeting in December 2012, 'the best way to understand this is that all the things on the right [of the equation] are fixed' (Peter, Team meeting 2012), demonstrating the way in which, through a statistical equation, error becomes a part of the data production process for the epidemiologists. Like Phil and Craig's checking for and taking away of error in the making of a measurement[35], the epidemiologists also worked out ways in which they can measure and therefore take away error in order to make epidemiological data. These statistical practices shifted the focus from a problem of representation to the means by which a statistical solution is generated. Error is re-represented in Excel, only to become a particular component of a formulae through which it can subsequently be statistically removed.

[35] See Chapter Four

The simulation study data produced in this exercise of testing for measurement error according to epidemiological conceptualisations of error, proved to be productive for the epidemiologists. By re-producing modelled and monitored data in the simulation study the two types of data were brought under statistical control, a modification which made data further comparable and malleable for statistical work. For example, at the multi-disciplinary table the epidemiologists talked about 'the performance of instruments' in terms of error in modelled and monitored data from their simulation study. I suggest that the simulation study not only classified data according to standardised ways of conceptualising error in epidemiology, but also materialised a means of judging instrument-data practices - according to the epidemiologists own epistemic values. In other words, the epidemiologists produced new data which quantified measurement error in modelling and monitoring data. These new data intervened in the multi-disciplinary tension around what counts as a good representation of air pollution by becoming a locally-made reference point from which a standard of 'good data' could be gauged.

The point I re-state with this brief discussion of a very complicated statistical practice is that, for the epidemiologists, error related to contingent understandings of the ways in which data represent people and environments spatially. This relates to the representational veracity of data, which is the ability of data to represent the truth most closely, and which was considered differently by the different research groups on WHAP. Since the epidemiologists were the researchers who planned to use modelled and monitored data in their analyses, what data best represents air pollution was considered in light of their own data practices. The simulation study was locally-made criteria for judging the representational veracity of modelled and monitored data, used by the epidemiologists instead of relying on the methods of testing data in modelling and monitoring data producing practices.

A second intervention: 'DIY data'

The second material intervention in the modelled and monitored data tensions was the making of what the environmental chemists called 'DIY data', like the simulation study data, the DIY data became one of the ways in which the 'problem' of data was materially intervened with. The DIY data were collected by the environmental chemists in City B and City C. Making DIY data involves collecting, processing and analysing measurements made by different types of hand-held measuring instruments. Data are collected manually, although, as I will highlight, the collection also involved technical instruments and are also mediated practices. The simulation study produced data on the variation of the representational veracity of modelled and monitored data; they were then, data on data. However, the DIY data were new data on air pollution, providing finer scale measurements of air pollution, and thereby new information on the 'inner heterogeneity' of air pollution within the 5x5 km grid squares. The DIY data were useful because they offered purchase on the representational veracity of modelled data and monitored data by providing new data on the variation of air pollution levels within the grid square. Like the simulation study data, DIY data were also data on the 'mis-representation' of air pollution by the modelled and monitored data, producing information on the extent of discrepancy between the model and monitor measures of air pollution at a scale of 5x5 km. The finer scale measurements made by the DIY instruments were of particular points within the 5x5 km grid square. In this way, the DIY data could be used to check what the model and monitor may be missing with their less geographically precise spatial measurements.

Chris, a senior environmental chemist, officially led the work package, supported by Elliot, a post-doctoral researcher, who carried out fieldwork and data analysis. In City C, a senior researcher called Stuart was responsible for buying the DIY instruments from the manufacturers, which were then deployed within City C and City B by Elliot. Chris was described by Stuart as, 'possibly the most experienced person in the country on these tubes and their limitations' (Interview, 27th September 2012). This, alongside Stuart's background in monitoring technologies and environmental health research, illustrates the academic expertise of the environmental chemistry group. Moreover, Chris and Stuart told me that they had worked together on previous collaborations, and that Stuart's involvement in WHAP resulted from Chris's invitation to join the project at the early stage of writing the funding proposal (Stuart, Interview 27th September 2012). Their ability to collect DIY data relied on Stuart's access to the DIY instruments, and Chris's experience in deploying and analysing these successfully. For example, Stuart told me that he had already been in communication with the manufacturers who supply these instruments, and who also provide them at a low price for scientific research because it is a method of evaluating their products (Stuart, Interview 27th September 2012). They also published on these instruments, including papers written by both Chris and Stuart on the topic of measurement error in these different devices (For example, Heal et al., 2000). In an interview with Stuart and the PhD student based at City C, the DIY instruments were elaborated on as a method that exists alongside other data practices, with papers written about computer modelling compared with DIY data (Beverland et al., 2012), and which can be used as additional comparative measurements with existing monitoring site measurements:

> There are the existing measurements by government and local government. I guess they will in WHAP focus on the government measurements as they are vigorously evaluated before they are published[...] so that's one part of it and possibly the main part, that's the safest route to go with government data [monitored] that's one part of it and possibly main part... so I think it's a bit like your project so it is slightly separate from the overall picture, and it could develop in a number of different ways depending on what opportunities arise, but that's the safest route to do with government data, and I suspect that's why Chris is pushing Elliot in that direction, with 'Analysing Air[36]' software, because you can use that to examine all these government data sets […] But then there is the home made measurements [DIY], made by us, which can be split between passive diffusion tubes and real-time monitors […] so that would be one set of homemade measurements to do a further evaluation of their limitations […] the idea was to use both of these approaches to gain some extra measurements, to supplement what is going on at government side, to try and understand what is the limitation of just having this one site [one monitoring site in City B] (Stuart, Interview 27th September 2012).

Stuart's description of the status of DIY instruments compared with other measurement practices offers insight into the tensions around different data practices, between those which are 'safe' because they are made by government led monitoring stations and validated via standardised procedures, compared to the more

[36] 'Analysing Air' is a pseudonym for a UK based programme which has produced software for air pollution analysis. Developed by a leading academic in the analysis of air pollution data, the software is presented by those on the programme as a way to gain insight into the multiple and large data sets of air pollution available. The programme also suggests the centralisation of data and data practices at a UK wide level.

basic, home-made style measurements collected with small monitoring devices. What is significant in this explanation of the different measurement practices is the way in which Stuart suggests that these data can each relate to one another in productive ways, and how one data practice is not considered alone, but in comparison with others. Furthermore, Stuart made the point that DIY data are also a good way to test the data produced by monitors, explaining that ‘the government has this one system, but then there is always innovation going on’ (Ibid). Thus, the additional data collected were understood as a means of not only measuring air pollution but to further understand different ways of measuring air pollution and therefore air pollution as an object of research.

Collecting and measuring air manually

Figure 9: The mapping of a field site (Presentation slide from WHAP project meeting June 2013).

The spatiality of modelled and monitored data, as particular representations of air pollution, were suggested as problematic by both Stuart and Elliot. Stuart suggests that having one monitoring station for the whole of City B might be limiting, in terms of representing air pollution. The collection of measurements within City B is a way to materialise multiple measurements of air pollution at a finer scale, rather than as a singular measurement for a given area. As the map shows (Figure 9), the DIY instruments are placed in different sites across City B. This is organised by Elliot who creates a visual map with points where instruments could be placed, he then goes out to the sites to see if they are suitable, with an aim to achieve 'maximal useful data' (Elliot presentation, June 2013). 'Being useful' relates to the suitability of the site: is it going to be tampered with? Is there somewhere safe to leave the device? But also, whether the site is different or similar to the other locations where the devices have been placed (to ensure, for example, that not every device is placed by a road side and therefore measuring 'background air'). The siting process directly shapes the representational form of the subsequent data collected (one of the rationales for making DIY data). Indeed, the decision making processes reflect wider concerns about the spatial veracity of models and monitors in their measuring of 'the same type of air'.

Elliot emphasises the construction of the spatial contours of the data to be collected, and the map above numbers the different locations where the monitoring instruments are placed, some of which are pictured on the right of the map in Figure 9. Like in the building of a model, or in the classification of areas where monitors are to be placed (rural, urban, roadside etc.), Elliot constructs a space within which measurements are made and made sense of. However, rather than one measurement being made for one 5x5 km grid square space, the DIY instruments are manually placed in different places within the grid 5x5 km grid square.

> Another area is variation of exposure within grid square, and that's only something our additional measurements could get some sort of handle on, although very limited, and to examine ways in which variations, days with high Ozone vary within a grid square. We could pick a general square but with some difficulty considering how many hundreds of grid squares there will be. But still it is possible to get an idea of the magnitude of variability and whether anything in particular drives this so that we could do something more systematic about what's going on within a grid square (Liaison meeting 2nd April 2012).

Different instruments are used to measure different types of pollutants. So, in each location marked on the map, Elliot places a number of devices. There are four types of instruments (see Figure 9), presented by Elliot. One of these is a 'Palmes diffusion tube' (Figure 10), which measure gases NO_2 and O_3, often referred to as 'passive samplers' because they measure the concentration of pollutants in the air without electrical power (the other instruments shown require electricity). The diffusion tubes work by measuring the absorption of the pollutant at a regular sampling rate. Elliot explains that the technical features of the DIY instruments work in a similar way to the monitoring stations, and that if it was possible to have monitoring stations everywhere then they would collect data that way, because of measurement accuracy. Like monitors, the instruments absorb the pollutant that is being measured for. In a monitoring station these are measured by UV light, in DIY data practices these are measured in the laboratory.

Figure 10: A Palmes Diffusion Tube (Presentation slide from WHAP project meeting June 2013).

I observe the process of taking a measurement of NO_2 from the diffusion tubes sample collected outside and stored inside the fridge in the laboratory, at The University:

> The gauze at the top of the diffusion tube is soaked in a chemical called 'triethanolamine', which absorbs the pollutant you are measuring for, and the tube contains the space for a measurement to be taken, otherwise wind and external elements affect the instruments, and by "encasing the signifying thing in the tube then you can stabilise the measurement taking process" […] Laura then adds water to each of the diffusion tubes. The samples inside the tubes wash off the pollutant absorbed and it is this stuff that is 'washed away' that is the pollutant and what is measured for (Fieldnotes 25th July 2014).

Like in the process of measurement at monitoring stations, the air pollution is separated from air, but here it is carried out by adding a chemical that separates the pollutant from the other parts of air captured in the tube. The separation can be seen because the solution turns different shades of pink. The amount of pollution can be determined by eye, where the darker the solution the higher the concentration of air pollution (Figure 11):

> Once the water is added, Laura shakes the tubes so that all the NO_2 goes into the solution. The absorbance of NO_2 is measured by adding Sodium Nitrate to the solution, which then absorbs the NO_2. You can determine the measurement in terms of the colour of the sample, which ranges from pale (lower concentration) to dark pink (higher concentration), but this was then officially measured using a spectrometer[37], which were located in a separate laboratory but in the same building (Fieldnotes 25th July 2014).

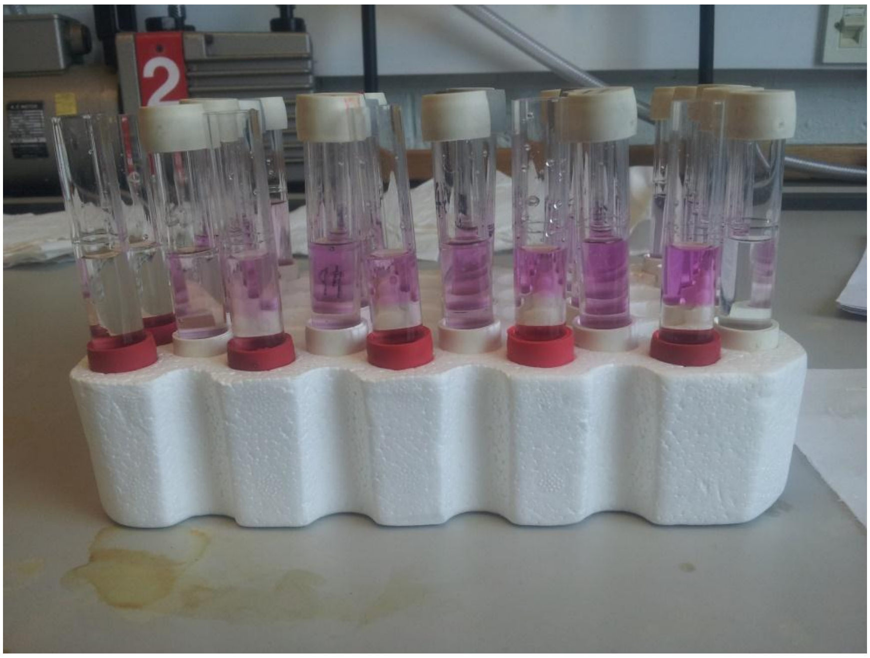


Figure 11: Analysing Palmes tubes in the laboratory (Personal photo).

[37] A spectrometer is an instrument used to measure properties of light over a specific portion of the electromagnetic spectrum, typically used in spectroscopic analysis to identify materials. They function similarly to the monitoring station monitors, where the amount of pollutant in the same of air absorbed into the chemical solution is measured by measuring the absorbance of light.

Analysing air

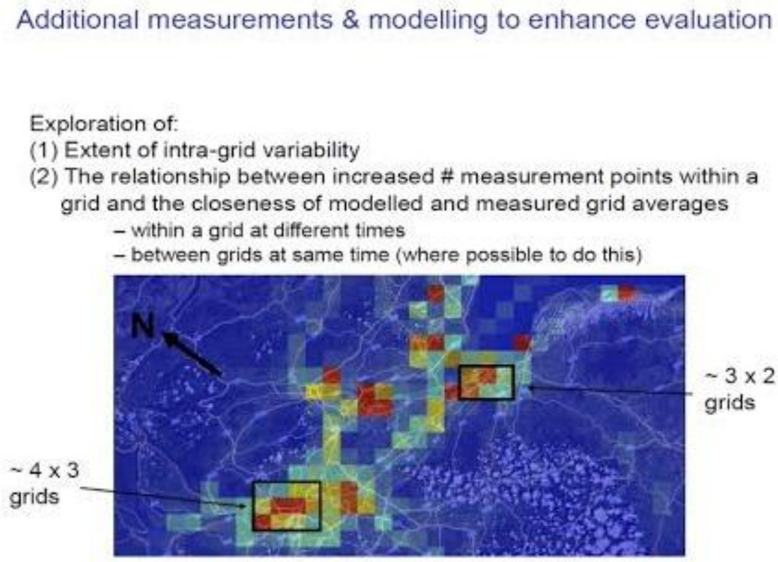


Figure 12: Supplementing data with data: increasing the representational veracity of data in terms of 'space' (Elliot's Presentation, Team meeting June 2013).

From these devices, measurements are collected and taken to the laboratory (or, for automatic samplers, automatically collected and downloaded through a data logger, which connects the instruments to a database at The University). Once a week data is manually collected at the sites and the instruments are checked. Elliot describes the arduous work of walking around City B, explaining that he was often accompanied with an MSc student to help him carry the equipment and take the readings. The 'winter campaign' had just finished when we spoke, and Elliot recalled the hard and often unpleasant task of walking long distances in the rain. The campaigns refer to the period, usually about ten weeks, during which the installed devices take readings of air pollutants, which are continually processed and stored together. I mentioned that I would have liked to join him on one of these expeditions (instead I observed the formative stages of developing the DIY experiments and the final stages of analysing the data in the lab); he laughs, saying that everyone says this. Staff from other departments, and even other members of the WHAP project based near City B, always say they would like to join him but never do, they always have something different to do, inside. Again, the reactions of myself and other scientists suggests the novelty of physically doing science, without the mediation of offices, laboratory environments and computer systems, and a related kind of authenticity to this manual scientific work.

The comparison process is carried out with the computer software package ‘Analysing Air’, which enables the comparison of different instruments - of model, monitor, DIY - is carried out by Elliot, who produces a comparative table of all the instrument readings. This map (Figure 12) of an increasing number of measurements within the grids produce by the model demonstrates visually the extra information that the DIY data provide, highlighting the increased granularity and spatial scale of data. There are not monitoring stations in every grid square. For example, in City B, where the additional measurements are collected, there is only one monitoring station, which, it is claimed, does not provide representative data of air pollution in the city. As a result the additional measurements are used as a way to understand ‘inner grid square heterogeneity’.

Representing the spatial area in new ways was aided by a process called ‘conditioning’ (Figure 13), using an open source software tool (analysing air) to ‘characterise the data further’ (Elliot, Personal Correspondence 25th July 2014). The tool enables the modelled data, monitored data and DIY data to be compared together as a way to produce a better representation of air pollution in the 5x5 km grid square. These two types of additional data are, then side-lined both as a non-conforming process of data collection in air pollution monitoring more generally, and within the project as an additional, secondary practice to the main data production processes as suggested by Stuart.

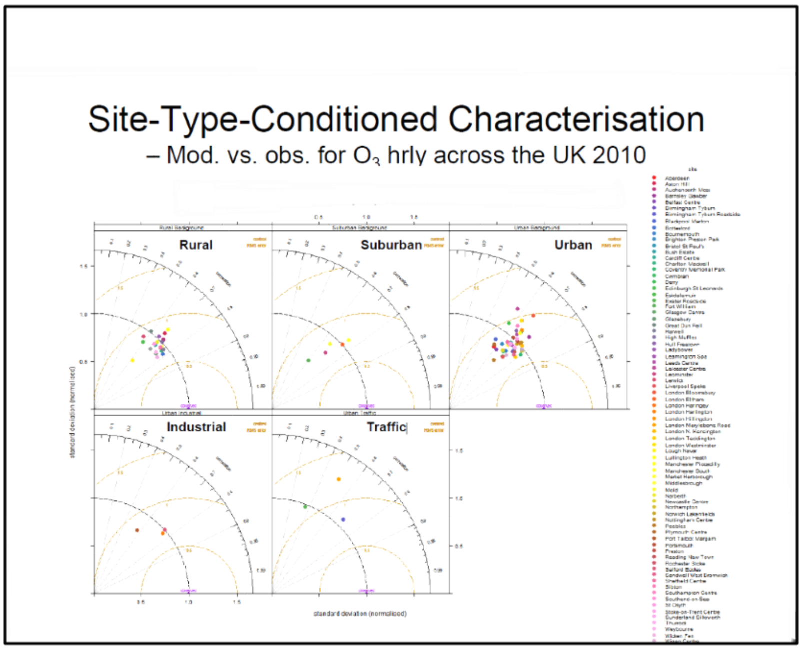


Figure 13: Comparing data with ‘Analysing air software’ (Elliot’s presentation, Team meeting June 2013).

DIY data are used to provide purchase on the representational veracity of modelled data in the WHAP project. As I highlighted in the previous chapter, modelled output is always compared with available monitored data. The way in which these data are used in ways together shows the different informational value different data offer. DIY data are used in addition to monitored data measurements as a way to assess the ‘fitness for purpose of the model simulations for epidemiological analyses’ (Project Protocol), but are not used as ‘data’ in epidemiological analyses. These side-lined data practices (compared with the central role of modelled and monitored data), enable the construction of a material and conceptual space through which negotiation across data practices can take place.

Finding a shared object of research

I have taken the tension around what data counts as good data to explore the ways in which different technological objects enable practices and generate entities. The relation between different data and data practices on WHAP highlights the shifting relations between data and the entities they materialise. In this way, I show that sensing practices through monitors and devices, but also through statistical data practices, are a way of engaging with air pollution as data sets. The connections made between these relations are used to produce knowledge on both air pollution and methods of sensing air pollution.

The multiplicity of data and of the air pollutions they materialise is problematic for researchers because of the way in which space is configured through data practices. This is significant to the study of air pollution as a phenomena which is un-fixed and often rather insubstantial. For WHAP researchers, testing the representational veracity of different data, in order to work out what counts as a good spatial representation of air pollution required the production of new data. This in-between process of data-in-negotiation, illustrates the ways in which data are an emerging composite of material and non-material relations: they are the material through which phenomena come to be known, but also embody scientific ideals and values (which also make data particularly good boundary objects in principle).

Furthermore, a distinction between simple and complex data practices became apparent within WHAP, a set of relations drawn upon by Elliot in his presentation of ‘the additional measurements’ at the June 2013 team meeting:

> Presenting three options, Elliot explains that in order to decide where to place the instruments they will not use GIS technology but follow the ‘primitive’ option of ‘judgment’. This causes laughter, with the PI reinstating the decision presented by Elliot: ‘I assume that that is because it is the primitive option?’ (Fieldnotes, Team meeting 13th June 2013).

This extract highlights the ways in which data were considered in light of the practices which made them, and relates back to the epidemiological concern for empiricism. DIY data are ‘collected measurements’, and represented by Elliot as a simple process used to test and validate models. ‘Simplicity’ and ‘mediation’ also relate to the form of representation that the epidemiologists feel comfortable with, as I highlighted in Chapter Four. Indeed, I suggest that the literal and physical form of contact with air pollution that DIY data collection inheres was rather valuable to the epidemiologists. In practice, the collection of DIY instrument data did not seem to be simple, but rather complex, involving the physical movement of the measurement from its environment into a laboratory for further transformation. ‘Simplicity’ then seems to be a rather more difficult concept to unpack, which relates to the nature of the mediation in the making of a technologically-mediated measurement. In this way, the DIY instruments, are simple technological instrument, are considered as less mediated interactions compared with computer simulations.

I am now going to examine the implications of these two data interventions in terms of ‘realism’ and ‘empiricism’. There was a tension in the epidemiologists’ assumptions that ‘they know how it works’, with reference to the DIY instrument and monitoring practices, and what happened in practice. The use of DIY data as a comparative reference of the modelling was built into the WHAP project, and is illustrative of the epidemiologists’ concerns about observing reality. Indeed, a paper reflecting on the notion of causality in epidemiology suggests epidemiological concepts of empiricism are configured by particular notions of realism and empiricism:

> The realist postulates that empirical observations do refer to some reality in the external world (independently of theoretical models); the empiricist strictly sticks to observable entities, avoiding any judgement about the essence of reality (Daston and Galison, 2007: 85).

According to Daston and Galison, then, the epidemiologists appear to be both realist and empiricist, and it is this combination which provides them such powerful leverage as the science of public health. However, in order to maintain this in the multi-disciplinary setting of WHAP, where the methods of achieving ideal standards of representation are different, ways of measuring being ‘empirical’ took place. In epidemiology, modelled or monitored data are considered as the raw data to be used not as an explanation but as part of their statistical model. Monitored data are considered more pure and less processed than the modelled data, implying that modelled data are too processed, in terms of the extent of technological mediation. The epidemiologists are not interested in intervening with air pollution through data, but in its appropriation within their own data practice. Although, as I have argued so far, modelled and monitored data are both the results of complex crafting, for the epidemiologists there is an element of authenticity to the monitoring measurement which are physically collected. This finding contrasts with those who argue that objectivity coincides with ‘mechanical objectivity’ (Daston and Galison, 2007; Daston, 2008) and ‘digital objectivity’ (Beaulieu, 2002). For the epidemiologists it is the very act of being in the spaces where air pollution is considered as being that adheres to their notion of empiricism and what counts as a ‘good representation’.

The non-material and representational dimensions of practice were in play during this intervention, where conceptualisations of being ‘empirical’ and related notions of ‘the scientific method’ were materialised through the production of new data. The simulation study data didn’t directly represent air pollution, but were instead silent representations. Yet, these silent representations had material effects because modelled and monitored data were studied in terms of the amount of statistical error they produce, and as a result new claims were made possible about modelled and monitored data. Indeed, the production of the simulation study data and the DIY data demonstrate the material ways in which multiple research practices relate to one another. What is more, they show how data practices, and the relations between them, shape the meaning of different data, permitting particular actions and thereby further articulations of air pollution.

As a result of these two interventions, the M&M tension became an issue centred on four different forms of data: the simulation study, the modelled data, the publically available monitored data used by both groups, and the DIY data. These different kinds of data, and the contingent multiplicity of air pollutions they enact, although problematic, worked to construct a material space through which the different ways of coming to know air pollution were made tangible, and thereby the means by which difference was materially engaged with.

A multi-disciplinary interface

A ‘successful’ multi-disciplinary interface emerged in the form of the two bi-annual collaborator meetings: primarily in December 2012, and in the subsequent summer June 2013 meeting. By ‘successful’ I mean a research space co-ordinated in a way that enables different data to be used at the same time, which results only in a partial achievement, as a means of making difference commensurate. During these two meetings the simulation study data and the DIY data were presented as a way to make tangible the multi-disciplinary discussions around what data best represents air pollution. Rather than epistemological impasse, the intervening data (simulation study and DIY data) enlivened and materialised data in ways that engaged with difference. The two meetings also work ethnographically as a useful microcosm of the debate so far, thereby providing a synopsis of the playing out of the tension into a tentative co-existence.

