SPACE, RESEARCH OBJECTS, AND INTERDISCIPLINARITY IN GEOMORPHOLOGICAL INQUIRY

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A thesis submitted in partial fulfilment of the requirements for the degree

of

Doctor of Philosophy

Michaelmas 2016

This thesis is dedicated with thanks to

Roy, Ann, and Hari

and to the memory of

Carolyn

ACKNOWLEDGEMENTS

I would like to acknowledge the following, with apologies to anyone who should have been mentioned but was left out. Any errors or omissions in the thesis remain mine.

Firstly, I owe a huge debt of thanks to those who participated in this study. Particular thanks are due to Professor Heather Viles and members of the Rock Breakdown Laboratory at the School of Geography and the Environment, University of Oxford. Further thanks is due to the researchers who so thoughtfully and generously engaged with my questions during interviews.

Secondly, thanks are due to my supervisors, Professor Andrew Barry and Dr. Richard Powell. I am highly appreciative of their patient guidance and support throughout the doctorate. Further thanks is due to Ruth Saxton, the Co-ordinator for research degrees at the School of Geography and the Environment, to the members of the academic staff who gave feedback on the doctorate at different stages, and to the academics who have served as Directors of Graduate Studies during the period of my research. Thanks also to Professor Roy Dilley, Professor Nigel Rapport, Professor Daphne Hampson and Professor Alan Torrance – your encouragement during my undergraduate studies and your support since has been of decisive importance for me. Sincere thanks too to Dr. Alirio Parra, Dr. Stephanie Schwandner-Sievers and Professor Garry Marvin for your kind guidance and encouragement as I returned to academic study.

Particular thanks is due to Sarah O'Brien, the academic administrator at Kellogg College, and to Dr. Andrew Simpson and the other academics who have served as Senior Tutors during my studies. When disability threatened to get in the way of my work I relied on you for unstinting support. My heartfelt thanks. Further thanks to Dr. David Mills for being such a thoughtful and kind college advisor – I hope perhaps in the future to see you in college for a bowl of soup!

Likewise, at the Disability Advisory Service, Julia Hill, Jane Clark, Deborah Popham, and, more recently, Helen Young have offered me support, practical guidance and a listening ear - thank you. Sincere thanks too to Tim Jones who assessed me while various medical diagnoses were still sketchy and helped me to get the support I needed.

Similarly thank you to Dr. Silvester and to the specialists who have offered care and skill in getting to the bottom of the challenges I've faced and for setting me on a path to overcome them. A particularly big thank you to Mark Wilson, the HEMS team and the neurosurgical team at the Royal London. I wouldn't be here - never mind presenting my doctorate – without your skill and dedication.

Sincere thanks are due to the ESRC who generously supported me with a 1+3 studentship (ES/I025472/1) through my MSc and the first three years of my doctorate. Grateful thanks too to Morag McCormick-Power and Daniel Meacoe at the Social Sciences Division, University of Oxford – your help made such a difference.

Last, but decidedly not least, thanks is due to my family and friends who have been there through the ups and downs. Mum, Dad, and Hari you have been my cheerleaders, you've given me every possible practical and emotional support, you've shared the many

moments of joy, one or two (!) of the opposite - you've picked me up when I've stumbled. Thank you. To family scattered around the country and the world – thank you for your patience and loving support. I'm sorry I've neglected you and I'm looking forward so much to meeting up. Finally, thank you to Carolyn – for sharing week by week goings on big and small. We miss you.

ABSTRACT

This thesis considers (inter)disciplinary spaces and research objects associated with geomorphology. Employing an ethnographic methodology, established Actor-Network Theory (ANT) themes are examined; the study considers the displacement of research objects amid the spaces of scientific inquiry and the role played by a range of actors in enabling this displacement. However, these well-known themes are given new impetus; the thesis seeks to address deficiencies in ANT accounts of space and it offers an alternative approach to conceptualising scientific research objects. Building on this analysis it considers how the spaces of geomorphological inquiry are reworked to accommodate interdisciplinary research objects. In this way, the study provides a strong theoretical contribution to the literature. However, this theoretical contribution is complemented by an important empirical base. The study is one of only a few Science and Technology Studies (STS) analyses to consider the geosciences. Focusing on late twentieth and early twenty-first century geomorphology, it provides an insight into an interdisciplinary field of research found in both geography and geology departments. In this way it offers an important contribution to the histories of geography literature and the literature on interdisciplinarity. Relatedly, through its attention to the interdisciplinary field of biogeomorphology, the study provides a bridge between existing STS literatures on the biosciences and nascent explorations of the geosciences.