The December meeting begins with Peter, the epidemiologist who initiated the M&M tension, presenting some of the technicalities of the simulation study, the results and their implication for the epidemiology group, claiming that: ‘for Ozone, monitors are always better than model and for NO_2 neither monitor or models perform well’ (Team meeting 6th December 2012). Following the sharing of the data produced by the simulation study and the DIY data collecting and analysis responses to the M&M issue, Peter suggests a compromise. Continuing on the topic of using monitored data informed by modelled data, the notion of ‘hybrid data’ is employed to indicate the mixing of two types of data (Team meeting, 21st December 2011).

The concept of hybrid[38] data highlights the significance of language and meaning in multi-disciplinary work, as Peter explicitly reflected: ‘the “data assimilation to hybrid shift” was a linguistic issue’ because the epidemiologists came to realise that data assimilation had a ‘fixed meaning’ for the modellers: ‘my intention was to conjure a word which didn’t have all the expectations of data assimilation in terms of coherence of the model’ (Peter, Liaison Meeting 2nd April 2012). Unlike the technical process of data assimilation the concept of hybrid data worked as a flexible, discursive tool, used in an attempt to cross disciplinary boundaries and to find a space for negotiating data.

Rather than assimilating modelled and monitored data, which, as the modellers maintained, would require re-writing the model, the epidemiologists suggest using

[38] Another concept was also tentatively tested by the epidemiologists was ‘data fusion’, which, like assimilation, had its own disciplinary and technical history relating to particular data techniques, and was therefore soon abandoned.

both data according to the results of the simulation study. This would mean using modelled data for some pollutants and monitored data for others.

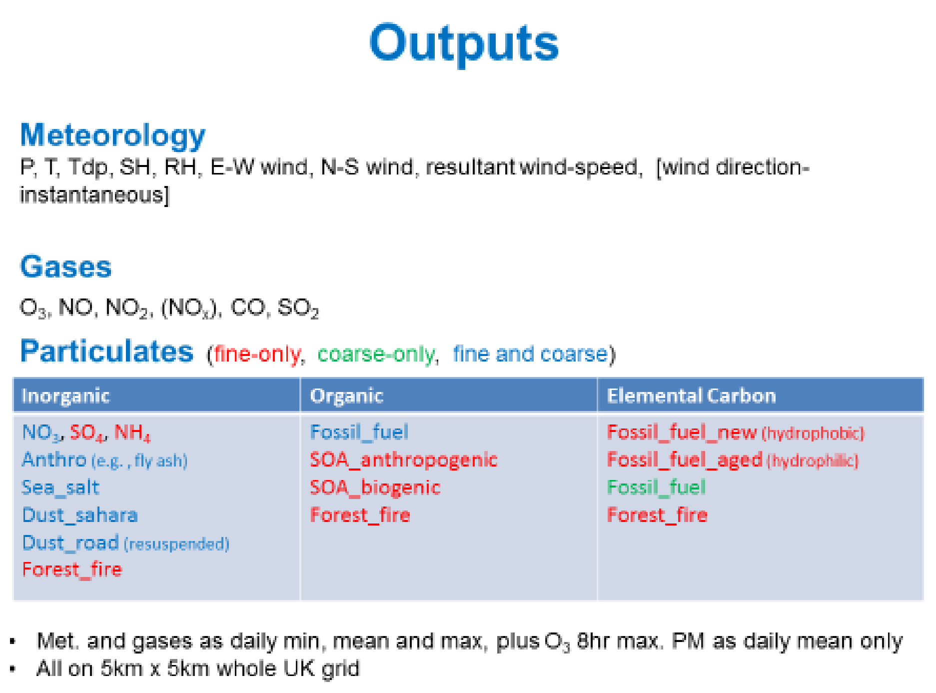
> […] in terms of "the modelled and monitored data tension", Peter explains that, in changing the pollutants of focus, the epis have found that actually relatively few monitoring stations measure the pollutants of interest, or rather "species of interest", so data from some monitors are redundant, in many cases "modelled data is the only game in town" (Peter, Team meeting 13th June 2013).

Modelled and monitored data were juxtaposed through the presentation of a table of the simulation study results by the epidemiologists, as a way to visualise and directly compare the two types of data, and as a means to distinguish which data should be used for which pollutants. As such, it was suggested that the epidemiological analysis would become the hybrid space where data are used together, and the modelling process would not use monitored data in their model construction or simulation runs, thereby maintaining the distinction between different data in different data practices.

Although the simulation study results were considered as legitimating the use of both modelled data and monitored data, they were employed by the epidemiologists as evidence for or against data according to pollutant type rather than data type. This is significant to the debate so far because modelled data maintains its purity by not assimilating it with monitored data, and the separation of data and data practices is agreed upon by the epidemiologists, shifting the focus of the modelled and monitored data tension from 'data' to 'air pollution'. Accordingly, the epidemiological analysis would then become the hybrid space where data are used together, and the modelling process would not use monitored data in their model construction or simulation runs. Modelling is the only practice which generates data on this complex and heterogeneous pollutant: they are, in Peter's words, 'the only game in town'. $PM_{2.5}$'s inherent heterogeneity and characteristics as a pollutant are drawn upon by the epidemiologists, to argue that modelled data are better than monitored data for their epidemiological data practices. Not measured by monitors or DIY instruments, $PM_{2.5}$ cannot be validated by external, 'empirical' data, and so the epidemiologists have no choice but to use modelled data for this pollutant. Although this decision is one that is shaped by the data available, rather than a resolving of the epistemological tension, it did shift the M&M tension from what data is best to what kinds of air pollution the team are interested in. As Peter points out, $PM_{2.5}$ is a pollutant of interest these days because of its understood negative health effects[39].

> For the first time in Europe, we now report a relative risk estimate for $PM_{2.5}$ based on $PM_{2.5}$ derived from monitored PM concentrations […] Results from a follow-up study of Medicare patients indicated that smaller particles and their components derived from combustion sources (ie, $PM_{2.5}$) are principally responsible for cardiovascular hospitalisations attributed to the combination of fine and coarse particles (ie, PM_{10})[…]where the recorded underlying cause was an arrhythmia, including atrial fibrillation (though the proximate cause may have been a complication of such arrhythmia), and pulmonary embolism […] (Milojevic et al., 2014: 1096).

The increased interest in health concerns related to $PM_{2.5}$ also shapes the modellers interest in developing the CM-WM model's capacity to simulate this complex pollutant. The slide below presented at the December 2012 team meeting highlights the different components of $PM_{2.5}$:



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[39] It is the heterogeneity of $PM_{2.5}$ as a pollutant that is of interest to both the modellers and the epidemiologists here. For example, in the paper quoted below (Milojevic et al., 2014), the epidemiologists highlight the increase health risks associated with $PM_{2.5}$.

Figure 14: CM-MW Modelling outputs (Atmospheric chemist's presentation, team meeting 6th December 2012).

Elizabeth explains that $PM_{2.5}$ is a pollutant which the model has only recently been able to simulate.

> Elizabeth further exemplifies the importance of modelling $PM_{2.5}$, claiming that there are many different components of $PM_{2.5}$, and "there are lots of processes we don't know until it is simulated" (Fieldnotes, Team meeting 13th June 2013).

Here, Elizabeth points out that the model is the only practice which can simulate the complexities of $PM_{2.5}$, which relates to what data the epidemiologists choose to use for their analysis because monitors do not measure $PM_{2.5}$, specifically[40]. $PM_{2.5}$ functioned, then, as a pollutant around which different data practices could be re-considered in terms of what and how they represent.

The role the pollutant $PM_{2.5}$ plays in the negotiations between the modellers and epidemiologists exemplifies the shift from a discourse around representation to the material work of making data, and from the informational and material relations of data to the research object of air pollution that data enacts (in multiple ways). Indeed, the particulars of data as kinds of air pollution became the object around which the different interests of the epidemiologists and the modellers were accommodated. The questions 'what data is best?', then, is contingent on the kind of data available, the practices which use and re-use it, and the wider researcher interests which come to shape local-level research practices. The tension around modelled and monitored data was negotiated through data, and in doing so the particular modes of ordering enacted by data practices, although temporarily unsettled, remained intact. Furthermore, the shift from data to air pollution is materialised and made tangible across research practices through reference to one pollutant in particular: $PM_{2.5.}$

[40] Monitoring stations do measure Particulate Matter, but are presently unable to distinguish between particles which are smaller or bigger than ten micrometres in diameter.

The simulation study data and DIY data do not directly or materially contribute to this decision, because neither data practice produce comparative data on $PM_{2.5}$. Yet, the simulation study data and DIY data facilitate this process of co-ordination because they enable researchers to share, but also engage with, multiple types of data and thereby different air pollutions. Here, I return to the craft-work of data production, which I described in Chapter Four, where seeing and knowing air pollution as data relied on the holding together of embodied, tacit knowledge with the collective standards of establishing error free, clean data. Yet, the two interventions of the simulation study and DIY data are productive here too because they provide a means by which to carry out multi-disciplinary dialogue and exchange. Indeed, a joint paper between the modellers and epidemiologists on modelled and monitored data was subsequently written on the re-use of modelled data in epidemiological analysis, illustrating the movement from a highly charged epistemological tension to a material output of multi-disciplinary working.

The two types of data were shared and presented by different researchers on WHAP, and in doing so, so were the values and theoretical principles of scientists, which get entwined with data in practice. As I highlighted in my tracing of the emergence of the M&M tension, abstractions such as 'truth' and 'representation' are potentially fractious, making engaging in multi-disciplinary research extremely tricky. However, relating data in new ways made the theoretically complex problem of working out what counts as a good representation a locally configured practical solution. The intervening data made difference and multiplicity tangible, and therefore forms which could be engaged with by different researchers, so that, although different data and different air pollutions were the initial problem, I've shown that they also form the basis from which 'non-difference' and stability emerged.

Co-ordinating data, achieving 'multi-disciplinarity'

This brings us back to the question: how does a multi-disciplinary team co-ordinate different data and the epistemological differences embedded within data practices? Star and Griesemer (1989) show us that different epistemic communities can produce shared representations of nature with co-operation rather than consensus. Boundary objects allow for the heterogeneity and complexity of knowledge objects to remain in focus. By following data practices of WHAP researchers, and the manipulation of data and objects of research through these, I've show that data are conceived of as phenomena embedded in evolving assemblages, rather than as discrete entities with unshifting boundaries by researchers.

I use the term co-ordination rather than co-operation because of the material ways in which data are re-configured in relation to one another and across different spatial scales. 'Additional data' are produced in order to investigate how best to represent air pollution within the space of the 5x5 km grid square. Indeed, the pertinent question, 'what data best represents air pollution?' re-produces air pollution as a composite of different instruments, and which enables particular kinds of practices, as the points where multiplicity emerges. The M&M tension is an interesting case study because it shows that multiplicity is not always contradictory. For researchers on WHAP, each type of data seems to determine the other to some extent, in that they provide supplementary information of air pollution within the grid square. My case studies highlight the relationships between different kinds of data, in terms of what they represent and how, and subsequently how these different data are used by researchers to intervene in multi-disciplinary tensions over the representational veracity of data in productive ways.

In tracing the practice and processes of data-in-negotiation, I have also shown the way in which data work as both epistemic things and boundary objects for researchers on WHAP. Data are not only informational but are the material means by which relations are opened up and extended between researchers and their concomitant data practices. Where science studies stress the importance of focusing on practice to examine the interactive and material engagements of scientists with the world, I show that the researchers on WHAP also pay attention to practice as a way to negotiate and intervene in ways of engaging with the world. I contribute to discussions on sharing data by illustrating the material ways in which data participate in sharing and achieving multi-disciplinary interactions in productive ways. The composition of data practices, and the relations between different data, can be disentangled by focusing on the movement and transformation of data between different research groups.

'The simulation study' data and 'the DIY' data are both ways to manage the stuff that is not measured and therefore the error that data belie. For example, DIY data are additive, filling out the spaces in-between modelled and monitored measurements, whereas the simulation study provide new data on statistical error of modelled and monitored data, which measure the size and character of the error in data. As a result, these new data become a reference point for the epidemiologists to lever their own conceptualisation of data's meaning (previously only data producers had that ability), with the effect of delineating data according to particular air pollutants rather than simply by the practice which produced it.

Unlike the 'packaging' of data in bioinformatics, where Leonelli found that work took place at the local level in order to free data from its provenance, data's properties, boundaries and meanings, are packaged and re-packaged in a constant tacking back and forth between different data practices in WHAP. Furthermore, 'data friction' (Edwards et al., 2011) at multi-disciplinary interfaces do modify components, because data in WHAP is negotiated materially by researchers, even if ultimately modelled and monitored data maintain their shape as distinct entities. As a result of these negotiations through different data, the object of enquiry for researchers shifts from data as representation, as the epistemological dimensions of research practices, to the object itself, as the site of ambiguity and therefore potential for transforming the object of research. This means that data become the objects by which different research practices negotiate multiplicity and difference, functioning as tools for intervening with multiplicity.

The modelled data's ability to capture air pollution's inner heterogeneity and complexity means that data, the heart of the M&M tension, were also the method by which epistemological difference was transcended. Starting with multiplicity I've ended by illustrating the role the relations between different data play in co-ordinating difference. This was done through the abstracting of relations between data and air pollution, in their re-articulation through new data. And as a result, this made the specific kind of air pollution that the project is interested in more particular. Multiplicity and difference were, then, productive, enabling a material and discursive space for sharing data and talking about epistemological problems, facilitating the different research groups on WHAP to see, appreciate and engage with different ways of coming to know air pollution through data.

Summary

This process of co-ordination through data also marked a wider shift in ‘the nature of the problem’. The modelled and monitored data tension was primarily characterised by researchers on WHAP as a representational issue concerned with what data best represents air pollution. However, I show that this developed into a more ontologically orientated concern, around what air pollution the project is interested in producing knowledge on. This meant that researchers practically engaged with the problem of multiplicity because it was not simply that different data offer different perspectives on air pollution, but that these different data enact different kinds of air pollution. That there were multiple air pollutions produced by different data practices was rather problematic, and one of the ways to manage this was by focusing on pollutant type rather than data type. Indeed, that it was the epidemiologists who ultimately claimed what data is best according to pollutant type rather than data type, does not mean that the modellers came to share the epidemiologists’ concept of empiricism. As I have argued, data-as intervention was a process of co-ordination rather than consensus, which did not shift researchers’ notions of ‘truth’ and ‘representation’, even though the tension was resolved, temporarily at least. The solution was shaped by this shift from the form of representation - data - to the object of research - air pollution.

The tension was reduced through a locally constructed, practical solution: the pollutant $PM_{2.5}$. What worked about $PM_{2.5}$ was, first, that it is internally heterogeneous and therefore intrinsically multiple. Indeed, it was classified according to size rather than its chemical make-up. Second, there is scientific consensus around its related negative health effects. Finally, the CM-WM model is the only method for measuring $PM_{2.5}$. $PM_{2.5}$, then, carried both wider meaning in terms of the configuring of new relations between air pollution and health, but also appealed to the particular research interests of the atmospheric chemists and the development of their model. As a complex and heterogeneous chemical species $PM_{2.5}$ was also of mutual epistemological interest. I show that 'social' and 'scientific' concerns were entwined in WHAP, and $PM_{2.5}$ as a particular kind of air pollutant offers purchase on the indefinite nature of these boundaries, which were not about the correspondence between representation and reality (of nature or of culture) but related to matters of practice, doings and actions (Barad, 2003). In the next section I explore the movement of data and the transformations they undergo in their connection with new phenomena, discourses and fields of practice, showing that multiplicity and co-ordination are only ever partially resolved in practice.

PART III - External difference: *linking* data, making a *selection*

Chapter 6: Data linkage and the visual practice of epidemiological data production

Introduction: *linking* data, visualising data patterns

> [All] this makes superb logical sense if in the world we found a nice consistent association with mortality, then we would be sussing out which components of each one were adverse, but unfortunately we are not in that world (Peter, Liaison Meeting 20th January 2014).

> [I]n the science we study, the problem appears not to be, as Merleau-Ponty said (1962:78), that "what you see depends on where you sit", but rather "nothing is more difficult than to know exactly just what we do see". Whatever role perceptual grammars may have in shaping what counts as evidence in disciplinary traditions, these grammars do not resolve the manifold problems associated with visual sense data in day-to-day laboratory work. The point is that just as scientific facts are the end product of complex processes of belief fixation, so visual "sense data" just what it is scientists see when they look at the outcome of an experiment are the end product of socially organized procedures of evidence fixation (Knorr-Cetina and Klaus, 1988: 134).

I am sitting in the basement meeting room at The College, where Tim and Rob are checking that the document being shared with other researchers is the same version. Most of the epidemiology research group are joining in remotely, and it is important for everyone present to be looking at the same results. The epidemiologists are presenting their preliminary analyses of the air pollution and health data, the first analysis with 'the real data' so far. This is presented as a series of graphs, used as a way to discuss, primarily with the environmental chemists, options on explaining the results and thereby ways in which to proceed with their subsequent research. Although previous analysis has been conducted on different, 'old' data sets with access to both government health data and air pollution data, the epidemiologists are now in the process of analysing and refining their statistical model in terms of the 'real data' (Fieldnotes, Liaison meeting 20th January 2014).

The results are presented as graphs, where the air pollution data (x axis) and health data (y axis) are correlated through statistical practices, and the subsequent patterns on the graph used to work out ‘health risks’ of exposure to air pollution. Here, a health risk is the statistical probability of illness or death from exposure to certain levels of air pollution. The graph below (Figure 15) demonstrates what a negative correlation looks like. I use this graph as a starting point here because it forms the initiative for my writing of this chapter, in terms of the epidemiologists concern around how to best visualise their epidemiological data. Indeed, the graph is the first of a series of ways through which the epidemiologists claim that their preliminary analyses of the linked data show ‘surprising correlations’.

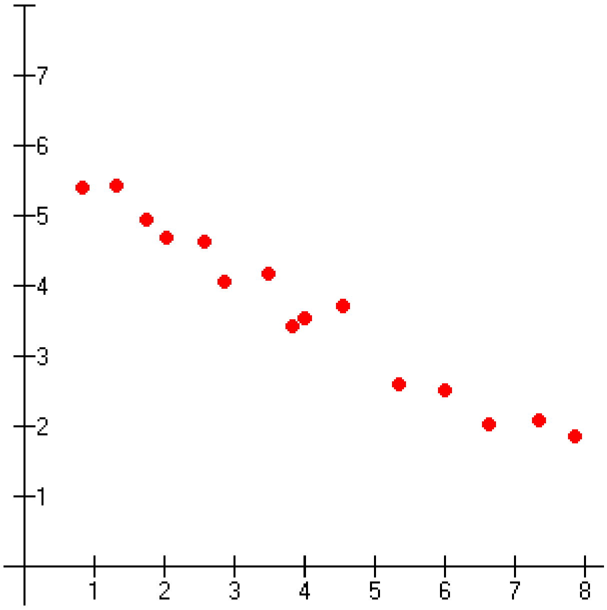


Figure 15: An example of a negative correlation between two types of data.[41]

I ended the previous chapter by demonstrating how multiple data were co-ordinated by researchers. $PM_{2.5}$ anchored differences between researchers, being both a pollutant of concern in terms of health and a key pollutant in modelling practices (because of more recent developments in the model’s ability to simulate the different component parts of $PM_{2.5}$ separately). In this way, it was the particulars of air

[41] The graphs I show in this chapter are not the graphs shared at the liaison meeting, the fieldwork which forms the basis of this chapter. However, the statistical techniques employed are the same as those discussed in a paper published in the same field, relating to the same statistical problem. Accordingly, I used the data sets from this referenced paper in order to produce the graphs used in this chapter.

pollution which guided the use of modelled and monitored data by the epidemiologists, and which shaped the very composition of air pollution as a shared research object. Rather than what data represented air pollution best, researchers now focused their efforts on considering what kind of air pollution the project was interested in.

The next move, and the topic for this chapter, involved the re-use of air pollution data in combination with health data in order to construct statistical correlations between the two. In this way the object of research moves away from measuring air pollution to the external relating and creating of new relations between air pollution data and health data. The process of bringing together the objects of research, air pollution and health, through data was called 'data linkage' by the epidemiologists. One part of this process of linking and correlating was 'over-laying'[42], which enabled the different data to be linked and prepared for the making of 'statistical correlations'. The practical work of linkage was like a process of layering, in which each type of data was entered into a database, where they were stored as a map with a specified theme, called a 'data layer' (Nuckols et al., 2004: 1007). The material work of over-laying was a way of preparing the multiple data sets in order for the epidemiologists to be able to carry out their statistical analysis.

By focusing on statistical data practices through images I extend what, in the opening quote, Aman and Knorr-Cetina describe as a process of evidence fixation (1988), because visual images are not only the endpoint of 'sense making', but a fundamental component of epidemiological data practices. As I will go on to show, the visual images were in many ways the starting point rather than end point of negotiations. The images were a way to untangle and re-configure the practical, epistemological and ontological demands of air pollution as a statistical research object. I draw upon Coopman's notion of 'artful revelation' (2014) to examine just

[42] Linking, over-laying and the making of 'associations' were three processes through which the epidemiologists described the process of working out statistical correlations. I re-use the term 'correlate' for my own conceptual work here also, as a way to capture the work that goes into making data relate in meaningful ways, and the mutual effects of the different data on one another in practice (beyond simply a statistical correlation, of course).

how a visual aesthetics informs the interpretation and implementation of graphs in the process of revealing the 'right' kind of correlations between data of air pollution and health. I argue that statistical data practices are inherently hybrid (Carusi and Hoel, 2014), both material and abstract, taking place on WHAP as a dialogue - between researchers, scientists, instruments and particular instrumental arrangements - of visual considerations (de Rijcke and Beaulieu, 2007). In doing so, I examine what these statistical techniques do to the visualisation of statistical correlations, tracing the changing shape of the data in graphs. As a result, I show that making stable correlations shifted the research object from data to their visual form, where the visual capacities of graphs made air pollution visible in new ways.

'Linking' and 'Over-laying': materialising air pollution and health associations

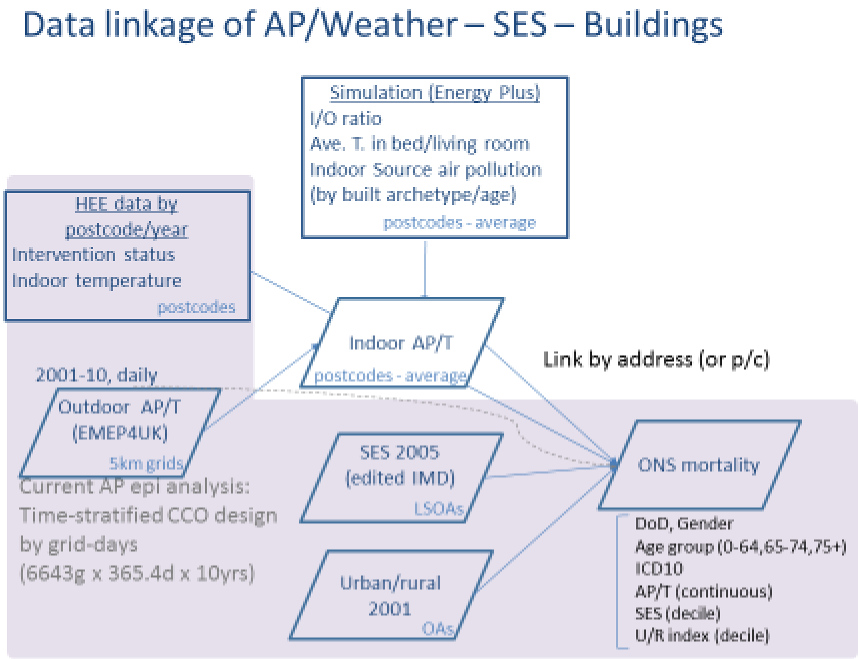
In order to produce epidemiological data on the relationship between air pollution and health the epidemiologists need 'empirical'[43] data on air pollution (indoor and outdoor) and health (mortality and hospital admissions). The national scale health data used by the epidemiologists includes: the Office of National Statistics (ONS) mortality data, the Myocardial Infarcation National Audit Project (MINAP)[44] data and the Hospital Episode Statistics[45]. These 'empirical data' are first linked together in one digital space, ready in order for the epidemiologists to carry out their statistical analyses.

Ann, an environmental epidemiologist, whose role it was to link the air pollution data and health data for the subsequent epidemiological analysis, described the process of over-laying as 'the last link in the chain' because it involves taking input from the other researchers (data produced by the other research groups on the project) with their (the epidemiologists) health data together (Fieldnotes, Team

[43] See Chapter Five.
[44] The Myocardial Ischaemia National Audit Project (MINAP) is a national clinical audit of the management of heart attack episodes, with the purpose of providing data on patients in hospital, their outcome and when they are discharged (http://www.ucl.ac.uk/nicor/audits/minap).
[45] Hospital Episode Statistics is a data warehouse containing details of all admissions, outpatient appointments and A&E attendances at NHS hospitals in England.

meeting 10th September 2014). It is Ann's responsibility to link all the different data produced and used in the WHAP project and to produce a complete data set which everyone can access (Meeting with Ann, 9th December 2013). The work of linking data was considered by researchers as 'technical' but not 'scientific', unlike the data cleaning or validation described in Chapters Four and Five. This meant that it was deemed essential for research to be carried out, but did not contribute directly to academic outputs. Data linkage was more of a representational device, through which further representations could be made. The Geographic Information Systems (GIS)[46] tool which enabled Ann to over-lay the different data within one digital space was referred to in the Project Protocol with a simple statement: 'each [health data sets] will be linked by day and location to the air pollution and weather data[…] using Geographical Information Systems methods'. However, the brevity of this explanation of data linkage certainly contrasted with the amount of time and energy researchers seemed to put into the process.

[46] GIS technology enables the visualisation of data in novel ways, used to explore the results of statistical analysis. Displaying the location of data on a map and the variation of these values over space and time are considered as contributing to epidemiological research.



.[47]

Figure 16: Diagram showing the different data and their particular informational characteristics which are significant for the process of over-laying (Ann, Personal Correspondence 26th November 2014).

Before the data can be overlayed they need to be made 'all the same'. Ann converts the three dimensional modelled data, which is in Network Common Data Form (NetCDF) format, into two dimensional text format so that it can be read by the epidemiologists (and by their statistical model). 'Reading' the data, of course, relies on particular, technologically mediated ways of seeing (Coopmans, 2011: 158). Converting data into the right format is not simply a technical modification, and requires discussion with the data producers in order to work out what it is that needs to be represented. For example, choices have to be made in terms of whether the data is the daily mean or the maximum or minimum measurement of air pollution. All these negotiations between Ann and the other data producers on WHAP shaped the subsequent material form of data, as a process of preparing it for further (statistical) analysis.

[47] Key to diagram: AP: air pollution, T: temperature, SES: socio-economic status, IMD: Index of Multiple Deprivation , U/R: urban rural, DoD: Date of death

I meet Ann in her office to learn about the linkage process. She collects some papers and a small folder before we move to a small, private meeting room on the same floor. I had, prior to the meeting, asked if she could go through the process of using the GIS tool to link data. I was slightly disappointed as, ideally, I had wanted to observe the process of over-laying at a computer interface. However, Ann explains that she has decided to show me more ‘literally’ what the tool does, by bringing print outs of data sets as physical representational forms of the data to be linked. She was acutely aware of how the computer interface leaves the work ‘black-boxed’ and was keen to explicate it visually.

> Sitting round a small table Ann spreads different sheets of papers with tables of data and diagrams of grid squares, explaining that there is a lot of data on the WHAP project and it is her job to ensure that these data can be transformed into a form that the epis can use. Ann demonstrates the process of over-laying by placing the different pieces of paper she brought with her on top of each other: “so if you imagine the grid squares I just layer the different data on top of each other like this, and I do this for pollutants, temperature chemistry and weather” (Fieldnotes 9th December 2013).

The process of data linkage is particularly interesting because it used the visual tool of GIS to represent multiple data in one technically mediated space. This particular action, involves a combination of practical, technical but also analytical discernments.

The table below was generated using GIS and orders the data according to specific instructions made by Ann. Each column in the table relates to a particular data and storage location. For example the ONS mortality data can be found according to the typed in command: ‘Mortality:sample_mort.csv (52 records)’.

Database	**ONS mortality**	**Air pollution / weather**	**SES (edited IMD)**
Time period (temporal resolution)	2001 – 2010 (daily)	2001-2010 (hourly -> daily)	England 2005, Wales 2006 (every 3yrs)
Geographic area (spatial resolution)	England & Wales (postcode/address-points?)	UK -> E&W (5km grid, 6643 grids inland only)	LSOAs (32482 in E, 1896 in W)
Data (*geo-identification)	date of death sex age category (<15, 15-64, 65-74, 75+) ICD10 region postcode/address of residence* RUAC (rural/urban category) place of death	Date X, Y in BNG (-> gridid)* daily mean temp. daily mean PM2.5 daily mean PM2.5_EC … other PM2.5 species daily mean PMCO daily mean PM10 daily max 8hrly mean O3 daily mean NO2	Region edited IMD rank edited IMD score decile group of edited IMD rank LSOA code*
Data format	Stata	netCDF -> stata	Csv

Figure 17: ‘The lookup table’: structuring data and information to preparing making correlations’ (Personal Correspondence with Ann 18th November 2013: ‘WHAP: Specification on data linkage’).

Ann describes the process of generating the table above in terms of a series of steps, beginning with the scale of detail required by the epidemiologists:

> To link the above databases, I create a look-up-table which lists all postcodes which have more than one death during target period and other geographical markers (grid ids, LSOAs, OAs) using GIS (if necessary) first. I then conduct text-linkage using the look-up-table [...] If health outcome data is available in smaller geographic marker (such as building, address points), then these markers will be added accordingly (Ann, Personal correspondence 18th November 2013).

The data are first materialised as textual descriptions. This requires inputting, locating and arranging the data according to the variables of interest, here: mortality, air pollution, buildings, socio-economic status. The table in Figure 17 highlights the organisation of data in terms of these variables and their spatial and temporal particularities, for example, the different pollutant species measured, the spatial and temporal scale and also the data format. These different ways of structuring different data, as holding particular kinds of information signal the kind of epidemiological correlations which will be made. The relations, or at least anticipated relations, between air pollution and health (the first two columns on the left), are not representing anything as actual statistical correlations yet, but they are anticipated in the organisation of data in the table, where the columns represent the different types of data and information they provide: the number of deaths, alongside the air pollution concentrations, the socio-economic characteristics of the population data and the rural/urban dichotomy which frame the spaces in which these interactions take place.

The unit of analysis for the epidemiologists become these spatially and temporally combined data units, which are both a digital and visual analytical form. The action of over-laying involves inputting the different data sets produced within WHAP (data of air pollution) and outside the project (health data) into one database on Excel. Using GIS this can then be visually over-layed within the same 5x5 km grid squares that the modellers use. So each grid square contains the different kinds of data: health data, chemistry data, weather data, building data[48]. In the final passage of the quoted extract above, Ann doesn't refer to 'data' but to the different components of air pollution, the chemical species and the conditions within which they exist in 'the atmosphere'. This suggests that linking different data together, as a move that links a series of material forms of information on air pollution is constitutive of linking the 'real' relations of air pollution. Again, this relates to the previous chapter, where I showed that inter-dependencies between data and researchers shaped the kind of research object that emerged (from air pollution-on-the-ground to air pollution-in-the-atmosphere, for example). Here, the ways in which data are held together in a linked state enable particular thereby enactments of data and subsequent claims to be made.

Following on from this process of over-laying, the next stage for the epidemiologists involves becoming familiar with the input data to be used for further analysis. The object of research becomes the visual objects themselves rather than the data, and this move makes visible relations between the different information, which data

[48] Here, I only focus only on the health data and the chemistry data, but the diagram (Figure 16) shows all the different data in each grid square.

embody, in the presentational forms of graphs and tables. The laying of data on top of one another is both addition and transformation, where the process of combining data also changes the informational capacities of these data. As Figure 17 shows, the air pollution data are linked to the health data (here, the ONS mortality data) so that each can be held in relation to one another. This is not simply adding two different types of data together, but shifts the phenomena of interest from air pollution to health. The computer software tool which enables linkage also produces a graphic visualisation of the multiple data within a constructed spatial (5x5 km grid square) and temporal unit (daily pollution concentrations), and it is these graphs that become the starting point for further analytical iterations.

Problematising time: constructing correlations between air pollution and health

> The epidemiological analyses will estimate associations of air pollution with health from the fluctuations over time of modelled daily concentrations of pollutants in each grid square and of counts (also daily) of outcomes of interest (deaths etc.), controlling for other time varying risk factors (weather, season, long term trends, day of week, and viral epidemics) (Project Protocol).

As I have shown, data are spatially organised, and each grid square (referred to sometimes as a ‘tile’ by the epidemiologists when talking about their time series analyses) becomes its own study for the epidemiological statistical analyses. This contrasts with my analysis in the previous chapter, where I argued that space is the problem for researchers in their making of good representations - spatial representations - of air pollution. As the object of research shifts from air pollution to health, the analytical discussions shift from space to time, because how to represent space has been fixed through the resolving of the M&M tension[49]. The problem for the epidemiologists, instead, becomes how to correlate different pollutants (as data) with different health events. This was worked out through the visualisation of relationships between air pollution data and health data (See Figure 18).

In order to construct relations between air pollution and health, the epidemiologists look for correlations between the fluctuation of health events and air pollution

[49] See Chapter Five

episodes; of how many people died or how many hospital admissions occurred on a certain day or days in a given area within each grid square: 'we compare events within the tile to the fluctuation in air pollution' (PI, Fieldnotes 23rd June 2014). This act of comparison means that, for example, the epidemiologists can work out whether, within a tile (a particular space: 5x5 km square area or post code area) and at a particular time (daily), it is more likely for people to go to hospital (a health event). A correlation, then, is worked out by quantifying the relationship between a pollutant level and a health effect. This statistical measure is descriptive in epidemiology, used to understand 'exposure- response'[50] relationships between human exposure to particular air pollutants and their associated health effects.

The notion of exposure-response relationship is also illustrative of the kind of work involved in the working out and materialising of the 'right' kind of relations between air pollution and health. Relations are materialised through data, and the object of inquiry becomes the visual forms of linked data. The first step is over-laying, as I outlined with Ann's literal performance of how over-laying is carried out with the GIS tool. The second step is analysing these materialised relations as 'linked data'. However, here a problem emerged. It became clear that the statistical correlations the act of over-laying enabled and made visible were problematic for the epidemiologists. They showed that in summer air pollution is bad for you and in winter air pollution is good for you. This is visualised as an inverse correlation (Figure 15) in the winter and as a linear correlation in the summer. For the epidemiologists, this preliminary finding was unexpected, because it suggests an inverse relationship between Ozone and Mortality[51].What was expected was that there would be an inverse relationship in winter *and* summer, meaning that high

[50] The response as exposure to a certain level of air pollution can be measured in different ways, but primarily as mortality or morbidity 'counts'. Counts are the number of admissions to hospital for lung and heart related diseases (these are understood as health risks for short term exposure to air pollution) or number of deaths (government mortality data). The statistical strength of the association of air pollution and health results from the fluctuations over time of modelled daily concentrations of pollutants in each grid square and of daily 'counts' of interest, of deaths and hospital admissions.

[51] It is well-known that in the summer exposure to high levels of Ozone increases a population's mortality risk (Pattenden et al., 2009; Bell and Dominici, 2008).

levels of Ozone are bad for you in the summer and winter. However, the initial results showed that high levels of Ozone are bad for you in the summer and good for you in the winter (Fieldnotes, Liaison meeting October 2013 - January 2014). Because this outcome goes against common-sense knowledge, the epidemiologists decide to test out their statistical methods to 'see what is really going on'.

The image of changes in air pollutants and deaths over time, specifically yearly patterns from January 2002 through to January 2007 work as a visual comparison of 'the variable' and 'the outcome' (See Figure 18). The graphs show patterns which signify a statistical correlation because of high levels of Ozone in the air and increased number of deaths, and also that there are less deaths when there is low levels of Ozone. For example, the graph plots shown in Figure 18 demonstrate exposure to Ozone in terms of the number of deaths over a five year period, where one wave is a year. The visualisation of a year is used to quickly reveal high-level patterns in the data (Bhaskaran et al., 2013: 1188).

As illustrated in Figure 15, the epidemiological results, however, show both a negative correlation and a positive correlation, suggesting that as air pollution levels increase, health events decrease in winter months, but in summer months, as air pollution levels increase, health events also increase (the latter relationship being what was expected). That these correlations are unexpected means the epidemiologists want to work out why the results show these correlations: 'the epi team are scratching their heads on this, about whether, about what we do in terms of these sharp distinctions between seasons?' (Peter, Liaison meeting 20th January 2014):

> Peter: we did have the discussion genuinely a priori in the epi group, and I don't think it was too far from Chris'. I resisted any multiplication of results on the grounds of not wanting multiple testing but both Tim etc. wanted seasons as relatively high priority to keep in [the analysis].
>
> Tim: I did, but I didn't expect one to be positive and one negative result I was expecting different positivities (laughter all round), but that's research for you. I have to say, one issue we will have and it may be that we are pushing the limits here a bit, but apparently, with these fairly precise results we have something that

> is good for you in winter and bad for you in summer errr, and that will take a bit of explaining.
>
> (Liasion Meeting 20th January 2014).

The graphs were, then, a starting point for working out how to visualise ‘the right relations’ of air pollution and health. ‘The wave’ is a form itself, described as ‘smooth’, ‘peaked’, or ‘dipped’, and each descriptive term used provides the data pattern with meaning, contributing to a trajectory of further modification. It was understood by the epidemiologists that it was the statistical work on the data (of the variables of interest) which made the patterns appear like they did, rather than anything intrinsic about air pollution and health relations in reality.

Since the epidemiologists were interested in short-terms effects of air pollution on health, it was important to try and remove the long-term seasonal patterns, currently visible in the comparative graphs (example Figure 18: between Ozone and daily deaths), so that only the short term patterns are visible. ‘Controlling for season’ in the statistical model, was one of the ways to reconfigure season as a ‘natural’ category. This was not a straightforward endeavour.

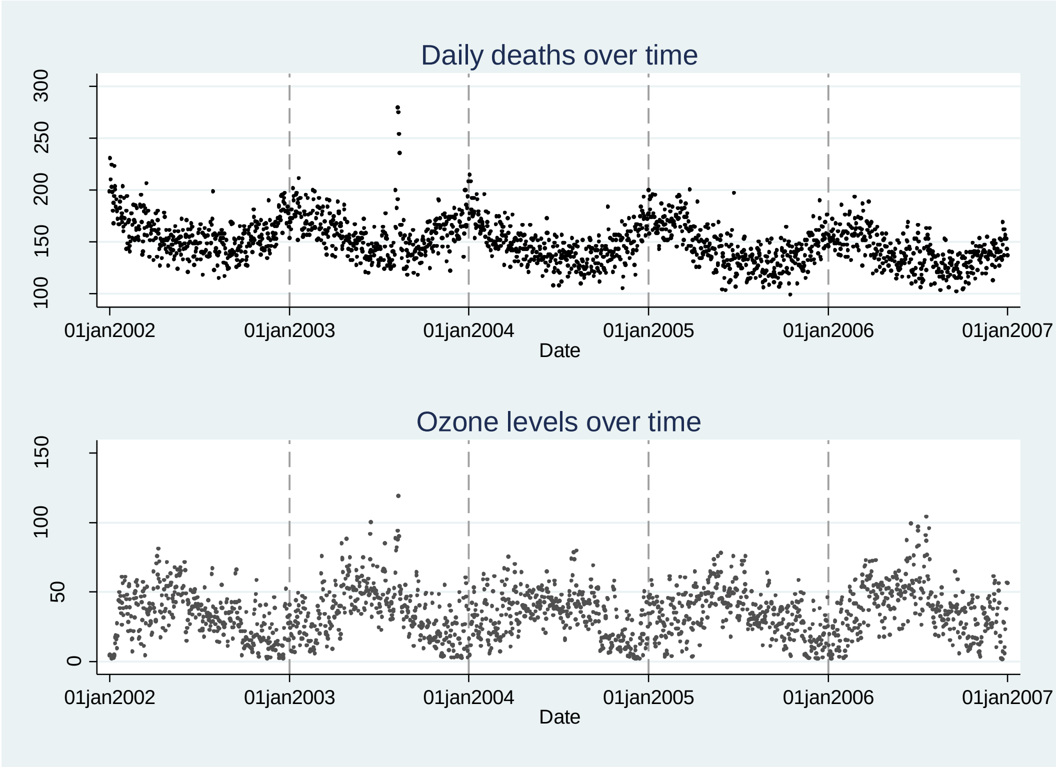


Figure 18: STATA run: Two scatter plot which show the correlation between levels of Ozone and mortality (Data from Bhaskaran et al., 2013).

The following dialogue takes place at a weekly liaison meeting and is illustrative of the difficulty of working out what a seasonal distinction does to the epidemiological analysis:

> Tim: yeah but my point is, on this, even though they are the traditional definitions of seasons... they are still somewhat arbitrary definitions of what a season is. And what I'm trying to work out, therefore, so what is the difference between autumn and winter?
>
> Chris: you are right they are a blunt distinction, I mean I guess it is convention, you have to work with something
>
> (Liaison Meeting, 20th January 2014).

As Tim points out, four seasons are 'traditional definitions' and 'somewhat arbitrary' posing the challenge: 'I'm trying to ask what is the difference between autumn and winter?' (Tim, Liaison meeting 20th January 2014). The expectation was that the results would show a positive correlation between air pollution and health across all seasons (see Figure 19 graph b).

A positive correlation between two variables means that as air pollution levels increase so do the number of health events. A negative correlation means that as air pollution levels increase health events decrease, or vice versa. A negative correlation between two data sets can be seen in a ‘x-y’ plot in Figure 19 e, where data points correlate in the top left hand side of the graph and decrease towards the right hand side, in the bottom corner of the graph, meaning that where there are high number of air pollution there are less deaths. A positive correlation is opposite, where the data correlate in the bottom left hand corner of the graph and increase towards the right hand side of the graph, meaning, say, that low levels of air pollution are related to low numbers of deaths and these increase together (See Figure 19):

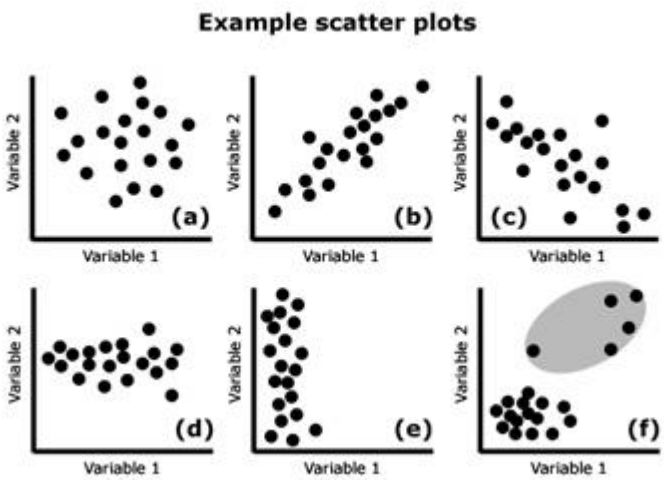


Figure 19: Illustration of scatter plots with various properties: (a) low correlation, (b) strong positive correlation, (c) strong negative correlation, (d) and (e) low correlation, with very little change in one variable compared with the other, (f) this scatter would generate a spurious high correlation because of the effect of the five points enclosed by the shaded area.

Tim proposes transgressing, what he calls, traditional seasonal classifications, suggesting that autumn and winter are the same in terms of the meaning of the data. He argues that health effects could be captured just as well with a binary distinction of a warm summer and a cool winter. However, Chris responds by pointing out that season is entwined with emissions production and climatology, arguing that season cannot be detached from the natural rhythms of the atmosphere. Chris argues that seasonal relationships are particular to specific air pollutants, thereby making the binary distinction of season, suggested by Tim, problematic because within particular seasons, air pollutants have their own specific characteristics:

> If we are not to throw in another complexity here, we're not told anything about NO_2, as well[…]and that will have its own complex, annual cycles and variable interactions, or correlations, I'm sure, with Ozone and PM components (Chris, Liaison Meeting 20th January 2014).

Chris argues that the effect of season on air pollutants is non-linear, thereby problematising not only the distinction of seasons but the relationship between particular air pollutants and seasons as a temporal figure which can be manipulated by the epidemiologists. What the 'real relations' are is problematic because of the 'non-linearity' and complexity of air pollution relations, not only of the relationship between air pollution and health, but also of particular air pollutants in time and space. Complex relationships refer to 'non-linear' relationships between season and air pollution, and between different pollutants, which are not proportional, so that, for example, as temperature increases in summer air pollutants do not increase at the same rate.

Tim maintains, nonetheless, that for their analytical focus, 'the more general' increases or decreases of pollution levels over time should still be sought:

> Tim: I mean the question I want to ask, I mean we are being a bit driven, because we can see the results (Peter laughs) but some of these results look paradoxically a bit negative and we weren't really expecting it, so we are trying to think through patterns and how they vary across the country[…]what did we expect by season[…]because we've got a lot of things here we can look at, and we are trying to look at a lot of numbers, small numbers, which highlight statistical significance because they have a lot of power, but what would we have picked if blind to results.
>
> Chris: I think season becomes an issue because it drives emissions, climatology as well.
>
> Tim: Ok and what would you expect with seasons, so for example, with $PM_{2.5}$ would you have expected umm broadly the same impact [of air pollution on health] on each season, all positive with different magnitudes, or some positive and negative, what would you have guessed?

> Chris: well that [question] presupposes some health impacts in there, if you asked me more general question about what I would have expected with their concentrations, as you said before I would say higher $PM_{2.5}$ in summer, because you have contribution from secondary, in winter, maybe because you have a greater trapping of primary local sources, the composition may be different but the concentrations the same, but what impact this has on health, depends on what the causal exposure is between $PM_{2.5}$ and health.
>
> (Liaison Meeting 20th January 2014).

Chris indicates, however, that the epidemiologists are pre-empting causal relations between air pollution and health by playing with season as a classification, arguing that an increase in pollutant and related health effect is more complex than just a correlation, dependent on the specific nature of causal exposure as a complex, relational atmospheric interaction, rather than a pollutant-human interaction. The problem is, Chris argues, that even though the correlations between season and health effects can be used to make the claim that in winter a certain level of a pollutant is good for you and in summer it is not, within these patterns a more complicated story can also be told. Splitting the statistical analyses into just two seasons was, accordingly, problematic.

A solution to this pre-empting of correlations was proposed by Peter:

> I wonder if another component of this […] you can argue $PM_{2.5}$ is a different mixture in different seasons, but could you argue the same for elemental carbon? We have elemental carbon here and isn't elemental carbon elemental carbon, whatever season? (Ibid).

By studying just one component of the pollutant specie it was hoped that a better understanding could be gained of the complex relations between the air pollutant and season. $PM_{2.5}$ became a useful co-ordinate once again here because of its internal heterogeneity: Peter argues that, because elemental carbon is the same in every season, then the statistical techniques and the classification of season could be tested, because elemental carbon is always going to be constant across seasons. Here, air pollution data become a way to understand what seasons do to their visual analysis, and the meaning of seasons is used as a point of intervention, as a social and cultural construction rather than just a natural system:

> [What] I'm really trying to get at here is, whether we can say, at what points is there a transition in the composition, or the exposure patterns, and if so, at what point does that come, is it really about when the heating, power generated heating season kicks in or is it a much finer, or more complicated transition. What is the main driver? (Ibid).

The suggestion here is that seasonal changes are not 'in nature' but 'social and cultural' as the 'drivers' which influence emissions and therefore the character of air pollution at different points in the year. To elaborate his point, Peter drew upon the research carried out by 'the epi group', and the other epidemiological studies on air pollutants and seasonal interactions. 'Elemental carbon', as a component of the category PM, does not shift in seasons, although PM is considered a seasonally influenced pollutant, therefore just focusing on Black Carbon (another term for elemental carbon) could be a way to better understand the non-linearity of seasonal effects on PM more generally.

The correlations (short-term health effects) which the epidemiologists argue the graphs show (similar to the patterns which appear in the comparative graphs in Figure 2), are understood by the epidemiologists as 'not real' because they are being dominated by long-term trends. This means the dominant patterns in the graphs are not the pattern of interest, i.e. the short term effects of different levels of Ozone. For the epidemiologists, then, the issue is that the current classification of season does not enable them to untangle the relationship between pollutants and health and their variation over time and in relation to other spatio-temporal relationships.

Air pollution data became a way, then, to understand what seasons do to their visual analysis, and flagging the social construction of seasons was a point of intervention. Indeed, the epidemiologists talk about this problem in terms of 'drivers', as the forces which shape the kind of correlations which are being displayed in the current results and their visualisations. Tim points to the multiplicity of relationships which are visible, and therefore the process was one not simply about making visible but making *more* visible through 'picking' out the significant statistical relationships.

Materialising the 'right relations' : the visual effects of the spline and sine/cosine statistical functions

By splitting seasons just by summer and winter, it was proposed by the epidemiologists that better visible correlations could be possible, compared with the more common split of winter, spring, summer and autumn. 'Splitting seasons' was a statistical process where season was re-configured, involving the adding of numerical functions to the statistical model. Linear time can be broken down into different parts, as functions of the main time variable producing a time frame within which air pollution can be defined. This allows for greater flexibility in the representation of time, and therefore air pollution, by the model. As an inherently fluctuating phenomenon, air pollution is a variable which increases and decreases over time, and for this reason a function needs to be added to the model to account for this movement. The function allowing the model to better statistically represent air pollution.

The 'two season or four' negotiation resulted from the epidemiologists' initial finding, of an unexpected inverse relationship between air pollution and health. This was significant because it suggested that in some seasons air pollution is good for you, which goes against established knowledge about the relationship between air pollution and health. There are a number of ways of managing time in epidemiological research and I am going to discuss two of the options proposed by the epidemiologists: sine/cosine function and a cubic spline model. These are well-known statistical techniques and I used a published epidemiological methodological paper to help me describe the process. This is because the graphs used by the epidemiologists on WHAP were considered sensitive and 'work in progress', and therefore only to be shared within the project. For ethical reasons, therefore, this chapter switches between my own fieldnotes made at liaison meetings, where researchers talk about the preliminary results visualised in a series of graphs, and the results and discussions from a published epidemiological paper. As such in order to write this chapter I re-created the graphs (Figure 1-6) with STATA using an open access data set from a published epidemiology paper (Bhaskaran et al., 2013). I use the graphs to examine the methods of visualisation employed by the epidemiologists to supplement the discussions I observed within the liaison meeting. In addition, the act of re-creating the graphs was a form of participant observation, through which I came to understand and experience the process of inputting code and making visualised output.

The 'spline' function and the 'sine/cosine' function are two of the different ways of capturing the multiplicative nature of air pollution over time in the model. Its effect on the statistical relations can be seen in the graphs by the changing shape of the wave. Adapting the classification of season is primarily a visual concern, a practical method to enhance visibility of the epidemiologists visual statistical correlations plotted in the graphs:

> Yeah, I mean, just as a presentational issue, having four seasons obviously is quite a lot of separate columns [...] I mean if we can make the case for dividing the year in two, I think there will be some benefit in this simplicity, but of course that does not fit well with our standard definition (Tim, Liaison Meeting 20th January 2014).

The visualisations of correlations were a central part of epidemiological data practices. This was also evident in the literature, where papers covered methods for best representing things like 'non-linearities in the variable of interest' (Bhaskaran et al., 2013). Indeed, 'bad' visualisations were a key topic and the point of departure for further visualisation, and several methodological papers in epidemiology journals, such as the 'International Journal of Epidemiology', deal with the topic of making better visualisations of statistical correlations. Graphs are, then, visual forms which can be worked on through various statistical techniques, in ways that render the data points into, for example, a smooth pattern rather than a 'wobbly wave'[52], as depicted below:

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[52] Long-term patterns can be modelled more smoothly by fitting Fourier terms in the 'Poisson Model' (the kind of epidemiological model used by the epidemiologists on WHAP). These are pairs of sine and cosine functions of time with an underlying period reflecting the full seasonal cycle (i.e. calendar year), which can capture very regular seasonal patterns. A single sine/cosine pair will model seasonal variation in the outcome as a regular wave with a single (equally spaced) peak and trough per calendar year (the actual position of the peak and trough are guided by the data). However, harmonics (extra sine/cosine pairs with shorter wavelengths) can also be introduced which results in more flexible functions.

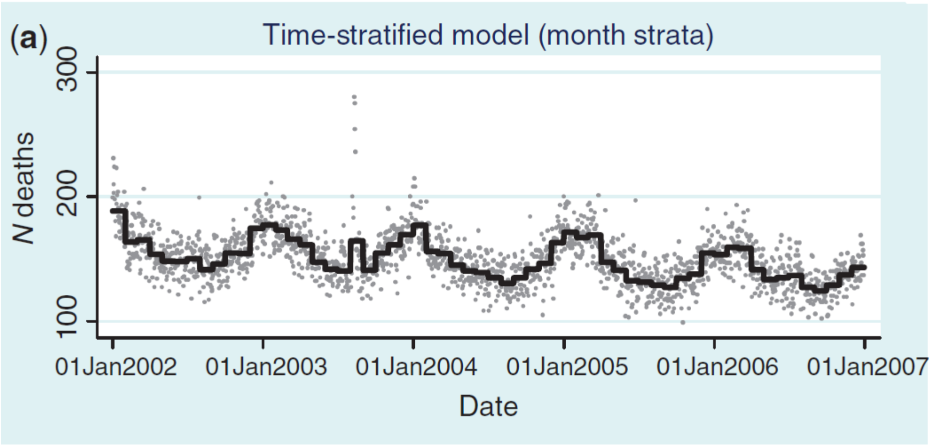

Figure 20: STATA run: a 'wobbly wave' (Data from Bhaskaran et al., 2013).

'The wave' is, then, a form itself, with different adjectives rendering the data patterns meaningful, playing a role in the trajectory for further modification. The graph is where air pollution as a statistical research object could be seen, materialised as patterns, lines and curves in the plotted data, and where the seasonal effects of air pollution on air pollution-health correlations could be studied more specifically. It is this latter shift which differentiates the epidemiologists and the environmental chemists on this issue, because health (in relation to exposure to air pollution), rather than air pollution, is the object of research for the epidemiologists.

The purpose of these techniques was to work out the role the classification of season (as four or two seasons) plays on the epidemiologists' visual correlations of air pollution and health (and therefore also the role season plays in the epidemiologists' statistical model). It was an experimental process:

> Tim: I mean they all do suggest that sort of pattern is deepest in winter peaks in summer. Something along those lines. So what would drive that? So, if you put it into sine/cosine terms or whatever, what we would learn? (Fieldnotes 20th January 2014).

The graph below shows what sine/cosine function does to the wave, in comparison to Figure 20 and the 'wobbly wave':

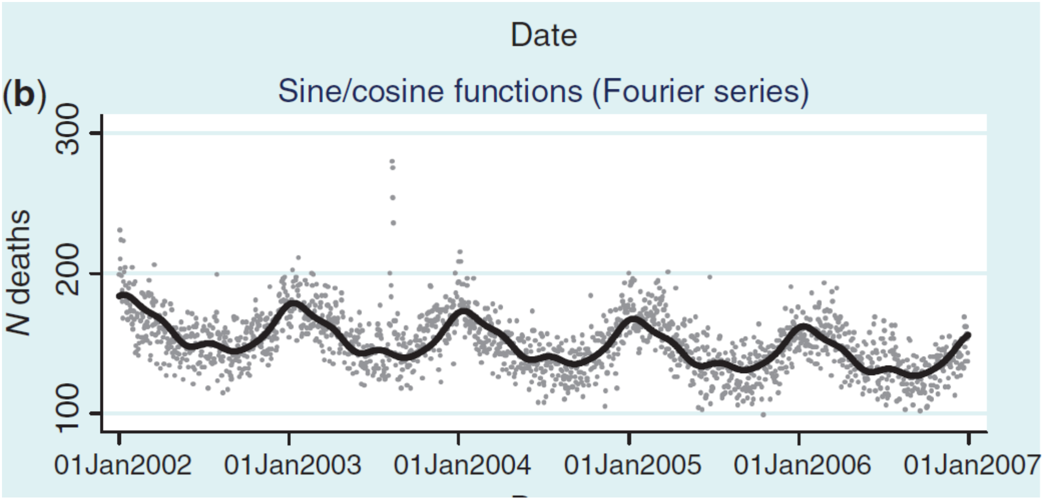

Figure 21: Stata run: Sine/cosine wave (Data from Bhaskaran et al., 2013: 4).

Long-term patterns can be modelled more smoothly by fitting sine/cosine as functions of time so that the model models seasonal variation in the outcome as a regular wave with a single (equally spaced) peak and trough per calendar year (the actual position of the peak and trough are guided by the data). The functions mean that the different data points are correlated as a smooth pattern, whereas ‘the actual position of the peak and trough are guided by the data’. Another option is the fitting of a ‘spline function’. This was considered superior to the sine/cosine terms because it allowed for a more ‘flexible curve’, where each segment of time has its own wave (called a knot). In this way, the splitting of time into seasons shapes the patterns in the graph in meaningful ways. In generating the spline basis, it is necessary to decide how many knots (join-points) there should be, which governs how many end-to-end cubic curves will be used, and therefore how flexible the curve will be: too few will fail to capture the main long-term patterns closely, whereas too many will result in a very ‘wobbly’ function which may compete with the variable of interest to explain the short-term variation of interest, widening confidence intervals of relative risk estimates (Bhaskaran et al., 2013: 1191).

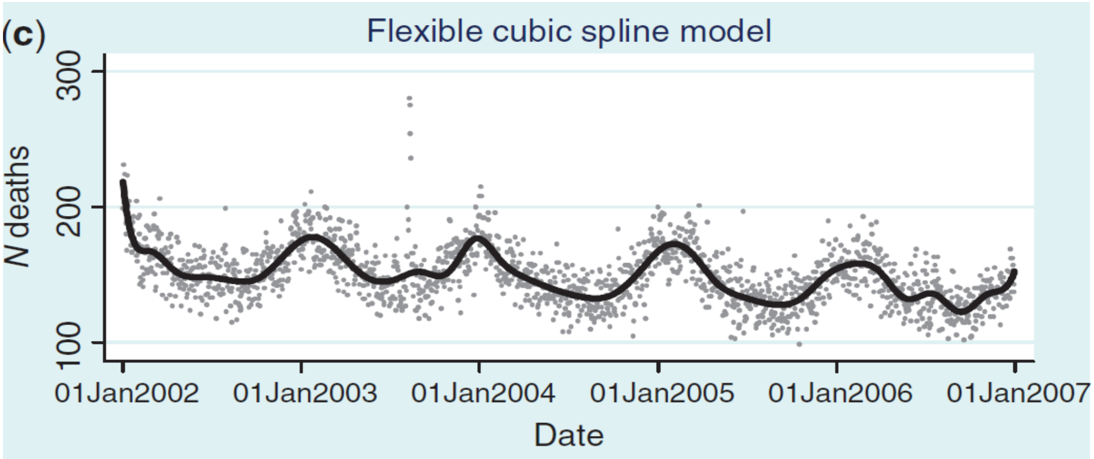

Figure 22: STATA run: Adapting the epidemiological model to measure 'the real relations of season' (Data from Bhaskaran et al., 2013).

The spline function works to shape the waves on the graph, so that the graph doesn't over-represent long-term patterns of air pollution and health. This is because time is understood as having multiple variables and the spline function enhances the multi-dimensionality of the linear time function in the first model (Figure 18). The concept of flexibility of the curve is interesting, for it points to the in-between process of statistical practice, and both the aesthetic and theoretical considerations involved in the playing out of these practices. Balance also returns here[53], because the numerical function is a balance of rendering the correct air pollution patterns visible, whilst leaving sufficient information from which an estimate of exposure effects can also be realised. If seasons become too complex in the model then it is difficult to visualise correlations with health.

If seasonality and long-term trends are controlled for using one of the statistical practices I have presented, then it is assumed that the long-term patterns are no longer apparent in the visualised data (Figure 23). When there are no waves then the data are understood as ready to use in the main analysis, studying the relationship between air pollution data and health data, and to see (quite literally here) whether

[53] An in vivo concept used by Craig, a modeller, to account for the multiple considerations involved in getting good modelled data, which I discussed in Chapter Four.

the health data can be explained by short term (because long term relations have been taken away by the statistical functions) exposure to air pollution.

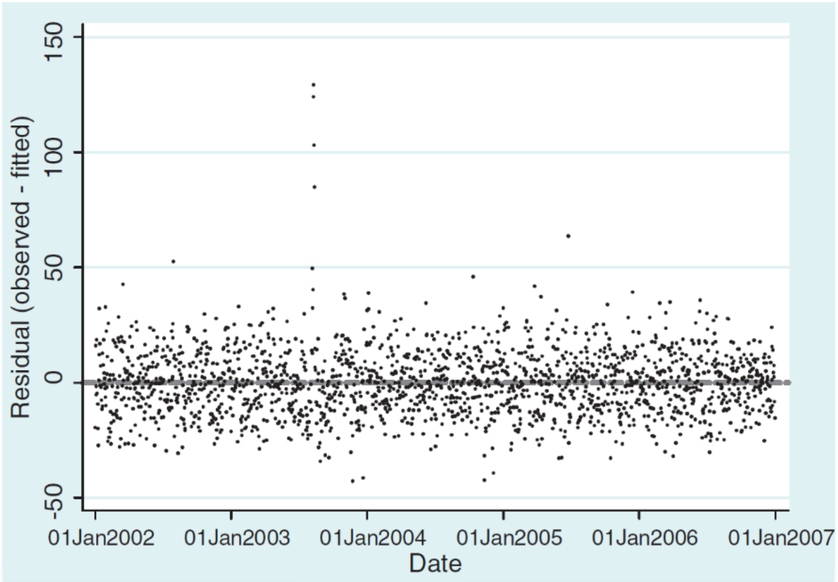

Figure 23: STATA run: An adjusted visualisation with no peaks no troughs' ('Residual variation in daily deaths after 'removing' (i.e. modelling) season and long-term trend') (Data from Bhaskaran et al., 2013).

These different functions were considered as producing a better visualisation of the relationship between air pollution and short term health. The functions added to the statistical model effected the shape of the waves on the graph because the different data points were temporally sequenced by the numeric function of sine/cosine or spline. The next step was to add the exposure of interest - health - to this model, and to examine whether the morbidity and mortality patterns can be explained by the remaining short-term variation of air pollution levels. However, in doing so, time related influences in the making of correlations between these two variables of interest are still problematic.

From air pollution to health: counting deaths that count

A common empirical anecdote used in epidemiology to describe the problem of measuring time in time series analysis goes like this: in a study which is relating 'ambient temperature' to 'hospital admissions' of heart disease, researchers found that admissions increased on days with very high temperatures, but several days after exposure there were fewer admissions than expected. This is explained accordingly: that 'vulnerable people' who were within days of dying may have simply had their death brought forward by a few days as a result of a high temperature episode.

This is a phenomenon known as 'harvesting'. The concept of 'harvesting' is used in epidemiology to account for the number of deaths 'which would have happened anyway'. This is a statistical problem which arises when applying time series analysis to grouped data, such as mortality counts. The concept is based on the assumption that the exposure can effect individuals whose deaths are 'brought forward' by 'a brief' period of time through exposure. Because these deaths don't 'count' as counts, statistical adaptations to models are needed to account for this and thereby reduce the quantified overall exposure effect. If harvesting appears to be present, the extent to which the short-term risk increases is 'cancelled out' and therefore reductions in risk at longer lags can be ascertained. As the anecdote depicts, the epidemiological model is shaped in a way that means what counts as a relevant death is embedded within it, through the lag technique, for example.

We shift now from the air pollution data to the health data. Harvesting can also be *seen*, as the graph in Figure 24 exemplifies. This graph of 'a lag' is used as a visual device by the PI in a one to one discussion I initiated on the topic of lags, as a way to show me what a lag does to the air pollution data. He explains that the problem is 'how to contain and constrain time instrumentally' in the statistical model. The problem of 'harvesting' on data relations can be seen in the sharp increase in number of deaths ('y axis RR increment per degree'), followed by a decrease. So, the risk is 'observable over a few days before declining to background level' (Tim, Personal Correspondence 9th October 2014).

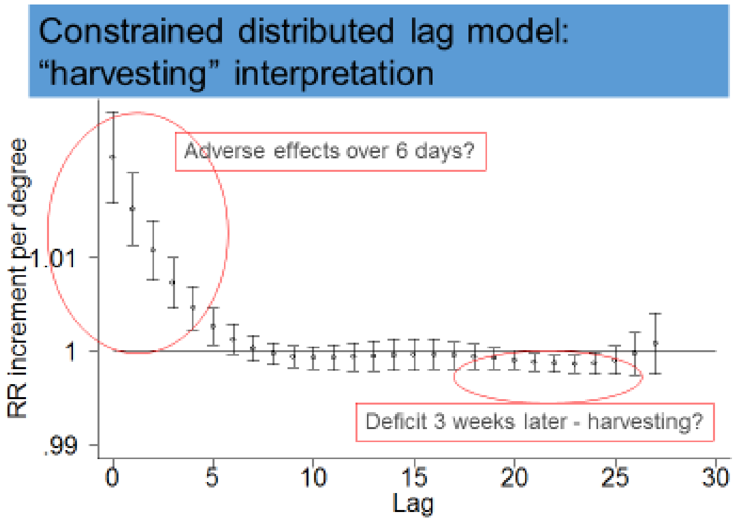

Figure 24: The visualisation of ‘harvesting’: time lag and its effects on mortality counts (Tim, Personal correspondence 9th October 2015).

The data are presented on the graph as a curve, which shows an increase in ‘relative risk’ (RR) on the day population exposure to air pollution is measured (though the linked air pollution and health data at a daily scale), and an increase in RR in the days that follow. That is the percentage of risk that remains as a result of exposure. The graph shows that from day five the RR is fairly constant, highlighted by Tim with the speech bubble with the key analysis inside: ‘that the adverse effects from exposure take place within the first six days of being exposed’ and secondly that the levelling off of the RR means that, ‘an effect can still be seen even a month after exposure took place’. In other words, the pattern is not showing the correlations of interest for the epidemiological analysis, the short-term effects of exposure.

This time effect is significant because it is health that is of primary interest for the epidemiologists. The delayed effects over time of air pollution on health means those deaths that accumulate get ‘wrongly’ measured by the statistical model, as relating to a particular exposure. The epidemiologists were not interested in the accumulated effects over time of air pollution on health, but, as I have already highlighted, on the short term, specifically within a day, effects of air pollution on health. The lag function is a way to ‘control’ for the health effects on a particular day from earlier days, which are not the result of the air pollution levels of that day:

> Lags just refer to the fact that high air pollution today (or a high or low temperature today) may not only affect disease events (deaths, hospital admissions) today, but also tomorrow (1-day lag) and the day after (2-day lag) and so on. In other words there is a time interval between exposure and some of the adverse health effects occurring. This is generally shown as a graph of risk (on the y-axis) against lag (on the x-axis) in which the adverse effect (i.e. raised risk) is observable over a few days before declining to background level (Tim, Personal correspondence 9th October 2015).

The focus for epidemiologists on WHAP was the quantification of correlations at a daily scale. As a result the statistical model needed to account for the death counts which are measured on a given day, but which do not result from exposure on that day. Bringing in 'a lag' enables a 'more elaborate' statistical model, because it means making the visualisation of time non-linear: 'the lag is added to the epidemiological statistical model, which, implicitly works on the log scale, in linear and accumulative time' (MSc course notes: 'Environmental Epidemiology', September - June 2011/12). Visualisation is a metaphor which works in two ways here, both as a method of testing out statistical technique and thereby making statistical practices visible, but also as constructing the research object itself, where health becomes visible in a statistical relationship with air pollution. Lags, like seasonal variation, were described as a technique which make air pollution and health relations more visible. The series of graphs below show the process of integrating a time lag into the model (Figure 25).

Adding a 'time lag' to the statistical model allows the effect of a single exposure event to be distributed over a specific period of time. Using several parameters to explain the contributions at different lags provides a 'more comprehensive' picture of the time-course of the exposure-response relationships. By creating time-shifted copies of the exposure variable and including them in the model, it becomes possible to explore the association between 'outcome today and exposure on previous days'. This enables one to see mortalities from exposure on days one and two following exposure to the given concentration of the pollutant under study, highlighted with the top graph in Figure 25 (the graph below shows the measure of risk of dying from exposure, in terms what counts as a death from air pollution). However, there is a third component of the model used by the epidemiologists that is that the distributed lag model is a *constrained* distributed lag model. Constraining the model means literally making the model assume that events happen all on particular days or over a series of different days. This means that since a health effect from exposure can be seen one to three days after exposure (See Figure 25 (b)), this can be assumed as the exposure effect and modelled as one constraint - telling the model to assume all exposure effects take place within this time frame - then days three to five are constrained by telling the model that all health effects on these days would have happened anyway.

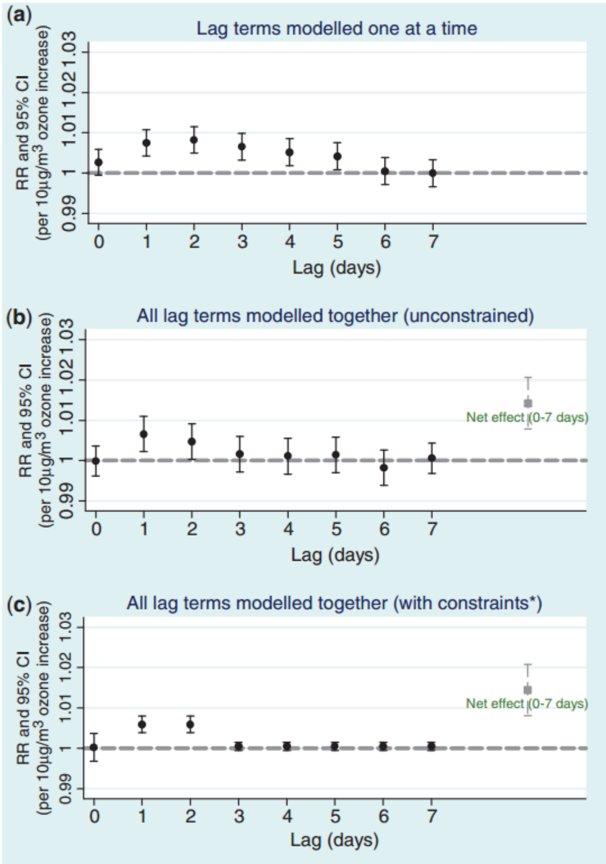

Figure 25: STATA run: ‘A constrained distributed time lag’: the process of building lags into the model (Data from Bhaskaran et al., 2013).

By accounting for different time lags, the epidemiologists get a sense of the different kinds of relationships in the data. So, for example, the average plots can also be used to supplement raw scatter plots and draw out patterns. Such plots effectively smooth out the raw data by averaging over a fixed number of adjacent raw data points (Bhaskaran et al., 2013). Although this sounds rather complex, what the description shows is that statistical techniques are used to visually represent data as a process of further *revealing* correlations of interest. Thus, it was the sequential nature of producing these graphs that offers insight into data's statistical meaning. These patterns relate to correlations which represent the causal relationships between air pollution and health, which the epidemiologists were trying to understand and quantify. This made the relationship between air pollution and health visible and something to think with, before proceeding to research the 'true relationships'.

The statistical software and the appropriate statistical commands are ways to make the data 'present itself' in an analytical way. The agency of data is described in the notes of an MSc module I took in environmental epidemiology, as allowing the data to 'speak for itself': 'the effect of adding both a sine and cosine term is to allow the data to decide where the peak and trough should be - the 'phase' of the wave' (MSc Module notes 2011/12, London School of Hygiene and Tropical Medicine), where the signal of air pollution-health will become visible as 'trough associations' (Fieldnotes January 20th 2014). How data look in graphs is a fundamental part of understanding data as a statistical research object, functioning as a trajectory along which correlations between air pollution and health are further revealed.

From air pollution to health

This brings me back to the beginning of the chapter where Tim talked about the epidemiologists' concern over the surprising patterns in their visual results. I've shown how a series of statistical techniques enabled the right patters to emerge, and surprising patterns investigated. In order to work out a meaningful relationships between air pollution and health the epidemiologists draw upon the external relation of time, playing with this taken for granted natural reality in ways that were fruitful for understanding the complex relationship between air pollution and health in time. This process delineated wrong relations from right relations, and in doing so shifted the research object from air pollution to health. As a result of these iterative visual practices the epidemiologists' constructed new relations between data, shaping the object of research and generating new meanings and further complexities.

GIS was described as a data generating and data analysing tool by Tim (PI, Liaison meeting, 23rd June 2014). However, the work of data linkage is also, I argue, constitutive of the generation of data and the object of research. The actions arrange data in a way that facilitates its analysis, whilst also constructing statistical relations between air pollutants and health events. This very literal bringing together of data is a process which makes particular relations of air pollution visible on the re-constructed 5x5 km grid squares: ‘so within the 5x5 km grid square you scale [all the data] the same, although these processes all happen outside of this scale’ (PI, Interview June 2014). Here, scale, and therefore what comes into focus, is considered a technique of making air pollution- relations visible as a tangible research object. This was done by containing air pollution within the spatial scale of the grid square. Indeed, it was the grid square which became the research scale rather than relating to a spatial scale ‘out there’.

The linking of data did not only ‘reveal’ relations - distinctions, patterns, information - of air pollution to the epidemiologists, but was also the means by which visual and theoretical insight emerged. The discussions between the two research groups was also informative for showing the ways in which the ‘real’ and what is ‘constructed’ was stabilised in that particular instance. Tracing the deliberations around how to classify season illustrated the kind of work involved when trying to measure and contain ‘the natural world’ for scientific research. The taken for granted nature of seasons as ‘out there’ was challenged by the epidemiologists, and in doing so, so was the notion that data are natural information. Indeed ‘seeing’ patterns in data also involved checking whether these patterns are ‘of nature’ rather than the product of social and cultural constructions of time. Season, in this sense, was a hybrid, a natureculture (Miele and Latimer, 2014), which functioned as a starting point for further iterations of data as visual forms through various statistical techniques. This process of visualisation was a way for the epidemiologists to familiarise themselves with data as informational and material forms, but also as socially and culturally imbued objects of research.

What comes into focus, and the scale that this focus brings into being, was a new object of research: ‘health’. The concept of focus works well with visibility, in that it can mean both emphasising something and fixing something, making its contours clear and well-defined. Air pollution became visible as multiple data-in-combination, materialised in ‘linked data’ before being related in a way that made epidemiological data on health possible. In practice, then, ‘instrumental embodiment’ was also extended to ‘data embodiment’ (Myers and Dumit, 2011), where the act of bringing data together meant researchers developed new capacities for seeing, by visualising two different phenomena in one technical space. By familiarising themselves with data, model adjustments were then carried out so that the ‘right correlations’ of air pollution and health ‘made themselves’ visible too. The epidemiological work relied on this push and pull, between data’s agential role enabled through the creation of conditions through which data were able to ‘speak’ (expressed as graphic patterns), and their role as practitioners, manipulating material data and visual representations of these correlations of interest.

Summary

In this chapter I trace the ways in which team members’ talk about, use, exchange and re-use different data of air pollution. By analysing the linking of different data as a technical, epistemological but also performative process, I emphasise both the material practices which combine multiple data and the analytical capacity this grants for epidemiological knowledge production. I introduce the process of over-laying, the visual forms the over-laying of data make possible, and the way in which ‘time’ became the figure through which correlations are made. Throughout these discussions I emphasise the analytical and conceptual frame of ‘health’, and the work involved in assembling data to achieve a visual correlation of air pollution data and health data.

Data-in-combination, in practice, involves the gathering of data in one conceptual space through a practice called over-laying, these layered data are then visually analysed through the GIS tool, as a technologically mediated way of seeing multiple types of data, but also different kinds of phenomena visible at once. This ‘all-at-onceness’ functioned as an analytical and conceptual process, which made the invisible visible, and thereby the relations between air pollution and health possible to untangle as epidemiological, statistical claims. At this point, it may seem like the research object is once again stabilised, through the refining of the epidemiological model and its enhanced representation of correlations between air pollution data and health data. However, these visual forms were in fact the starting point for further iterations. In the next chapter, I follow the movement of these graphs into new spaces of practice and examine their capacity to speak to different kinds of scientific audiences. These data correlations are valuable because they demonstrate a relationship between air pollution levels and negative health effects. This finding is of value to those outside of science: in the case of WHAP, for those in science and policy working in the field of environmental health. I will start by examining the ways in which these visualised data relations functioned as a shared point of interest between the researchers on WHAP and invited policy makers at the first stakeholder meeting.

Chapter 7: From data to evidence: selecting air pollution, extending the scientific network

Introduction: The boundaries of 'science' and 'policy'

> When you are sitting there doing your models and have produced all these pretty plots and you think well what does that mean, is it important? Do policy makers want to hear about it? (Elizabeth, Interview October 2011).

> The project is not just about serving needs of policy makers. Advancing science for own sake is important. It is an academic project. We hope it directly bears on questions of people in air pollution management, but, I suppose, reflecting my own biases, you have to get science right, that means we have to try and do the best we can with the methods we know, to be clear what we can and cannot say about relations between exposure and outcomes (PI, Interview October 2011).

I start this chapter by pointing out that the visualisation of data, which I discussed in the preceding chapter, was not a point of closure but in fact a point of departure. As the quotes above highlights, plotted data are of interest to issues outside of science too, such as health. So far I have traced the ways in which the research team managed problems generated by different data practices, and how visualisations were a useful method for exploring both. Moving on from an examination of the ways in which visualised data become the object of research (Chapter Six), here, I show that visualisations of data were also instigators for further iterations, primarily in terms of functioning as potential evidence for policy making. Once again, visual forms constructed a performative space through which air pollution relations were made material and tangible for a new audience: policymakers.

I followed the movement of these visual forms as part of the formation of a science-policy interface. The opening two quotes demonstrate the perceived tension between the domains of 'science' and 'policy' by researchers. In the first quote, Elizabeth, a modeller, explains that it is important for their modelled output to be considered in light of policy interests. In the second quote the PI explains that they, as scientists, need to be clear and careful about what kind of claims are made from their data. The final work package in the WHAP project involved this movement 'outside' of science, primarily though the holding of a stakeholder engagement meeting.

The stakeholder engagement process involves the holding of a science-policy meeting, the making of a decision about which policies and 'future scenarios' the WHAP project should produce data on, and subsequently the running of the scenarios on the CM-WM model. The plan was that the results from these future scenario runs would be used in a second meeting, during which invited stakeholders would then put a 'weight' (as value which is out into the mathematical model as a numeric weight, on a scale of 0-1) on which policies are most important, and these will be run to make 'the decision' as to what policy recommendations the project ultimately makes. This decision –making process would be carried out using a mathematical model for the 'weighing up', and the subsequent prioritisation of, different polices for reducing and managing air pollution. However, this chapter is based on the first stakeholder meeting and therefore on the making of initial selections of which air pollutants, model runs and policy outcomes the project will focus on.

The series of graphs in Figure 26 were used as a point of departure for the first stakeholder meeting held at The Institute in December 2013. The impacts of policies on emission levels and thereby air pollution concentrations cannot be seen[54], and the colour coded maps make the effects of air pollution visible and tangible for those present at the meeting. The model runs different scenarios by manipulating the emissions sector data which are input into the atmospheric chemistry model (CM-WM model). The different emission sectors are mapped according to a pollutant released by the emission type, these are classified according to a widely-used air pollution emissions classification system: 'Selected Nomenclature of Air Pollutants' (SNAP). The visualisation of changes in emissions and their effect on health (See image on left of Figure 26), and the concomitant tangibility of particular policies'

[54] In Figure 26, dark blue represents the least amount of air pollution and the light blue to white as increasing concentrations (and then through to a dark red which would signify very high concentrations). This idea being that the policy makers comment on the data produced by the WHAP project in terms of their own priorities.

effects on air quality was also a way to share data. Both these images opened up a space for participation in science and engagement with policy.

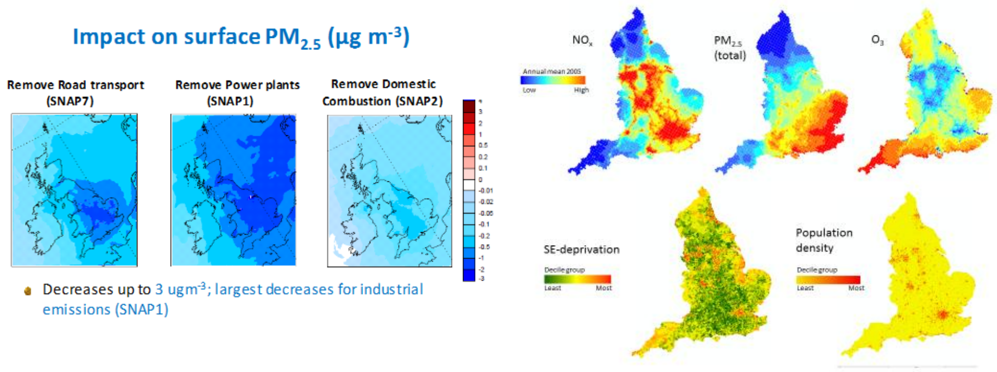

Figure 26: Colour-coded maps of ‘the future’: visualising the dynamic effect of changing the emissions data input in the CM-WM (Stakeholder meeting 3rd December 2013).

As I will go on to show, when talking about data practices in terms of policy, researchers used terms such as ‘reducing emissions’, ‘scenarios’ and ‘impacts’, rather than, ‘representation’, ‘validity’ and ‘good data’, which previously orientated discussions. I use this shift in ways of talking about scientific output, from discussions of data to discussions of evidence, as a starting point to explore what this semantic shift meant in practice. By tracing the articulated ‘engagements’ with policy makers, I focus on the articulation of boundaries between ‘science’ and ‘policy’, and in doing so, how their symbiotic relationship mobilised ‘data as evidence’.

Considering how scientific knowledge bears on its potential users is a common component of contemporary research protocols, encouraging what has been described by Webster as the making of ‘self-conscious science’[55](2002: 453).

[55] Although Webster is describing the tension between two different forms of ‘self-conscious science’, that of the involvement of ‘lay knowledge’ in decision making processes, compared with the systematic monitoring and forms of assessment in health policy, the concept highlights the tensions within this move, which reflect what I found in my own research, between the self-conscious recognition of

Engaging with non-scientists implies that science is governed by social and political agendas, and the PI grappled with this in his reflection that it is about being relevant but primarily about 'getting the science right'. Thus, at the same time, the process of engagement was considered as risky by researchers, and thereby the relation between science and policy was also made distinct from science, formalised by a mathematical model for analysing decision-making. The tension can also be expressed as a problem of knowledge flow from science to non-scientific realms, specifically, from local, situated scientific claims about air pollution to the more Universalist kinds of claims about air pollution that policy makers are often considered as making.

The science-policy interface has generated much interest within the social and cultural studies of science, with studies examining the co-production of science and society (Jasanoff, 2004), and the playing out of science and policy relations in different instances, from the construction of credibility between advisory bodies and their audiences (Hilgartner, 2000), to the mutual understanding that evolves between science and its publics (Shackley and Wynne, 1996). Such research highlights the ways in which scientific knowledge is not only generated in scientific laboratories (Latour, 1983), but in different kinds of spaces beyond the laboratory (Wynne, 2005). In this chapter, I am not interested in the mutual modification of science and society, or indeed science as policy, but in the construction and performance of an interface between science and policy - the relation itself - and what it does to the material output of scientific work.

I am going to trace this process by examining how scientists and stakeholders and scientists *do* stakeholder engagement, and therefore participate in the modelling of socio-technical relations, which frame how air pollution is conceived and anticipated as a political issue. As Mol suggests, the real is implicated in the political (2014) and the process of decision-making contributes to how an issue like air pollution is configured and materialised through scientific data. By relating data to policy problems, and thereby grounding them to the world, these data become potential

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pluralism and uncertainty at the same time as the need for closure of choice and certainty (2002: 453).

evidence, tied to particular questions and orientated around particular action. In this process, some air pollution relations are displaced by other air pollution relations. Indeed, modelling the future is political, in that it materialises air pollution as an environmental health issue and, in the empirical case of this chapter, used as a way to model its future (Edwards, 2010).

‘We are doing science *with* policy implications, not science *for* policy’

As the quote in the heading illustrates, the PI of WHAP, Tim, was quick to describe the project’s relationship with policy as one that was ‘engaged’ with policy concerns, whilst retaining a level of ‘objective distance’. Gieryn (1999) suggests studying the science/ non-science distinction as a conceptual resource, arguing that, by studying how these distinctions and boundaries are used flexibly and resolved contingently by individuals and groups we can learn more about science and its ‘others’. However, Gieryn assumes that these domains of science and non-science are relatively stable, and, although considered flexibly, a marked distinction remains. I found the distinction between science and non-science, represented here by the stakeholders, difficult to differentiate in practice.

The pros of cons of linking science to policy, with the aim of ‘having an impact’ and ‘making a difference’, and the considered risk of mistranslation and reduced objectivity was a tension drawn upon by WHAP researchers, as highlighted in the opening quotes. As a public health project it was often assumed that the epidemiologists were the means by which the project might make an ‘impact’. The nature of the WHAP collaboration, as oriented towards health, was important to researchers because it provided their research with ‘wider meaning’, and was a way to evidence impact, which was also significant to the economy of academic research funding:

> Our previous experience shows a clear need for engagement with stakeholders and policy organisations at the start and at regular intervals through the project. The first workshop will be the most important as it will allow us to shape the detail of our work plan at the outset to be of most benefit to end users; for example, discussions at this workshop will identify aspects of air quality policy likely to be most important in the UK context from which we can develop appropriate air

> quality policy scenario assessments. We will also present the elements of our [decision-making framework] for comment and refinement to be most useful to policy makers (Pathway to Impact Plan, WHAP funding bid 2010).

There were two main reasons which team members gave to explain the importance of this relation with policy. First, that engaging with policymakers, as the potential users of scientific work, was a way to ensure that the work of the project did not stay in a 'policy vacuum'. The protocol states that the output of the project's findings needs to be useful and useable and to achieve this requires communication and dialogue with the alleged 'end users'. Indeed, communicating with stakeholders was strategic, and something 'we said we'd do' as 'part of the funding call'. The act of relating to policy, then, reflects part of the contemporary character of academic research and the importance of making a measurable impact, in terms of tangible social, environmental and economic benefits.

For policy makers to be able to use data as evidence, they are required to work on data so that it carries particular types of information and a level of use-ability for policy making. Indeed, making data have the right kind of capacities was described in terms of 'degrees of certainty', which were understood as relating to the different thresholds which are required for evidence making:

> An important element will be the use of 'post-processing' of the exposure validation data to examine the uncertainty in the evidence and its potential bearing on policy recommendations (Project Protocol).

However, accounting for these new thresholds of certainty necessitates reducing the integrity of scientific claims. The stakeholder meeting was an opportunity to balance these different requirements. As the PI explained, it was about achieving a balance between being useful for policy makers and being useful for academia: 'The project is not just about serving needs of policy makers, advancing science for its own sake is important: 'it is an academic project' (Interview, October 2011).

Communicating with stakeholders was therefore strategic. Allowing data to travel to policy makers was considered an important relationship because it fulfilled the duties of scientific work according to those that fund it. The relation with policy was also a carefully policed boundary because the integrity of scientific knowledge and evidence based policy relied on it. As a result, I argue that the science-policy interface was performative of normative conceptions of science and policy as distinct domains of knowledge and practice. The shift from producing information for science as data, to producing information for policy as evidence was realised through the stakeholder engagement process. It was this distinction between data and evidence, and its enactment, which made science and policy separate. I found that the act of engagement was prescriptive, but also one that had material effects, enabling the potential movement of data from the scientists to policymakers.

The second motivator was related to the former, in the sense that engaging with stakeholders ensured that scientific work related to health. One of the central motivators for engaging with stakeholders was the unquestioned value of 'health' for many within the WHAP team. Relating scientific data to the task of responding to the health effects of air pollution was recognised as a socially responsive and moral driver for doing science. Generating 'meaning' was understood as making a situated scientific claim speak to policy in terms of 'health' interventions. As one researcher said:

> If you are demonstrating there is a health impact then it is logical to say what we are going to try and do to ameliorate that health impact. So, yes I think there is a strong angle to look at policy interventions which therefore means sort of gazing into future about what you could do to ameliorate it (Chris, Interview October 2011).

The logic of moving data along the science-policy network in order to make evidence was governed by a concern for linking scientific work to 'real problems' enhanced with a human and moral sensibility. The PI described engagement more like a transaction between science, as the producer of knowledge, and policy makers, as the consumers. However, this relationship was also productive because knowledge was shared and potentially re-used. This productive relationship was, claimed Elizabeth, about allowing data to travel and 'do' something else, as the opening quote states: 'what does that mean and is it important?' Here, Elizabeth comments on the movement of modelled data to policy makers through the re-use of their data by the epidemiologists. This movement enables the data produced in the situated practice of modelling and simulating to say something more in terms of 'health impacts', which 'adds meaning' beyond the situated practice of producing and interpreting data. Indeed, as Chris, an environmental chemist elaborated, relating to public health is a tangible output of their work:

> I would say that one of the things I like about this part of my work and this project is that it has a very clear public end point in terms of public health, and trying to understand public health and tying to derive policy actions that will improve public health (Interview 12th November 2011).

Meaning, here, is about making a more general claim than data can provide, which requires extending the scientific network and producing evidence. It also suggests that new realities are made beyond the laboratory (Knorr-Cetina, 1999), made sense of because health is an unquestioned natural good. Indeed, public health is one of the main ways in which knowledge produced by the WHAP project is understood as having potential societal value, which, during the stakeholder meeting, became as important as the science itself.

The stakeholder meeting becomes a space where 'scientific' knowledge and 'non-scientific' knowledge is made symmetrical, and where situated knowledge can be made less specific and more general. This means that different kinds of knowledge on air pollution can be considered on equal terms, as both legitimate forms of making knowledge claims about air pollution. Indeed, policy claims were given equal weight (quite literally in the decision making technology, which I will go onto discuss) to scientific claims in the context of decision making.

Mobilising the science-policy network

Organising and carrying out the stakeholder meeting was a way to bring together already established contacts and colleagues together. Indeed, the science-policy network was in many ways expressed through the holding of the stakeholder meeting. This became apparent to me at the start of the stakeholder meeting, when I realised invited stakeholders seemed to already know each other, picking up on previous conversations, ongoing debates and past and future collaborative work:

More stakeholders arriving, it is clear that people already know each other when they greet one another. No name badges are required either, which shows that people must already know one another by name and affiliation. Team members also seem to know many of the invited stakeholders by face, only having to introduce themselves briefly (if not simply to distinguish themselves as ‘scientists’) and connections quickly affirmed with a handshake. Little effort needed to encourage conversation, the hoped for ‘dialogue’. Tim starts to ask everyone to quieten and settle to get the meeting going (Fieldnotes, stakeholder meeting 29th November 2013).

I am going to briefly discuss how stakeholders were chosen and invited by the project, and how their roles were formalised in the decision-making tool. This is important because who is invited shapes the kind of air pollution relations made salient, and how these inputs are organised demonstrates how these air pollution relations become materialised as prescriptive actions.

Who are the stakeholders?

‘Who counts as a stakeholder?’ was not completely clear at the outset of the project. In a conversation with Francis, a mathematical modeller based at The Institute who led this final work package on stakeholder engagement and decision analysis), about what sort of organisations are going to be invited to the meeting he explains that ‘it is to be decided, but certainly people from the policy sector’. He elaborates on this, stating that he intentionally uses the word ‘stakeholder’ interchangeably with ‘policy makers’, noting that this distinction is not the case for all contexts, but maintaining that policy makers are likely to be the stakeholders of choice for the WHAP team. In the weeks running up to the stakeholder meeting discussions, ‘who to invite’ was a topic on the weekly agenda. Eventually, however, invitations were sent out by researchers to policy makers in their related field. The completed list, which was then circulated among team members, included the individuals and organisations with whom team members held well established working relationships. On questioning this process, because the invitation process seemed to happen without me knowing, I asked Francis to explain how invitees were decided upon:

[…] we had a stakeholder meeting for that [baby WHAP] project. Elizabeth took the initiative this time and started with the list of stakeholders/policy makers who attended the meeting of the first project (about three years ago). She naturally added/replaced some names from the same organisations. In addition, John

> suggested his contacts from the Department of Energy and Climate Change and the Department of Community and Local Government but I do not think they came[...]You are correct, there was no discussion dedicated to this topic in the sense we went through name by name. Most of the correspondence about the names was done off-line and involved mainly Elizabeth and me. The fact that we had a contact list form a previous collaborative project helped a lot (Francis, Personal Correspondence 29th January 2014).

This description of the process of inviting stakeholders to the stakeholder meeting also exemplifies one of the ways in which the science-policy network is made and mobilised, and thereby the WHAP project, among others, sustained. Stakeholders include ex-colleagues who work, both as scientific researchers at The University, whilst also affiliated to organisations which use research to shape policy by, for example, contributing to policy reports for the EU and IPCC. Stakeholders also included representatives from government departments such as DEFRA, the Climate Change Commission and Public Health England. Such representatives are firmly established contacts built on previous work and research projects in related fields. It was, then, 'usually down to a few individuals' (PI, Interview 14th November 2011), many of whom I became familiar with by name because of frequent reference to them in weekly liaison meetings. For example, one of the 'key stakeholders' on WHAP, who was also an insider to the project - attending collaborator meetings and frequently in touch with the more senior researchers on WHAP - was once described to me as 'a stakeholder interested in the science', but one with policy legitimacy, 'formerly in a senior positions at DEFRA' (Fieldnotes 7th December 2013).

Who counts as a stakeholder on the project, and therefore as the proposed end users of the WHAP project's research, highlights the ways in which researchers actually generate wider meaning through their work in practice. The process of carrying out stakeholder engagement resonates with an idiom of 'co-production' (Jasanoff, 2004), because scientific knowledge of air pollution and policy knowledge of air pollution is potentially produced *together* by researchers. The logic of moving data along the science-policy network in order to make evidence is governed by a concern of linking scientific work to real problems, rather than them remaining in a vacuum. In a way, the stakeholder meeting was an opportunity to think about the ways in which air pollution, as mediated by policies, is experienced by 'the public'. It was not, however, considered to be about ‘public participation’ or engaging with the public to inform science with lay conceptualisations of air pollution. ‘Impact’, instead, was hypothetical and prescriptive, focused on the issues of implementing policies in practice:

> [Our] two main mechanisms for engagement will be through (i) a set of three major workshops open to all stakeholders in air quality and health measurement, modelling regulation and policy making […] and (ii) a policy focused Advisory Group [of science and policy experts) (Project Protocol).

Engagement with stakeholders, then, was done informally, as well as the official meeting carried out by the project in December 2013. As the PI explained to me ‘they [already] have quite a lot of contacts’ with government departments and policy making bodies such as the Health Protection Agency (HPA), Department for Health (DH) and Department for Climate Change (DECC). These ‘scientific’ and ‘non-scientific’ relations seemed to play out during the stakeholder meeting, rather than being established. Instead, distinguishing between these science and policy was formalised and embedded within the decision-making tool.

A mathematical model for decision-making

The process of making credible, useful and responsible science is a complicated process. As the PI reiterated, it is important, as scientists, to primarily ‘get the science right’, a process considered as distinct from the values and perceived ‘subjective’ shaping of policies. The decision-analysis tool functioned to make scientific evidence and policy choices distinct and bounded, and thereby aiding the movement of data into evidence. Francis, who built the decision-making model, describes this process:

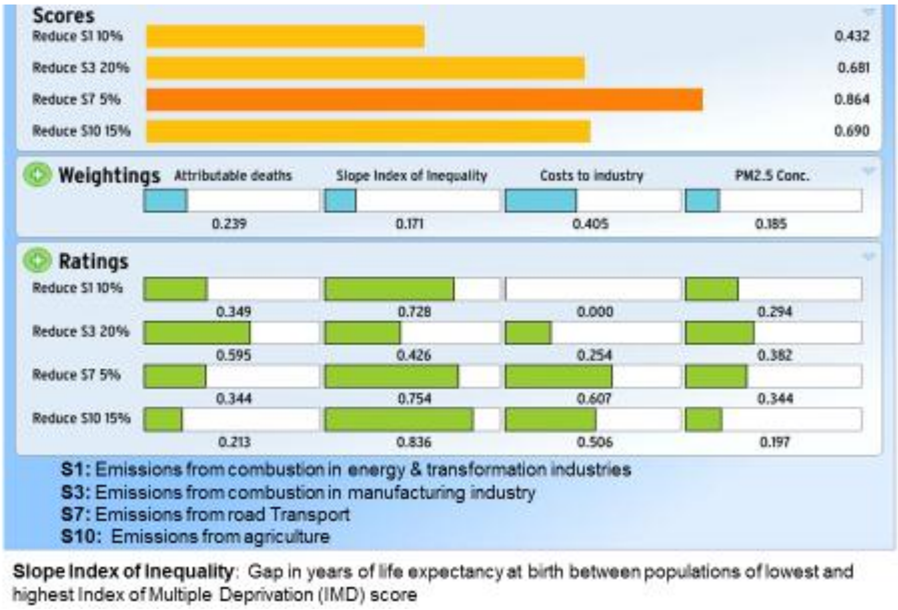

Figure 27: 'Presenting the MCDA tool in WHAP' (Shared Presentation, Team meeting 18th November 2013).

> [Francis moves the cursor] Well what I was going to show is, for example, how if you move this, and then these weights move this (as the weightings change the orange bars at the top of the screen, 'the scores' change)... so you can see how one changes things [the scores]...when you change the evidence, and you change the weight, the policy options which scores the highest changes... and the ratings are what the modellers bring, the calculations, and the weightings are what the stakeholders in the 2nd workshop decide on (Francis, Fieldnotes 18th November 2013).

Francis emphasises the inter-dependent components of the decision-making tool and how these technical processes ease the process of translation from data to evidence. It was considered a functional system because 'if all information were available it is capable of showing what policy options are best'. A decision was, then, assumed to be rational, in the sense that it was deemed equivalent to the information available. Francis tells me that if all information is available then choosing one policy option is possible, because criteria account for 'uncertainty'[56], which then gets put into the

[56] Uncertainty, here, relates to available information, not only in terms of data, but on the ways in which air pollution is measured and, therefore, on how data is produced. For example, the emissions and sources of air pollutants are recorded and what kinds of effects of air pollution can be measured. These different components of information relating to air pollution are required in order to construct an environmental health problem.

tool alongside certain knowledge. During the meeting, stakeholders were interested in the role they play in the modelling of a decision:

> So, different policy makers have different views on 'criteria' [how to judge policies] and it is [the decision-making tool] sensitive to these in ranking the policy options. The policy options are also to be discussed, so the aim of this meeting is to look at the criteria which compare policy options. The impacts will be measured by models and experts and the ratings are the underpinning science, the contributions of the project. [One stakeholder interrupts asking if the weightings are subjective] the weightings are produced by you the experts, explains Francis (Fieldnotes, Stakeholder Meeting 31st November 2013).

Francis also described more specifically how 'the science' was shaped by the tool:

> [T]he science needs to be normalised, for example, for 'attributable deaths', say there are 20,000 [attributable deaths] this would be 1 and anything under [20,000 would be weighted] between 0-1, because they are different 'units of the weights" ... [and in terms of the weights] you ask people and decide on weighting and look at the impact on weighting (Ibid).

The conceived subjective process of using data as evidence for the making of decisions about which air pollutants are priority was managed by scaling it numerically as 'a weight'. The performance of making a decision at the stakeholder meeting was further rationalised by this process of measuring the stakeholders' 'qualitative' input. The process was understood by Francis as a way to take into account lots of different interests and viewpoints, 'because health is not the only criteria by which to judge scientific evidence'.

I suggest that, by using the decision-making tool, the role of the policy makers could be accounted for and made transparent. This meant scientific integrity was then ensured because it would be made technically distinct from the input of stakeholders. The policymakers' selection of 'criteria' to judge policy options was a way to construct standards by which policy options (as made possible from the SNAP sector model runs) could then be measured. The decision-making tool, then, was a device for policy makers, but also a boundary making tool for scientists. As I highlighted, the science- policy network was constructed in and of the WHAP project, yet the decision-making tool and the stakeholder meeting performed a distinction between science and policy. This led me to try and understand what making explicit these relations did to the status of scientific data.

Making data travel along the science-policy network

2001 UK Emissions (Gg/yr) for different sectors

Scenario	SOx	NOx	CO	VOC	NH3	$PM_{2.5}$	PM_{10}
BASE	1110	1696	5350	1442	337	97	68
-100% SNAP 7 (road transport)	1108	1113	2831	1289	324	69	58
-100% SNAP 1 (combustion)	290	1109	4372	1055	331	81	57
-100% SNAP 2 (non-industrial combustion)	992	1601	4579	1365	335	79	55

- Long-lived species -CO_2 and CH_4 emissions not included (for CH_4 we use fixed concentrations)
- Primary PM is divided into fine and coarse EC, OC and remaining PM
- Emissions from other European countries could also be modelled
- Options to reduce emissions of single or multiple species in these sectors e.g. 10% of all road traffic emissions, 10% of only NO_x from road transport

Figure 28: Scenarios and the measures of air pollutant concentrations (Presentation slide from stakeholder meeting 3rd December 2013).

In the process of stakeholder engagement, of planning the meeting and carrying it out, scientific output was considered under new research conditions. This is because making decisions about what specific data were to travel as evidence was considered as subjective. The way in which the PI commented on this process was noticeable for its detached commentary, as something ‘they are not used to’, and thereby hinting at its ‘unscientific’ shape:

> We are all going to learn about it [the decision-making tool]… it is about how we make an informed decision, given our evidence.
>
> We will use research evidence with the weights policy makers attach to criteria

(PI, Liaison Meeting, 18th November 2013).

In the second quote the PI explains that the role of the policy makers to other team members, and the process of 'weighting' these according to the selected criteria. What was interesting was both the PI's use of the word 'evidence' to refer to the projects work, a term not used when discussing output within the team ('data sets' or 'data' were how 'the stuff' of work were talked about), but also his construal of a decision as requiring external input. In line with some of the responses I received from informants during interviews, the PI suggests that it is the work of the scientists to provide data to be used evidence, but how it is used to make decisions is a different thing:

> There is a slight dissonance between the questions they want, related to a particular type of policy, say "I'm going to subside that, or hack that", asides from the generic, the slightly more, umm kind of the perturbations, experiments where you reduce by certain fractions [...] but if they are saying, what we really want to find out is, if we pass this law that improves all new cars by such and such amount, and we think that reduces emissions by 10% is that a way of linking up the two, or would you more, crude sectoral results have very different [?] depending on how the end is achieved? (PI, Liaison meeting 18th November 2013).

The PI suggests that the policy makers want to make general claims. The scenario of 'passing a law on cars' highlights one way in which he anticipates how scientific data could potentially be used as evidence. By using emissions data and a perturbed model run the PI delineates an epistemic link between science and policy. He also makes distinct the different means and ends of scientific research and policy evidencing, so that the way in which data is made to relate to a reduction in air pollution would then be of interest to policy makers. The problem shifts both the *production of* data as evidence to *how* data will be used as evidence. This shift was materialised through the production and running of 'modelling scenarios', where the CM-WM model was used by the atmospheric chemists to produce data on air pollution in the future.

Constructing scenarios of air pollution

What the CM-WM model can do in terms of producing data on air pollution in the future was discussed over a series of liaison meetings. The running of the model into the future involved changing the input data into the model. The input model is the emissions data (as I describe in Chapter Four) and by changing the emissions data according to predicted emissions under particular policies, the model can then produce data of air pollution concentrations under these emissions changes. Indeed, Elizabeth reiterated to the other researchers that, 'our model is only as good as our emissions data'. The statement highlights the recursive nature of modelling, where emissions data are used as input into the modelling of air pollution scenarios, which are then used to demonstrate the effect of these emissions on air pollution.

The capacity of the model to model the future was shaped by the manipulation of the emissions data that was put into it. Yet, the re-construction of air pollution relations didn't necessarily mean one can move easily between pollutant concentrations, the emissions that produce these and the related policies that may influence changes in emissions. During a meeting preparing for the stakeholder meeting, Elizabeth shared her desktop screen which listed the different SNAP sectors that the model can simulate (Figure 29) to those present in City A, maximised onto the larger screens in the meeting room. With verbal support from modellers Craig and Sam, Elizabeth emphasised the limitations of making policy claims with the modelled data:

> [For] example the source attributable for Ozone and Particulate Matter in a highly polluted location are distinctly different to the pre-industrial and present day atmosphere […] on what we are trying to say is if we turn off 100% emissions we have different emissions scenarios, and so there is some non-linearity there, does that make sense? (Elizabeth, Liaison meeting 24th June 2014).

Here, Elizabeth points to the inherent uncertainties involved in making scientific data on air pollution. 'Non-linearity' affects the modeller's ability to make a 'decisive association' between emissions and pollution levels, which hints at their perceived tension between scientific uncertainty and the policy need for a strong evidence-based claims.

The slide below lists the classification of ‘Selected Nomenclature for sources of Air Pollution’ (SNAP). These were important for modelling air pollution scenarios because they link the source of emissions data to the potential shifting of these sources in the future according to changing emissions.

Sectors in the EMEP emissions data

SNAP 1	Combustion in energy and transformation industries
SNAP 2	Non-industrial combustion plants
SNAP 3	Combustion in manufacturing industry
SNAP 4	Production processes
SNAP 5	Extraction & distribution of fossil fuels & geothermal energy
SNAP 6	Solvent use and other product use
SNAP 7	Road transport
SNAP 8	Other mobile sources and machinery
SNAP 9	Waste treatment and disposal
SNAP 10	Agriculture

SNAP = “Selected Nomenclature for Air Pollution

Figure 29: The classification of emissions sectors for modelling future scenarios.

The sectors map onto the scenario building process, which can be visualised as maps of data (Figure 30). Indeed, following on from the stakeholder meeting, the modellers adapted their model scenarios according to the policymakers chosen policies. Using the SNAP sector emissions, the model can simulate future emissions under different scenarios, which are then reduced or increased according to certain policies that are considered as influencing a related emission source. The SNAP sectors were, then, a way to categorise air pollution in a way that enables data to be used as potential evidence. By shaping the production of data through the influencing of emissions input into the model, the sector classifications ensure data carry meaning in terms of potential policy actions.

The circular nature of this process - of adding input data into the model and then shaping that input data to shape the output data into ‘evidence’ which policymakers are interested in - made the movement from data to evidence appear as a logical outcome. However, in practice it was rather difficult to make data practices and evidence practices cohere. As I highlighted in Chapter Four, the model works by simulating the atmosphere. In our case here, the CM-WM model was used to simulate the future atmosphere by influencing the emission data input into the model. The CM-WM model became a material interface around which science and policy engage, where the constructing of potential ‘policy scenarios’ became the hypothetical playing out of policies.

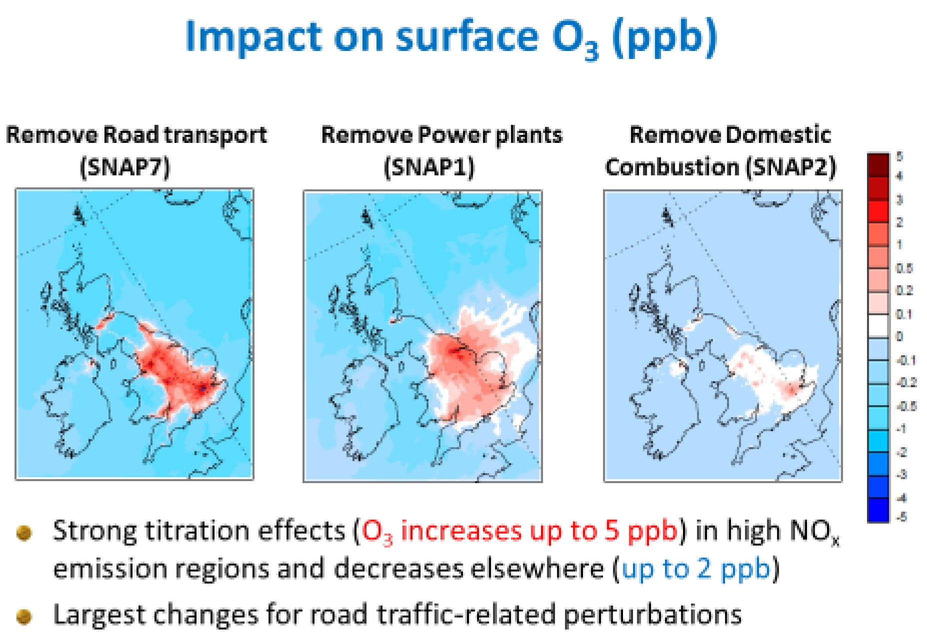

Figure 30: Coloured map of changes in Ozone levels following perturbed runs according to three SNAP sectors.

The running of simulations on the CM-WM were called perturbation runs, where a certain emission source is reduced by a percentage according to a given policy. In the image above changes in Ozone concentrations are shown in red, under scenarios where different sector emissions are removed from the simulated atmosphere. Perturbed runs were described by Elizabeth as a process which involves input from policy bodies, but also key individuals whose expertise carry a similar weight in value (knowledge value). For example, policy knowledge was prescriptive to modelling practices in terms of the building of future scenarios:

> So people [policy makers] talk about win-win situations which are good for both [...] other scenarios, you have one called "maximum feasible reduction" [developed by the] IPCC put together what they thought, what was based on the present day of feasible polices which give us a 40 percent reductions across the UK. So that's one of our emissions scenarios. They have also produced a current legislation scenario (Tim, Interview September 2012).

Scenarios were often talked about by scientists and stakeholders in terms of two extreme scenarios and a more neutral scenario, referred to as: 'maximum feasible reduction', 'minimum feasible reduction' and 'business as usual'. These different reductions and increases relate to particular pollutants because of the classification of emission sectors. In terms of Elizabeth's comments above, the Intergovernmental panel on Climate Change (IPCC) scenarios refer to CO_2 and greenhouse gases. Scenarios, then, were a way to explicitly engage with policies by orientating data production towards specific and potential legislation:

> It is more to decide if we can do the calculations and input the data because it falls on us to do this. And if we say we are going to do the costs to industry we need to work out what those costs are (Tim, Interview September 2012).

The SNAP sectors were not only classifications by which pollutants were ordered and managed in the practice of modelling, but framed the playing out of 'policy scenarios' in the model. The manipulation of data in perturbed runs meant that data carried information that linked it to an associated sector pollutant, and therefore to a sector of policy interest. However, creating a link between the pollutant and its source with data was not straightforward, as the previous discussion shows, which meant a lot of preparation before the stakeholder meeting centred on 'what can't be done', to 'define the limits' of the model, but also to negotiate the kind of story WHAP will tell about air pollution.

Prior to the stakeholder meeting, Francis suggested that the modellers provide a list of calculations which 'we can do' on the project, so that stakeholders would not suggest scenarios which can't be carried out (the stakeholders were eventually given a list of options to choose from):

> It is a balance of being pre-determined, but having to start with something, to demonstrate what the issues are, the aim of the workshop is to decide the policy options and on the criteria, so have to start with a candidate list with the policy options and with the criteria [...] maybe Sam and Elizabeth can be more concrete on what the options are (Tim, Liaison Meeting 18th November 2013).

The practice of modelling data as evidence had its own constraints, therefore, as the opening quote to this section suggests. For data to be used as evidence, the distinctions between science and the implementation of this knowledge in the real world are anticipated through the construction of scenarios. The making of a link between an air pollutant, what produced that pollutant and the effect of a pollutant was complex and uncertain. Yet, delineating between these different stages in the trajectory of air pollution (source, pollutant and effect) was essential for making data relate to policy, highlighting the implicit framing of air pollution as a policy concern.

Constructing a science-policy research object

> Our model can't model CO_2 emissions, but here is a taste of what we can do, so by removing Road Transport (SNAP 7), Power plants (SNAP 1), and Domestic Combustion (SNAP 2), we have examined the effects on NO_2, O_3 and $PM_{2.5}$ (Elizabeth, team meeting 30th November 2013).

During the stakeholder meeting, Elizabeth referred back to the emission 'SNAP sectors', listing some of the aspects of modelling she considers relevant to the decision making process, in terms of deciding on policies and criteria for assessing these policies. In the statement above, the role of the scientists and the stakeholders is clearly demarcated, and Elizabeth states what they as modellers can and can't do. However, the process of stakeholder engagement did also change the roles of scientists and stakeholders, where at times stakeholders became data producers, rather than simply the 'end users' of scientific output:

> What you can do is develop a model, make a model looking at, 'if you reduce emissions what the impact on the health will be?' a mathematical model or physics based model. But if too complex, or not enough information, you can elicit expertise to work out how to value it. So in an absence of models, maybe we ask

> an expert what their view would be of what that impact will be. [It is] a way of eliciting these probabilities, the difficulty is that different experts have different opinions (Elizabeth, Interview 8th December 2011).

A common differentiation between science and policy made by researchers on WHAP was the kinds of claims each make, where policy makers were, it was often stated, concerned with more ‘general questions’ and the making of ‘sweeping statements’, whereas ‘science’ was interested in the local-level details and ‘specifics of data’. However, during the stakeholder meeting the modellers were unable to answer some of the questions the stakeholders asked them because of the level of detail necessary. For example, one stakeholder suggested that the level of granularity of the emissions data input in to the model was problematic for the making of an evidence claim because of missing data of how these are recorded (by the Natural Atmospheric Emissions Inventory (NAEI)). Indeed, during the meeting, Sam (an atmospheric chemistry modeller) specifically asked one of the invited stakeholders to provide them with data at a finer level of detail, explaining that they have no knowledge of how the emissions are calculated for the NAEI inventory:

> Firstly, [stakeholder two] suggests that this information should be readily available, and they should contact the NAEI to find out how it is formally recorded. A different stakeholder then suggests a relevant project examining the increase in biomass in domestic sector due to different burning (Fieldnotes, Stakeholder meeting 31st November 2013).

One of the stakeholders (stakeholder 2) offered to provide more detail in terms of ‘patterns of fuel use’ within the domestic sector, adding further detail and complexity to the scenario runs and therefore the data it will produce. Stakeholders, therefore, also contributed to the ‘scientific’ remit of data production, which suggests that evidential claims by policymakers were not always broad and sweeping, but can also be specific and local. This complicates both a relation between science and policy as being defined as distinct and separate, and a trajectory of knowledge production that involves a linear movement from local scientific data to non-local evidence.

That this more messy process of science-policy relations in practice were not explicitly problematised by researchers suggests that it was not a surprising outcome and, as Elizabeth points out, they in fact needed policymakers to fill some of the gaps involved in producing modelled data. The modification of the model meant data was shaped by policymakers' input. Engaging with stakeholders therefore intervened with data practices themselves, illustrating that data and evidence can't be distinguished according to the scale of the claim being made.

Instead, one of the ways in which air pollution is materialised as a science-policy object was through choosing and deciding upon which air pollution relations are of interest, and which scenario runs to carry out, and therefore what type of data is to be made. It is this act which fixes particular emission sources, and which relates it to a particular air pollutant, along with its measured effect. Fixing the way in which air pollution is to be marked and made real enables the construction of a response, and therefore the making of data in a way that means it can be used as evidence for a policy implementation. As Tim explains to other team members, the process of 'bringing it into being' ('it' being air pollution) is the ethos of their work, pointing to the mobilising capacity of science-policy networks[57]. This analogy to 'becoming' resonates rather well with the notion of performativity that I have developed throughout this thesis, in terms of the ways in which practices don't just represent air pollution through data, but enact data as air pollution in material ways.

Making a *selection* and fixing data relations

Towards the end of the stakeholder meeting, as the final decisions were being sought, statements became more fixed:

> In terms of environmental impacts, the one metric, Nitrogen deposition, is the one to go for (Stakeholder 1)
>
> In terms of the UK, NO_2 is of relevance in terms of policy (Stakeholder 4)

[57] This is not simply the fixing of one perspective of air pollution over another, but also one that may get done and re-done in subsequent practices. For example, the materialising of specific configurations of air pollution, according to the selections made in the SH meeting, may lead to further enactments, for example, through the potential implementation of policies relating to these scenarios.

> [social acceptability] – put it in but rate it low (Stakeholder 1)
>
> (Stakeholder meeting 31st November 2013).

Stakeholders also suggested ways to manipulate the socio-material and technical relations which produce air pollution, pinpointing tangible actions through which emissions sectors could be influenced.

> Impact of new technology, cars electric, the only way is to change the fleet (in terms of modelling 'change the fleet'?"
>
> low congestion zones, differential taxes on cars.
>
> fleet age, exhaust pipes, hybridisation of buses.
>
> (Ibid).

These potential modifications were later materialised by the modellers as perturbed runs. Stakeholders decided which pollutants were of interest and what actions could be manipulated in order to effect the concentration of the chosen pollutant. These decision-making processes define the relationship between source-pollutant-effect in a way which made it appear as natural and logical, but one which ultimately relied on their ability to construct a causal relationship between the relations which make air pollution air pollution (emissions and their negative effects).

> Stakeholder 2: There is evidence which relates to NO_2 and O_2 to mortality […] but With NO_2 it is highly correlated to $PM_{2.5}$ and we don't have to modes we can...O_3 is all over the place, so divisive association NO_2 and mortality co-ordinated with $PM_{2.5}$ […]. My suggestion is you should at least start with what has come out of larger international groups.
>
> Stakeholder 1: I agree, long term exposure to $PM_{2.5}$... Another is Ozone, related to climate change and the effect modification at higher temperatures are correlated with $PM_{2.5}$
>
> Stakeholder 2: I agree, co-efficient there, but there are different grades of certainty.
>
> (Ibid).

Thus, the decision-making process was ultimately a selection process centred on 'do-ability', both in terms of the model's capacity to simulate the perturbed run and in terms of policy knowledge and anticipated implementation. For the policy makers to use data as evidence, the building of a narrative about air pollution based on the relation between emission source-pollutant-effect was required.

The contribution from policymakers was re-considered by the WHAP team at the follow up liaison meeting. Below is an extract from some shared meeting notes taken by a project member during the stakeholder meeting, which is useful for highlighting some of the conceptual questions which emerged as a result:

> Should we explore further the interactions between socioeconomic class and health impacts?
>
> Long term exposure to $PM_{2.5}$ / years of life lost / attributable mortality to $PM_{2.5}$ (evidence for other pollutants is shaky?)
>
> Attributable mortality to NO_2 (credible estimates, strong relationship found in Australian study, indicator of combustion related pollution, what are the UK estimates?)
>
> (WHAP internal meeting notes, 6^{th} December 2013).

The stakeholder meeting encouraged reflexive work, so that finding out what kind of relations the policy makers were interested in also involved a re-consideration of health as an epidemiological variable:

> […] but then the question came up, what does that actually mean, what does health entail, is it just a number of bodies, is it life years lost, is it some amount of morbidity or mortality?[...]this is something we need to think about (Tim, Liaison meeting 2nd December 2013).

The process of ensuring data was useful for policy makers became interwoven into epidemiological data practices themselves, just like in the perturbed model runs for the atmospheric chemists. Francis, Tim and Peter discussed how they could untangle 'health' as a relational yet linear effect of air pollution in practice:

> Francis: we are looking just at mortality, or are we going to look at morbidity too?

> Tim: well that is for us to decide. They [the stakeholders] did make the comment that they thought body count alone is not enough.
>
> Francis: so changes in life years? So one of the things I want to get is the WHO document that [stakeholder 3] commented on.
>
> Peter: to define impact from morbidity from air pollution, I mean, it is quite a tricky thing [another academic] has been sitting on this for five years, presumably without coming up with things[…] their [the stakeholders] job is to keep us informed, but I don't think we should rush to promise anything on that.
>
> Tim: no I think that is probably a wise comment. I think part of the issue they made about body count is, whether it is number of people, or life years lost? And I would be keener we do years of life lost, especially if we so something over number of years because then we can track it through
>
> Francis: Just, another query about whether we have we decided on health impacts and on which changes in exposure we are looking at, is it just PM?
>
> Tim: good point. [Stakeholder 3] made the point that PM dominates [policy] therefore that should really be our focus, and [stakeholder 6] chipped in, then [stakeholder 3] agreed with him, that maybe we should look at Ozone too […] even if a bit mixed not entirely sure what co-efficient to look at etcetera […] whether we do something separate for Ozone, and we are a bit divided on that [in the epi group], does it have relevance in terms of how chemical models work?
>
> (Stakeholder meeting 31st November 2013).

In the dialogue above, both what type of health associations to study and which pollutants to focus on were under question. A co-efficient is a measure of the strength of the linear association between two variables, in this case between PM and human mortality. In order to materialise this co-efficient through statistical methods, both air pollution and health need to be defined and fixed. However, as Tim highlights, how to separate and untangle these relationships is complicated, and *which* co-efficient to choose depends on their ability, as epidemiologists, to make an association, but also one that can be evidenced according the trajectory of source-pollutant-effect.

The trajectory of 'emission-pollutant-effect'

In this chapter, I suggest that the idea of stakeholder engagement was based on a normative ideal of science and policy, as distinct realms of practice, yet, in practice, these relations were rather messy and entangled. As such, I argue that the distinction between science and policy was performed through the act of engagement, materialised in scientific output as evidence rather than data. This relation was performative in the sense that, in practice, science and policy were difficult to distinguish, as were data and evidence. To make a distinction between stakeholders and scientists, the decision-making tool was used to delineate roles and responsibilities in particular ways. Indeed, evidence was actually made through the modelling of air pollution scenarios as a result of the stakeholder meeting. These scenarios integrate science and policy, fact and value into one space, which were then separated and contained from other scientific practices through the use of the decision-making tool. The decision-making tool similarly performed a distinction between science and policy, because scientific input and policy input were formatted through the decision-making model in a way that shaped the ways in which each *engaged* in the process – where science provide the data of air pollution ('objective fact'), and policy makers judge these by prioritising which air pollution issues are most important ('subjective value').

In the chapters which precede this one, I have shown that air pollution is a multiple and complex phenomena. As a result, I have focused on data practices as a way of tracing and making tangible the ways in which data were used to enact air pollution in particular ways. The modelling of air pollution scenarios was also a way of mediating these multiplicities through the construction of singular, linear stories - about green technology, or the reduction of fuel prices. Decision-making was not reductive, but a recognition of the multiplicity and complexity of the world, and the simultaneous need for 'credible evidence'. I therefore call this act of choosing policies a process of *selection,* which is a component of the making of decision, framed by three criteria: the capacity to measure and therefore accurately manipulate emissions, an understanding of and ability to model the air pollutant being studied, and finally to be able to trace this emission-pollutant relation to a measurable effect (health, environmental, economic, social effects). Decision-making was not about decisions but *selections*, and the mobilisation of a particular trajectory of air pollution from a multitude of others.

Stakeholders chose a number of different policy options, including the changing of transport fleets and introduction of ‘green technology’, which can then be simulated by the model and used to evidence the effect of a particular implementation on air pollution concentrations. Implementing a restriction on cars being driven is a transport wide intervention, simulated by the model through the changing of the emissions input data according to the SNAP sector classification system. The modellers reduced the emissions input data by a certain percentage, by manipulating the emissions input data into the model, which was understood as achievable through the making of certain restrictions to the use of transport. Both the scale of the problem of air pollution and the analytical scales with which researchers work was at once a movement of ‘extension’ and ‘narrowing’, which meant the movement of data into evidence was not simply a movement from local (data practices) to universal (evidence claims). Data only became evidence through their attachment to a policy problem: from producing new data in future scenario model runs (extension), to separating particular relations of interest from other atmospheric relations (narrowing).

At a rather fractious liaison meeting following the stakeholder meeting, the PI reminded everyone that, as scientists on WHAP, their priority was: ‘bringing air pollution into being’, emphasising the importance of ensuring their research did not remain in a ‘research vacuum’. This was a rather ambiguous statement and echoes science studies’ ontological theorising around the socio-material processes of knowledge practices. The PI’s reflection can be considered in two ways. Firstly, as relating to the discursive materialisation of scientific research into evidence and, secondly, in terms of the material effects the process of engagement implies. The latter is a process of articulation of air pollution through anticipating the potential (the playing out of policy options) reconfiguring of socio-material relations which influence air pollution as a material entity (their actual implementation). Although policy options were only hypothetically materialised, they resulted from anticipating how to manipulate relations and enact particular kinds of socio-technical changes. This configuring of relations affords particular kinds of actions, which construct air pollution as an environmental-health concern and as a problem which can be engaged with. This was, in a way, a process of further visualisation, in the sense that air pollution became a tangible science-policy object, where the interactions which produce and are effected by air pollution were made visible and traceable (in terms of the source-pollutant-effect relation).

Summary

I started this chapter by pointing out that the visualisation of data was not a point of closure but a point of departure, and therefore highlighting the ways in which the visualised forms of data were subject to further conversation and performativity in the process of stakeholder engagement. Data shift from being sources of information to sources of evidence. This chapter, then, introduces the next iteration of the journey of data, in its extension outside of the scientific relations of the WHAP project.

In the chapters which precede this one, I traced the collecting, producing, processing and re-use of data, and for each, I employed a key concept to hang the development of my argument on. I began with *difference*, focusing on the multiple ways in which data are produced on air pollution. I then showed how these differences are negotiated and ultimately *coordinated* by researchers, highlighting the particular tuning processes that data undergo as a result. From co-ordination between data practices I then moved onto how the object of research shifts from air pollution to health. I developed the concept of *correlate*, both as a statistical term (used to refer to a particular statistical relationship between a variable and outcome) and as an analytical concept to understand the visualisation of health through notions of time. I suggested that these visual correlations assemble data in new ways, which shift the object of research from air pollution to health. The sharing of these visual forms to new audiences led me onto the topic of this final chapter, that of *selection*. In the process of selecting some air pollution relations are materialised as evidence, through the making of modelled data of air pollution under anticipated, future policies.

The model of knowledge production I demonstrate through this story of data is one which involves a movement from ambiguity to further complexity. This is rather different to Latour's model of purification, in which the mediating roles of 'nature' and 'culture' are cancelled out. Air pollution is a particularly difficult thing to study, both for scientist and ethnographer, for it is an inherently hybrid, complex but also a paradoxical form. I have shown that it is impossible to have air with no air pollution, yet air pollution is problematic because it is not just air. In Chapter Four I detailed how pure air is made, demonstrating the difficulty of making air and air pollution visible as distinct entities. In many ways then, the work of collecting, producing, processing, reusing and translating data between data practices involves making air pollution more visible. In this final chapter I have shown how air pollution becomes visible as a policy concern. Here, visibility relies on an extension of the scientific network. Indeed, I show that the movement from the specifics of data and disciplinary interests to the wider dimensions of air pollution, beyond health, is analogous to the arrangements of scientists and policy makers (reified by the decision-making tool). In doing so, I demonstrate the assembling of what Latour calls a 'matter of concern', highlighting the humans, environments and non-human things which contribute to making politics happen.

Chapter 8: Discussion

Introduction: knowing, sensing and visualising research objects

> Bodies may be […] intersections of large- and small-scale practices; but if bodies are an intimate location of effects and agencies, air is substance that bathes and ties the scales of body, region and globe together, and that subsequently enables personal and political claims to be scaled up – to global environmental politics – and down – to the politics of health (Choy, 2012: 140).

Choy makes the argument that air is a good starting point for social theory because it not only transgresses spatial, temporal and analytical scales, but also effects and affects things, people and social and political structures. I started my ethnography with the idea of tracing everyday scientific practice, but in doing so have also traced the scaling effects of air, in science through to policy, that Choy so powerfully recounts. My field research was carried out about and alongside scientific researchers studying the relationship between air pollution and health. I also examined the ways in which these research practices were tied up with the politics of environmental health, and facilitated the extension of scientific practice to policy action. Air, as a substance, may enable personal and political claims to be both ‘scaled up’ and ‘scaled down’, yet it also offers an ethnographer the chance to ‘zoom in’ to the intricate workings of scientific knowledge production of air pollution, and how they may shape how we come to *sense* and *know* air.

Throughout the chapters in this thesis, I provided an in-depth analysis of the everyday practices and processes that go into doing collaborative science and making multi-disciplinary knowledge. The WHAP project provided a novel case study to ethnographically explore a scientific project from beginning to end, and therefore to access the formation and emergence of scientific information, objects and claims in process and under contestation. In the preceding chapter, I showed that scientific and technological assemblages were not just objects of research but knots of social and political interests. Through the formation of data on air pollution, and its constructions as a science-policy research object, I argued that constructing knowledge is not only ‘social’ but becomes ‘ontological’, where human and non-human agents, and affectively animated forces such as ‘concern’, are entangled in the ongoing material remaking of air pollution as a research object (Puig de la Bellacasa, 2011).

Pulling together the different chapters and their inter-weaving themes, in this final chapter I consider the different ways in which science gets done, and the conceptual and material dimensions of objects of research stabilised. In doing so, I also reflect on my own role as co-collaborator as part of these processes. By focusing on data practices and the conceptual tensions which emerge when working across different material and cultural ways of knowing, I demonstrate what makes science meaningful and challenging for researchers. The material, practical and conceptual enmeshing of science in practice can be seen through the production and articulation of different data, and the relational capacities of data in formation. In this way, I provide a sense of the role data play in WHAP; how they move, demarcate boundaries, and the ways in which different kinds of data ‘live together’, but also the role of these practices in the ‘issuification’ (Marres, 2014: 224) of data-in-combination, and the materialisation of air pollution as a ‘matter of concern’ (Latour, 2004b).

I argue that following data as the objects of interest for researchers renews purchase on human and non-human relations, and the practical and the social dimensions of scientific work. Furthermore, data are particularly good scientific objects for the conceptual and descriptive work of ethnography because they are informational and material forms that mobilise and mediate across different research practices and conceptualisations of air pollution. The ways in which researchers conceptualised, and practically configured, multiplicities in practice, was also the way in which I got a sense of, and described, some of the things, people and infrastructures required in order to make practices work. In tracing the practical and conceptual remits of data, I practice what Jensen (2012) calls ‘anthropology as a following science’, elucidating scientists’ concepts, structures and ways of materialising the research worlds rather than evaluating them from without.

This method of following data was particularly useful for the study of epidemiology, and indeed public health more broadly where what ‘is’ the object of study is rather intangible. Unlike the observational field work and experimental laboratory practices of physics or the life sciences, epidemiological knowledge practices cannot be seen, observed or touched through any of the traditional methods used within science studies. Unlike Hobbes and Boyle’s ‘air pump’, the WHAP epidemiologists’ practices did not centre around a kind of object, through which, as Shapin and Schaffer (1985) show, a particular mode at arriving at scientific truth can be delineated. It was, in fact, near impossible for me to ‘observe’ epidemiological data practices, and epidemiological knowledge objects remained rather opaque. However, as Chapter Six showed us, the visualisation of data were one of the ways in which epidemiologists shared their work and through which, as ethnographer, I was able to trace and materialise epidemiological data production. In this way, I demonstrate that research objects, such as data visualisations, mediate scientific relations (including ethnographic field work) in ways that highlight the concerns of scientists, the properties of phenomena and the external forces and events, such as human behaviour and policy objectives, which shape the form and structure of knowledge.

Overview of argument

I quickly came to realise that, different types of information about air pollution were produced through particular data practices and the sharing of these data between researchers. Data were both the informational form and material means through which research, and ultimately the production of knowledge, was carried out. My initial research finding, which shaped the form of this ethnographic study, was the ambiguity around the object of research air pollution. This wasn’t talked about as a real problem by researchers, indeed it wasn’t even a topic of discussion, yet doing multi-disciplinary work was. Beginning with my own puzzle, alongside the self-acknowledged challenge of doing ‘collaborative research’ by researchers, this thesis was ultimately about everyday practices and processes with scientific data. In each chapter, I revealed a particular moment in the research process of studying air pollution, using data as the material, prescriptive and informational form through which to trace this movement.

I split the organisation of my study into three parts, in terms of the generative nature of these practices and processes. In Part I, I described the process by which my field site emerged and research questions developed as an anthropologist in a multi-disciplinary collaborative research project. Part II focused on internal differences, exploring the multiple ways in which the research object air pollution was made through data practices and, subsequently, the challenges which arose for researchers on WHAP when these research objects came into tension. In Part III my analysis moved beyond data of air pollution, to the ways in which these data were made meaningful through their attachment to new kinds of data, values and discourses. As external relations were added to the research object new differences, relations and insight were made possible.

In terms of the social and cultural studies of science, this thesis makes three contributions. Firstly, I demonstrate the central role of data practices in collaborative scientific research, specifically in terms of the making of public health knowledge of air pollution and health. By describing the production, processing, negotiation and re-use of multiple types of data across different epistemic practices, I show that data shape and are shaped by both data producers and data users. The dynamic and lively negotiations which sharing data entailed points to the multiple values, meaning and interests which are tied up with data. Data were, then, not only the means by which research was carried out, but also the very stuff that was engaged with in the process of working out what the object of research was: of what counts as air pollution, health and, indeed, an environmental health problem.

I demonstrate the ways in which the very structure of the WHAP project attests to the multiplicity and heterogeneity of air pollution in ‘the real world’. It was understood by researchers that air pollution can only be studied as a public health problem by combining multiple data together. Thus, the multiplicity of air pollution, and its construction as a hybrid form, was enacted through the organisation of data practices in WHAP. Yet, multiplicity was also problematic. Data came to be both the way in which differences were made visible, but also the means by which they were ultimately overcome. The M&M tension exemplified this enabling and disabling capacity of data, where local data practices carved out spaces for ‘single’ and ‘multi’ disciplinary working. Indeed, data materialised relations across difference, producing sites where epistemological differences were articulated. Primarily this took place at the multi-disciplinary table during weekly liaison meetings.

Moving on with multiplicity, I showed that difference was itself productive of data, knowledge and scientific relations. I examined the ways in which researchers with different epistemological starting points worked out ways of communicating, sharing and combining data successfully. Secondly, then, in tracing the everyday practices which enable data to move between research practices, I demonstrate the locally-configured solutions which managed difference. I used two empirical examples to develop this argument, the first being the shift from representational claims to ontological claims, where $PM_{2.5}$ became the particular air pollutant of interest, described in Chapter Five. The second example was the shift from air pollution to health, performed through the visual work of the epidemiologists who linked and correlated air pollution and health in one analytical, conceptual and material space (described in Chapter Six). In this latter movement, the object of interest became the visualisations of data-in-combination, which mobilised multiple audiences, rather than the combination-of-data in practice, which took place at particular sites of practice.

In relation to this second point, I reflect on a number of different analytical models for analysing multiplicity and difference, which I outlined in Chapter Three. Beginning with boundary objects and trading zones, I found, however, that researchers engaged and developed ways of managing, negotiating and co-ordinating difference in a more lively and dynamic way than the rather static spatial temporality these models of sharing allow for. First, researchers themselves acknowledged this multiplicity as a constitutive part of doing multi-disciplinarity, and therefore reflexively observed the way in which air pollution can be studied in new and different ways. For the modellers and the environmental chemists, the multiplicity of data and air pollutions was not a problem. However, for the epidemiologists it was. As a result, the act of sharing required experimental work by both those producing the data and the epidemiologists who re-use that data. So, the movement of data also became a practical challenge rather than one about perspectives. I characterised this movement as one that was non-linear and functioned in a to-and-fro fashion, where the empirical and conceptual constraints of data were worked out across as well as within different data practices.

With this second point, I argue that the concepts of boundary work do not work so well, because it was not simply that different interests needed to be co-ordinated, but that what was of interest had to be worked out, together. There was, in Deleuze's words, 'intrinsic difference' (1994), in that there was a different conceptualisation of research objects by researchers, and extrinsic difference, in that there were differences in practices of representing air pollution through data. Much of the work of researchers on WHAP involved defining, re-defining and materialising, in different ways, where the boundaries of the research object should lie. To use the same metaphor, the researchers were interested in these extrinsic differences. I suggest, then, that a livelier model of co-ordinating difference is required for capturing translations between data practices when multiplicity and difference are accepted by researchers as constitutive of the research object.

The third argument I developed through my empirical chapters was around the way in which the moral value of 'health' shaped data practices and scientific relations by rooting them in non-scientific concerns. Health was a key driver for the different researchers on WHAP, motivating their multi-disciplinary collaboration and engagement with stakeholders. This finding relates to the model of translation demarcated above, for it was the movement of data that extended relations of data and thereby air pollution's attachment to 'real' concerns. Further, when data were tied up with health, additional work and modification was required, in order to find a single object to make claims about. Yet, these were all partial attempts to solve the inherent partiality of studying air pollution. Indeed, to act upon air pollution relies on its grounding with visible forms, such as measures of mortality or greenhouse gases, or in terms of physical changes to the UK transport system. Such external linkages did not resolve the multiplicity of air pollution in practice, and each required further effort 'on the ground' in order to manage and temporarily hold together new articulations of difference and tension.

Detailing the everyday complexities of data practices was, then, an ethical opportunity to flesh-out science as a human (and more-than-human) endeavour, to the extent that I even focus on individual scientists. This means that rather than examining how matters of fact are fabricated as part of a network of actors, I offer a more partial account of the role different actors play in the process of knowledge production, bringing to the fore the local, material and practical networks that accompany artefacts through the whole duration of their lives (Latour, 2000: 250).

Different data, different air pollutions: *multiplicit*y and *co-ordination*

In Part I, I introduced the concept of multiplicity both within science studies literature and in terms of the WHAP project itself. I used this as a point of departure for Part II, beginning with the empirical puzzle, how does work get done when there is no shared object of research? I found that data came to stand for particular kinds of research objects, and in practice air pollution was *done* in multiple ways.

In Chapter Four, I provided a detailed ethnographic account of two different data practices which measure air pollution. In doing so, I highlighted the craft-work, expertise and localised knowledge through which these practices were constituted. In air pollution monitoring, cleaning rids the measurement of unwanted contaminations to ensure that it is measuring the pollutant of interest. External contaminants are quite literally scrubbed away, leaving a 'pure' measurement of air pollution. In modelling, the de-bugging of a simulation run works along a similar principle, through which the modeller ensures that the model output combines the right kinds of information, in a way that is understood as coherent with the ways in which atmospheric relations interact in reality. In both settings, measurements were manipulated in technically-mediated ways, which transformed these numerical readings into data. The on-site nature of these particular data producing practices rely on the mobilisation of external references through which the internal measurement can be stabilised. The experiential knowledge and skill required to judge good data from bad data was contingent on the material arrangements of data practices, which meant that what counted as data or error was relative to the particular scientist-instrument-object of research configurations stabilised in practice.

In light of changing dimensions of accuracy and the technological capacities of representing reality in new objective ways, my ethnographic descriptions of measurement practices is illustrative of science and the scientific method more generally. Indeed, it has been stated that, increasingly, the scientific method is becoming obsolete (Khoury and Ioannidis, 2014) usurped by new technologies and ways of reading and managing large data sets. The multi-disciplinary field of public health, and primarily epidemiology, has changed immeasurably with the emergence of ‘big data’, which has enabled researchers to use and connect very different kinds of information. In WHAP, the different kinds of data shaped the very meaning of ‘scientific method’, and what counts as being ‘scientific’, a tension visible during the modelled and monitored data negotiations around the making of ‘good data’, as discussed in Chapter Five.

This chapter examined how researchers engaged with the multi-disciplinary problem of bringing together different ideas around what counts as a good representation of air pollution. The tension was primarily characterised by the epidemiologists as a representational issue concerned with what data best represents air pollution. However, this developed into a more ontologically orientated concern, around what air pollution the project was interested in producing knowledge on. These developments point to the de-stabilisation of data in re-use (and suggests the inherent instability of multi-disciplinary work), because ‘what data stands for’ became subject to further inquiry from those outside of the practices which produced them. I called this phase of instability, where what data represents, and how best to represent air pollution came under scrutiny, data-in-negotiation.

Data were the material means by which negotiations across different data practices took place. In many ways data functioned as boundary objects, enabling communication despite the explicit voicing of disagreement and tension, but also as ways of translating epistemological differences around ‘representation’ and ‘reality’. For the epidemiologists, multiplicity of the research object (in modelled and monitored data) was problematic and they wanted to find the best way of representing air pollution. For the modellers, multiplicity was more acceptable and modelled and monitored data were both used to achieve different means. This meant that different data could not be combined, like the epidemiologists suggested, because they represent different kinds of research objects. In order to work through this tension two new kinds of data were produced and used to offer purchase on modelled and monitored data. In the process of negotiating data, a solution emerged.

In order to resolve this problem of multiplicity, the epidemiologists used two new kinds of data to interrogate modelled and monitored data 'empirically': the simulation study and the collection of DIY data. These new data materially intervened in the problem of multiplicity. Not only did they provide new information on air pollution, they also had currency in a multi-disciplinary setting. The tacit, embodied nature of making data, as highlighted in Chapter Four, made it difficult to share knowledge, expertise and data in the primarily verbal and textual setting of the liaison meeting. Yet, data can be visualised and manipulated into material forms which make talking about different data practices possible, as something tangible, with which different kinds of researchers could materially engage and modify on their own terms.

I showed that the new relations between data initiated not only an epistemological and conceptual shift in research practices, but was also an ontologically orientated move because researchers started to talk about what the object of research was, rather than how to measure it. $PM_{2.5}$, as a particular kind of air pollutant, was the practical compromise to the M&M tension, rather than a conclusion as to what kind of data is best. It was the co-ordinate $PM_{2.5}$ that became the particularity within air pollution's heterogeneity, which enabled co-ordination. At the same time, paradoxically, it was the internal heterogeneity of $PM_{2.5}$ which aided its suitability for the ground-work of multiplicity and difference. The work of co-ordination was derived from a locally produced practical solution. This was also a generative process, in the sense that it was only a temporary solution, because with the addition of further data and data practices new differences arose. As such, co-ordination can be best understood as a situated and temporally unfolding process of technical accomplishment.

The production, processing and re-use of multiple data: *data linkage* and *selection*.

> [D]ata provide a way to survey the world, yet it is important to remember that surveying the world with data at some level means having data visibly before one's eyes, looking through the data is not always self-consciously looking at the data [...] Not only are data abstract and aggregative, but also data are mobilised graphically. That is in order to be used as part of an explanation or as a basis for an argument, data typically require graphical representation and of one involving a cascade of representations. Any interface is a data visualisation of sorts […] (Gitelman and Jackson, 2013: 12).

Part III focused on external difference, in terms of the ways in which multiplicity and difference were managed when data move outside of the WHAP project. At this juncture, negotiating data involved not only testing the representational veracity of data, but playing with the informational and aesthetic capacities of data. The section also attended to the ways in which new tensions emerge when data are linked with new kinds of phenomena, such as health or policy actions. I use Chapters Six and Seven to illustrate how data shape and are shaped by the forming of connections 'outside' of science.

In Chapter Six, I examined the problem of measuring time in statistical data practices, specifically in the forming of correlations between air pollution and health. I presented a sequence of graphs which were the product of a series of different statistical techniques used by the epidemiologists to understand and make visible the 'right' relations between air pollution data and health data. By making active the different visualisations produced by particular statistical techniques in my analysis of the building of epidemiological correlations, I made visible the material, non-material and dynamic nature of statistical data practices. I argued that statistical data practices were no less material than the measurement practices of modelling and monitoring. The mediated process of sensing and feeling for the phenomena of study took place, instead, through statistical techniques and visual modification. Moreover, whilst these images were made visible 'air pollution and health' as a statistical research object, statistical practices were made visible for ethnographic study. I therefore traced the process of modifying statistical techniques in symbiosis with the adapting of graphs, and indeed participated in the real-time dynamic of making statistical data through re-running STATA to produce the graphs.

What is more, by examining the particular instances of what Goodwin calls ‘professional vision’ (1994), I demonstrated the ways in which such technologically mediated visions extend the boundaries of what visual forms include and implicate (Alac, 2008: 81). For example, in tracing the process of over-laying I showed that particular work with different data enabled different information to be related in new and productive ways. In doing so, I argued that GIS was performative of the research object as a multiple, informational and material hybrid form. Over-laying data produced a physical rendering of multiple data together, whist also making space for analytical work. Specifically, the GIS tool made it possible for different data to be combined in a way that made new kinds of claims to be made about air pollution. The linked and visualised data encouraged new ways of relating to the research object, where the epidemiologists both got a sense of the different data they used to make correlations, whilst also coming to understand the ‘real’ relations of air pollution and health outside of the research setting. The partial nature of data on air pollution overlaps in the statistical space visualised in the GIS device, which was conducive of shifting the research object from air pollution to health.

These statistical methods managed multiplicity and difference in practice, and highlighted the ways in which air pollution as a ‘whole’ was inferred through different data layers and statistical data practices. The GIS tool was one way to manage large and multiple data sets in WHAP, a technological device which enabled the epidemiologists to use and hold together multiple data for statistical analysis. Time was understood as a socially and culturally constructed concept by the epidemiologists, and was used to better understand correlations between air pollution data and health data. ‘The real’ was then used as an external reference, through which the epidemiologists generated a statistical relationship between air pollution data and health data. This had the effect of further revealing the research object, making it more visible in the shape of the graphs. For the epidemiologists, ‘the mutual modification’ (Candea, 2013) of the graphs and the visual representation of data in the graphs were a way of working out how to allow data ‘to speak back’.

These visualisations were also subject to further iterations, extending scientific relations to the conceived ‘policy realm’. In Chapter Seven, I showed that the visualisation of data was also a way to make data do new things, and this involved what at first appeared to be a semantic shift from referring to the output of scientific work as evidence rather than data. I interrogated the distinction between data and evidence made by researchers. It was not, I argued, about the reality of either, nor their particular qualities, but the performance of the distinction between the two that was significant. This was done in two ways. Firstly, through the carrying out of a stakeholder engagement meetings, where invited policymakers participated in the making of ‘relevant’ data which could then be used as evidence in policy. The second way was through the mobilisation of a decision-making tool, which mathematically accounted for, separated and contained ‘science’ and ‘policy’ input. In this way, science retained its status as objective and value free, whilst fulfilling the demands of funders by being ‘relevant’ and ‘having an impact’. I emphasised that this was not merely ‘window-dressing’, but intervened materially in the making of scientific data. As such, I showed that for data to be used as evidence, air pollution (the research object) needs to be linked with the emissions that produced it (external relation) and the effect this pollutant has on the environment or health (the matter of concern).

Because of the gaps and uncertainties in information available, in order to construct evidence on air pollution some types of data and information of air pollution were selected over others. Constructing and shaping air pollution as a science-policy object, then, shaped not only how knowledge on air pollution was made, but also ‘future air pollution’ articulations and, potentially, policy actions. I demonstrated, then, a particular form of ‘Mode 2’ (Gibbons et al., 1994) knowledge production, where knowledge practices are located across disciplines and through which the object of research is made to relate to the context of its application. I showed that the nature of such knowledge production processes introduce new kinds of relations, considerations and accountabilities. With increasing amounts of data, new uncertainties and partialities arise and how to manage and account for these were subject to much debate within the team. Furthermore, researchers on WHAP were very aware of the risks involved in relating their work to policy, ensuring they made the right kind scientific claims about air pollution.

Rather than only looking at how researchers came to know air pollution data, I also accounted for the ways in which they came to ask particular kinds of questions about air pollution. The scientists required input from stakeholders in order to ask the rights kinds of questions about air pollution. Although this process was referred to by researchers as ‘decision-making’, I used the term *selection*, which disrupts the normative, linear model of knowledge flow from science to policy. The act of selection allows for multiplicity and complexity to remain in the foreground. Indeed, air pollution did not become more singular but remained a hybrid form, demonstrated by the trajectory of source-pollutant-effect, where the ‘natural’ (chemistry, weather, particles) and ‘cultural’ (emissions, human behaviour, season) dimensions of air pollution were part of the science-policy object of study. The act of selection, then, shifted air pollution from a matter of fact to a matter of concern, where the multiple constituents of air pollution - data, scientific and non-scientific identities, technologies, instruments and values - were arranged according to socio-technical relations (source-pollutant-effect relations), and it was these connections which influenced what kind of knowledge claims could then be made.

Beyond WHAP: researching data, multi-disciplinarity and the science-policy interface

As the movement of data between researchers on WHAP illustrate, making data and knowledge claims about air pollution was only one part of the process of making data meaningful. Data had to be interpreted, re-worked and, for example, statistically defined, in order for it to be used in the process of knowledge production. On WHAP this was an iterative process, involving different kinds of knowledge and expertise, and the crossing of scientific disciplines and epistemic boundaries. How to manage, synthesise and translate across data practices became an empirical problem, which required understanding how and why data were used, and therefore the root assumptions which underpin particular data trajectories. Organising and managing data were integral to everyday work with data, but also shaped the informational capacities of data and how they were used to make knowledge claims. Studying data practices ethnographically was therefore an opportunity to capture a more complex picture of human-technical relations with data, and the kinds of technological, social and political considerations these data demand. In this final section, I outline three potential contributions of my research: to the study of data as a social and material form; to the practice of multi-disciplinarity as a contemporary mode of doing science; and to the tensions of working in and outside of public health as an anthropologist of science.

A thick description of data practices

Situating my study as part of the increasing amount of research on the generation and use of multiple kinds of data, I point to the multiple practices, politics and selective processes which working with large data sets in local settings entail. An in depth analysis of data practices carries relevance for other scientific practices and processes more generally. I illustrate some of the work involved in enabling data of global phenomena, such as the atmosphere, to co-exist with more particular and local kinds of data, such as point measurements made by DIY instruments. How these different representational forms were managed transformed ways of studying global and local phenomena whilst also re-configuring this distinction altogether. For example, it was difficult to situate air pollution either in local or global spaces because it transgresses both these spatial units, as Choy's quote at the beginning of this section elaborates. Researchers instead worked to configure practical solutions for studying the amorphous object of research in particular instances and for particular means.

That data remain attached to practice is not new for those in science studies, but how data are attached and become detached (to different degrees) to the other kinds of scientific relations within which data are imbricated remains a key interest. As Leonelli (2009) shows, data are embedded within the context through which they are produced, however data can also be made 'non-local' through particular packaging processes. These processes 'enhance the evidential scope of data and ensure that claims about phenomena are understood in the same way across research communities' (Ibid: 745). In WHAP, the concept of the local was rather problematic, for these were multiple. The notion of collaborative research implies the study of a single research object in a shared research setting. If data remain attached to particular practices, then how can data travel between the different data practices which make up contemporary research settings? My ethnographic research shows how researchers on WHAP manage the multiplicity of research objects in practice.

Considering the world as one big data problem (Shaw, 2014), therefore, is rather too simple. Instead, I show that there are multiple research worlds with multiple data problems. Data production and data use are not two separate trajectories, but entangled in complex ways. What air pollution 'is' was very much tied up in the practice of producing data. As a result, when considering data for re-use it was accountable to both the practices which produce it and the practices which re-use it. Unlike the 'packaging' of data in Bioinformatics, where Leonelli found that work took place at the local level in order to free data from its provenance, data's properties, boundaries and meanings were packaged and re-packaged in a constant *tacking back and forth* between local *and* non-local spaces in WHAP. Furthermore, 'data friction' (Edwards et al., 2011) did modify components, and data were not only negotiated but engaged with materially by WHAP members, even if ultimately the modelled and monitored data maintained their shape as distinct entities.

Furthermore, meaning and interpretation took place all along the trajectory of data, from its acquisition to its potential use as evidence in policy making. In this way, the world conceived of as a data problem not belies the internal heterogeneity of, not only data, but also the multiplicity of research worlds which they are assumed to emerge from and map onto. Indeed, talking with my supervisor, other academics and fellow PhD students in the field of public health, I was struck by the similarities between the problem of sharing data on the WHAP project and the issues of access and re-use of other researchers' data experienced more generally. Indeed, that contemporary research problems are orientated around ‘complex problems’, requiring the input of multiple kinds of information and data configurations, means how to turn information into knowledge produced across multiple realms of expertise and practice is increasingly salient.

Multi-disciplinarity in practice

Although I offer a thick description of data practices in one particular multi-disciplinary research project, my analysis carries relevance for science and policy relations more broadly. The ways in which data are made, used and interpreted across different knowledge practices and expertise generates complex and multiple data objects, which perform air pollution as a composite of informational and material relations. By following data as objects, I showed that multi-disciplinarity was both a kind of scientific practice and a sensibility of *doing together*. This distinction was one that had to be made and re-made in particular instances. Even though I was not looking for ‘why’ this team was successful, the role of boundaries in scientific practice, between research practices and through the formation of disciplinary and multi-disciplinary spaces, do highlight some of the ways in which researchers managed tensions and differences, which may be transferable to other projects, settings and ‘inter-disciplines’.

The multi-disciplinary table, for example, was one particular mode of scientific practice, which co-existed with other modes of work, such as individual-focused or closed spaces of practice. That the multi-disciplinary table was not considered as ‘real work’ highlights the reflexive components of multi-disciplinary research, distinguished by WHAP researchers in terms of ‘being scientific’ and ‘being collaborative’. Indeed, I show that single disciplinary working were productive, and were not explicitly ‘anti-multi-disciplinarity’ but necessary for the more explicit ‘working together’ that producing shared data and results. It was, I argued, in the movement, translations and transformation which data undergo where multi-disciplinarity was enacted and materialised.

This mode of sharing data that I outlined previously, in terms of multiple data practices, also sheds light on practices of multi-disciplinarity. I have highlighted the importance of taking into account epistemological and ontological differences, and their boundaries, in multi-disciplinarity, by tracing how these were handled in particular instances in the process of knowledge production. Differences between data were productive and researchers instigated boundaries to work around tensions. This finding also demonstrates another dimension which could be added to the model of 'productive difference', outlined in this final chapter, in terms of the translation and movement of research objects between different research practices, in practice. For data to function as traveling objects in WHAP, researchers reflected on ways to account for difference in creative and respectful ways. In this way, reflexive practices were not simply technical and ad-hoc, but conceptual and theoretical. Rather than focusing only on the material, ad-hoc work of working through 'data friction'(Edwards et al., 2011), I show the more affective dimensions of translation, and the reflexive awareness, care, and compromises which are required for the handling of difference in particular instances.

In WHAP, multi-disciplinarity was not simply a way of dividing labour and delineating research roles, but constitutive of new ways of producing knowledge and materialising objects of research. That the WHAP project was a 'successful' multi-disciplinary team means that tracing how this team carried out research in action may offer insight into achieving multi-disciplinarity in practice. There were shared publications[58] and continuing applications for further funding throughout my time with the project. I am reminded here, of one WHAP researcher's comment in an early interview, during which he explained that working with other disciplines was exciting and necessary, but also that the problems which arose were a constitutive part of the 'trail-blazing' nature of their research. In this explanation, disciplinary boundaries were no longer boundaries which made research difficult, but boundaries which defined the very nature of

[58] There were around ten published papers written by different researchers on WHAP (with several more in the pipeline), along with numerous conference presentations.

the research. Therefore it is at these friction points where new research has to be conducted.

'Collaborative' ethnography [59]

The work involved in working across different research groups was also a point of reflection for researchers, and my own role in the project attests to this internal reflexivity. Indeed, I showed that very often my own knowledge practices were similar to the scientists on WHAP, who carried out their own para-anthropological reflections on multi-disciplinarity. The increasing circulation of social research techniques by non-social scientists is a finding that has been considered as productive for social scientists, in terms of the re-distribution of roles of social research (Marres, 2012a) and in the shaping of the very meaning of ethnographic engagement (Holmes and Marcus, 2008; Marcus, 2013). By following data as objects of research practice, I demonstrate one kind of engagement through which science in the making can be studied and engaged with.

That data were not only objects of interest for researchers, but also for myself, was primarily the way I accessed science in practice, because it enabled me to trace the movement of scientists and their objects between spaces of practice, whilst also following topics of interest, their emergence and development. Following data was also a way to experience the momentum and transformations of research in process. Indeed, the tentative and experimental nature of collaborating led me to consider what the role of the anthropologist is in science, and how an ethnographer could work with these movements and rhythms of data. 'Collaborating' was, then, an opportunity to pose new questions about scientific practice and to draw upon the nuances of everyday working.

By following data I was able to take seriously researchers' own practices, ontologies, ideas and values, because I remained in the mess of everyday practice. In doing so, I was able to provide a fuller and more colourful picture of science in

[59] I want to thank everyone on the panel 'Ethnography as collaboration/experiment' at the EASA biennial conference in Tallinn, June 2014 for helping me develop my thoughts on this mode of ethnographic engagement.

action. Indeed, what I highlighted in Chapters Four to Seven, were the different modes of doing scientific work (theoretical to conceptual, and single disciplinary to collaborative) and the kinds of research spaces these opened up (from the weekly liaison meeting to the ‘offline’ epidemiology meetings). Collaboration was both a process of knowledge production which I studied and a sensibility affecting my own ethnographic engagement. I not only observed the craft-work of science, but participated in the crafting of data for others’ use, the work involved in making data move and the reflexive efforts for making sense of difference.

One of the ethical dilemmas of studying research in process is its inherent instability and uncertainty, where scientists’ own research credibility may be at stake. The difficulty of accessing these sites and the policing of what is shared and not shared with other researchers on WHAP was illustrative of the tentative modes of doing and being collaborative, whilst demarcating the ethnographic contours of my research and methods of engagement. However, by taking on the role of ‘anthropologist’ and ‘collaborator’ I was able to capture the in-between states of knowledge, experiencing the particular roles collaborative science fosters and the experimental shaping of ‘doing together’. Such experience was also informative of the nature and meaning of material work. Data were a way for researchers to circumvent my fieldwork, for example when data were considered too raw or data practices too experimental for ‘outsiders’ to observe. Not only did this highlight the material and epistemic contours of data, but it was in such moments I came to appreciate the sensibilities underpinning the maintenance of ‘good’ and ethical fieldwork relations.

The multiplicity of data and data practices made shifts across temporal, spatial and conceptual scales possible for scientists. At the same time, doing ethnography of data practices was an opportunity to consider the transgressing of scales as something which constituted rather than framed local, situated practices. For data practices are always local, but in their interaction and combination with other data seeing large-scale and long-term phenomena become possible, as ‘participants in the assembly of global knowledge’ (Ribes and Jackson, 2013). As contemporary social, environmental and political problems are orientated around the harnessing of data’s potential, it is important that attention is paid to the ways in which related technologies, ideas and values shape both the experiences of scientists, the kinds of practices and processes these involve, and the particular framings of research problems more generally.

Implications of multiple and heterogeneous data practices for the science-policy interface

I have provided an account of the complex, contingent and multiple nature of doing science with data in a multi-disciplinary public health project. Contemporary institutional arrangements, as well as science-policy knowledge interfaces, have implications on how we come to understand data practices more widely. Indeed, data practices not only play a role in scientific knowledge making, but shape the ways in which evidential claims are made at science-policy interfaces.

The sequential nature of the scientific work reported in my thesis highlights the collecting and production of data, but also the shaping and selecting of data in stabilising air pollution as an environmental health concern. In Chapters Four and Five, for example, I demonstrated the heterogeneous and multiple ways in which air pollution gets done in practice. I then discussed the ways in which this multiplicity was problematic for the epidemiologists, and who as a result attempted to ground this seeming heterogeneity by attaching it to a theoretically singular world. Although researchers found ways of negotiating and coordinating multiplicity and difference in particular instances, so that multiple worlds hung together rather than displacing one another, Chapter Seven marked a rather different approach to studying the complexity of air pollution. The multiplicity found in scientific data practices were negated, and the singular world which seemed to frame policy practices ultimately influenced what counted as 'evidence' of air pollution.

It is this latter finding which I want to develop here. If the complexity of scientific matters of interest, experiment and negotiation keep heterogeneity at the fore, yet these are reduced to homogeneity at the science-policy interface through decision-making and selections, then what is lost? Subsequently, how can policy discussions keep heterogeneity and difference in the foreground? One way in which to think about this is by examining how the concept of 'evidence' gets used, and for what means, in practice. As I traced, evidence was a status given to particular kinds of data relations, where the source of a pollutant, the pollutant itself and its impact were measurable as discrete entities. In this case study, the relation between these different elements could then be considered in causal relation to one another. Perhaps it is this notion of causality when negotiating policy knowledge that lies at the bottom of the seeming discrepancy between scientific data practices and policy data practices.

Keeping this notion of causality in mind, I am going to briefly outline the implications of what potentially multiple and heterogeneous policy making data practices may mean for STS engagements with science-policy interfaces more generally, beyond WHAP. The stakeholder engagement meeting was an opportunity to consider the ways in which framings of air pollution change and emerge as do-able problems. If causality was considered in terms of source-pollutant-effect for the stakeholder meeting on the WHAP project, what would happen if science-policy interfaces made mess acceptable and 'do-able'?

First, a different kind of relationship would have to be configured, over and above a causal one, if other kinds of relations are to be taken into account when making policy claims. One way to think about this is to go back to the messy entanglements of scientific data practices which I discuss in Chapters Four to Six. The scientist-instrument-data relations used to configure air pollution as a scientific entity could, then, perhaps become the space where decision-making and air politics play out. This would mean that rather than what counts as a causal relation, the contingent and particular arrangements of researchers, 'the public', technologies and *feelings for* air remain in focus, as part of policy action.

If policy-making data practices took seriously the different human and non-human relations which make ways of knowing and experiencing air possible, then what gets defined as polluting in the air would extend beyond a human action and a chemical reaction to the different kinds of agencies which make such 'casual effects' possible to construct in the first place. For example, the question would not be what data relations can be held together in a stable, linear way, but instead, what the arrangements of different data enable in terms of new ways of thinking about and materialising air, spaces of air and what counts as polluting in particular instances. Second, and in doing so, just how to capture air and what counts as air and air pollution may be a productive outcome of discussions rather than assumed or taken for granted. Some of the more nuanced ways in which air is encountered, breathed and known by those capturing, collecting, but also experiencing air as breathers could then be brought into policy discussions. Accordingly, rather than only ‘cause’ and ‘effect’, an encounter with air could be used as ‘data-as-evidence’, thereby capturing airs in process and becoming rather than as a form to be measured in linear time and Cartesian space. This shift in focus could open up new repertories of action, composing an alternate set of relations from that of the source-pollutant-effect, and through which different kinds of interventions with the air we breathe taken. In doing, so ‘who’ has agency and the power to speak would extend the people and sets of relations which often get mobilised in policy spaces, as exemplified in the WHAP stakeholder meeting.

Not defining policy’s remit and interest in terms of linear, measurable problems does also have implications on the status of science, however. For example, not separating scientific data practices and policy data practices means the boundary work and maintenance between these worlds cannot take place. This would make ‘the science’ less clean because scientific interests and non-scientific interests would not be made distinct from one another when actionable claims are articulated. The status of claims from research would then carry different weight and meaning, because, for example, the way in which science is used for making social, economic and political claims would be explicitly entwined. So, including mess and non-linear relations in policy making will have implications on practices of ‘evidencing’ with data, but also local scientific practices and data production.

Implications of multiple and heterogeneous data practices for multi-disciplinarity

I’ve shown that data both enable and constrain multi-disciplinary working. What is significant here is that studying air pollution, or other complex research objects understood as requiring multiple kinds of data, always involves a re-configuring of data and data practices. I detailed the way in which producing modelled data, for example, was shaped through encounters with epidemiological data practices and fieldwork data collection. Data are not holistic entities; their meaning, effects and affects always exist in relation to other material and non-material forms. So, for example, what counts as an epidemiological measure of air pollution - such as an air pollutant as a statistical variable - was contingent on measures of statistical error, the atmospheric chemists' conceptualisation of atmospheric conditions and the environmental chemists' knowledge of chemical species.

The re-configuring of data relations was, then, technical and instrumental, but also social, cultural and performative. It was not only information that changed the way in which scientists came to understand air pollution, but the active engagements of scientists with data, who became attuned to different kinds of air pollution realities and practical engagements with the world through sharing data. By considering data as a relational form and particular kind of researcher attachment, the way in which scientific identities and relationships between research teams were made also came into view. What may seem like technical, ad hoc work, for example, may also be the playing out of multi-disciplinary tensions and the building of research teams, and therefore the shaping of trajectories for future knowledge making. It is these 'collaborative moments' which are significant to science and scientific knowledge as an emergent social and cultural form. For how scientists make and sustain relations and research worlds is significant to how science and policy comes to shape and intervene beyond these spaces of knowledge making.

The concept of cross-disciplinary research, whether 'inter', 'multi' or 'trans' (Barry, Born and Weszkalnys 2008), refers to a type of approach in scientific research which involves combining, to varying degrees, the skills, knowledge and expertise from different disciplines. Policy makers and research councils increasingly advocate for cross-disciplinary research as a strategy for tackling contemporary problems considered too complex for a single discipline. As Fielding (2010) notes, combined methods are seen as giving policy makers a fuller understanding of the complex effects of policy interventions, and moves amongst policy makers include increasing emphasis on mixed methods in invitations to tender for research. For example, in a working paper on training researchers to conduct inter-disciplinary research (Lyall and Meagher, 2012), 'inter-disciplinarity' was described as an ontological and epistemological challenge, resulting from the problematisation of issues as complex, interconnected, contradictory, located in an uncertain environment and embedded in landscapes that are rapidly changing. Crossing disciplinary boundaries and working on 'the borderlands' (Strathern, 2004) is risky because these spaces of knowledge production are fundamentally uncertain. By focussing on data practices I have highlighted some of this work at 'the borderlands', delineating the ways in which data practices not only constitute bur are also a means of transgressing and re-defining these borders.

Funding for cross-disciplinary research collaborations is shaping the nature of researcher encounters and ethnographers, and other social scientists, are increasingly becoming co-collaborators on scientific research projects. I've shown that social and cultural studies of science can play an important role in examining the challenges and consequences of some of the complex ways in which data configure and transform the ways in which science is practiced. In addition, the taking seriously of researchers' objects of research informs anthropological ways of seeing, studying and coming to know social worlds of research. Studying science as embedded within particular networks of knowledge production, whilst also connected to larger configurations of informatting and quantifying phenomena, makes visible the often novel kinds of social and cultural relations which are being forged through data and data practices.

Summary: making air pollution visible

> Visibilities are systems. Here the seen was read into the said. But equally, the visibility can function to produce the unseen along with the seen. It is the nature of visibility, particularly modernism's historical variant, to operate though its others, it shadows, its residues, its unprocessed remnants, and its highlighted icons. Differences provide the very possibility of signification, in visual as well as discursive registers (Daston, 2007: 372-373).

I have shown that both my own ethnographic practices and the practices of researchers on WHAP were orientated around the task of *making air pollution visible* - as a shared research object, in different kinds of inscriptions, as statistical forms and by grounding it to a ‘real’ concern, health. I also take Choy’s (2012) suggestion, of not thinking about air, or research objects, as solid forms, seriously, in order to consider the particular sensibilities which emerge in the process of sharing and re-using data, and to therefore reveal the affective dimensions which *doing together* engenders.

Daston argues that visibility is not just about the seen, but also the unseen, visibility’s shadow. With reference to the shifting meanings and materialities of objectivity, Daston suggests that what counts as knowledge and fact, or indeed, in our case here, data, can be considered in visual as well as discursive registers. To start a conclusion, this quote seemed rather fitting in its capturing of the insubstantial, partial and emergent nature of studying air pollution. The emergence of visibility was both a challenge for myself as ethnographer, and for other researchers working on a multi-disciplinary and multi-sited project, where practices, material output and meaning were often hidden or limited. I have argued that, in fact, knowing air pollution is only ever partial, each addition of new information and data opening up new questions, relations and complexities.

In each chapter of this thesis, I have undertaken a process of further revelation of the research object ‘air pollution’. Starting with multiplicity, I show the different ways in which techno-scientific practices make air pollution visible as measurements. I then move on to difference, for if there are multiple ways of making the research object air pollution visible, then which representation is most truthful? Yet, I found, and as Daston also points out, that difference was productive, signifying not only different perspectives but new particularities of air pollution. Rather than which kind of data more truthfully represented air pollution, the very nature of the problem of researching air pollution was reconfigured. The visual techniques used by the epidemiologists further enhanced air pollution, which was understood as making the relations of interest ‘more visible’. Furthermore, the act of selection made air pollution visible as a political problem, by grounding it to prescriptive actions, and through which air pollution's effects could be measured, and thereby discursively visualised.

Visibility, then, is not only a quality of air pollution as scientific phenomena, but also shapes the very ways we come to study and know it. Tracing the often difficult to access and hidden dimensions of scientific practice, led me to consider what the act of revelation, sharing, and the work of *doing together*, really involved. Another layer of visibility, which my study of science in practice reveals, therefore, is the often invisible dimensions of science, and scientific sensibilities which emerge and take form over time. The production, movement and materialisation of data was a way to share different kinds of information, but were also the means to argue, make claims and incite action. Data also functioned as a form through which scientific relations were established and sustained. Indeed, the WHAP project was one of several other 'collaborative' projects involving WHAP researchers, and during my fieldwork more successful bids were funded. As a result, rather than finding a multi-disciplinary project made up of tension and difference, I found a much richer amalgamation of what doing contemporary science looks and feels like.

The heuristic of 'tuning' emerged from my field research, as the practice of amending and tweaking an instrument in order to make a better measurement of air pollution. Tuning is also a useful way to describe the small, mundane everyday practices which make up multi-disciplinary scientific practice. Indeed, tuning suggests a mutually modifying kind of relationship, highlighting the adjustments between researchers, and between researchers and their objects. Tuning, therefore, can be at once technical, material and affective, through which, for example, compromise, care, responsiveness and sensitivity emerge and take form. When multi-disciplinarity is considered a necessity for understanding and governing air pollution, and when ethnographers are tied up financially, academically and ethically within these research practices, the nature of the problem for all those involved also shifts. As Kaushik Sunder Rajan encapsulates so well: 'no longer was the question: 'how do we generate information to create meaning?' but rather 'how do we sift through the information we have to create meaning?' (2012: 23). Thus, as ethnographers, how do we make sense of something, when the very thing to be studied is both the problem and what is at stake?

I've attempted to tackle this instability of working out what it is that one is in fact looking at when studying an invisible research object, as a problem for both ethnography and science in practice, by following data as the relational entities and animated forms through which science takes shape. In doing so, this thesis offers a thick description of the socio-material realities of contemporary scientific practice. I pay heed to the different layers of visibilities, their ebbs and flows, which data practices make possible, whilst also foregrounding the fragmentary, uncertain and in-between processes, movements and transformations which are sometimes hidden from anthropological and STS accounts of science in practice. By bringing to the fore the affective dynamics of 'sharing' and 'doing together', which current academic research structures encourage and commend, my ethnography points to some of the ways in which wider scientific structures shape the formation of scientific research objects in practice. An ethnography imbricated within practices and processes of scientific knowledge production exemplifies these complex entanglements of epistemology, ontology and technical practice. Emphasising the tentative tuning of research bodies themselves, from representing to enacting, I foreground the scientific sensibilities which make up, and give life to, research worlds-in-the-making.

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