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INTRODUCTION

In 1807, the naturalist Georges Cuvier argued, paraphrased in Outram's words, that 'the only real voyages of exploration into nature take place in the sedentary naturalist's study' (1996: 261, see also Livingstone 2003: 40-41). These words find echo in Latour's comment that 'I have never followed a science, rich or poor, hard or soft, hot or cold, whose moment of truth was not found on a one- or two-metre-square flat surface that a researcher with pen in hand could carefully inspect' (Latour 1999a: 53). Latour, however, only shifted to consider field sciences (1999a) twenty years after his earliest work on 'laboratory life' (Latour and Woolgar 1979) and, even then, with an imaginary dominated by the idea that a science's 'moment of truth' is essentially *indoor* (1999a: 53). However, by retracing our steps, in this introduction, to the moment of Cuvier's comment, it is possible to sense an alternative set of threads in the history of the sciences: through attention to the specificity of the field sciences, and, in particular, the geosciences, alternative conceptions of space, (inter-)disciplinarity, and research objects may be deciphered. Such conceptions do not seek to relocate the 'moment of truth' of a science from the museum or laboratory (indoors) to the field (outdoors); instead they challenge such insidious binaries. However, this is leaping ahead; first, it is important to ponder the 'geo' and the spatial and disciplinary relationships that shaped early work in what would now be termed the 'geosciences'.

Although there have been a number of important studies of the history of the geosciences (Rudwick 1976, 1985, 1996, 2005, 2008, 2010; Secord 1986, 1991, 2014; Oreskes 1999, 2001), there are remarkably few studies of the late twentieth and early 21st century geosciences. To be sure, there is a rich strand of contemporary work theorising the 'geo' (Clark 2010, Clark 2013, Yusoff 2013); however, such work is yet to engage empirically with those sciences that have, since at least the eighteenth century (Guntau 1996), been quietly attending to the 'Gē (Earth)' (Conway 2016). According to Guntau, natural history 'changed its nature' during the eighteenth century; 'the study of the three realms of nature that it had contained – minerals, plants and animals – developed into mineralogy, botany and zoology' (1996: 228). Rudwick explains that 'By the late eighteenth century, mineralogy was already far wider than the modern science of the same name, because it encompassed the geographical dimension of the science of the earth' (1996: 271). Similarly, Guntau explains that 'Eighteenth-century mineralogy included geognosy, from which geology would be developed' (1996: 228). The term 'geology' was 'coined in the decades around 1800' (Secord 1986: 9). Rudwick notes that:

> The nineteenth-century meaning of 'geology' was even wider than the modern, because it usually embraced the older 'mineralogy', and also such specialities as palaeontology and geophysics (not named as such until later): in effect, it was close to the modern phrase 'earth sciences' (1996: 481).

This study focuses on a geoscientific discipline that likewise has its roots in the eighteenth and nineteenth centuries (Tinkler 1985), with its name only being given in the second half of the nineteenth century (Gregory and Goudie 2011: 1): the field of geomorphology.

Geomorphology is chosen as the focus for this research for a number of reasons. Firstly, as a field science it provides an alternative way in to the conceptualisation of space. Secondly, by engaging with a contemporary geoscience the study addresses an important empirical gap in the literature. Thirdly, geomorphology is an interdisciplinary field; this means that the study engages productively with, and contributes to, the literature on interdisciplinarity. Of particular interest, in this regard, is the field of biogeomorphology which forms the focus for chapter seven. This area of research considers both ecological and geomorphological processes. In addition to providing an interesting interdisciplinary case study, this type of geomorphology provides an interesting bridge across which science studies can be extended from the familiar ground of the biosciences into the less familiar geosciences. Finally, since geomorphology often functions as a sub-discipline of geography, its investigation provides an opportunity to contribute to literatures on the history of geography. Similarly, the ethnographic research provides a chance to participate in ongoing dialogues 'across the divide' (Harrison et al. 2008) that often, though with notable exceptions (Lorimer and Spedding 2002, Harrison et al. 2008, Harrison et al. 2004, Lane et al. 2011, Whatmore 2013), separates physical and human geographers.

Before considering recent and contemporary geomorphology in its specificity, this introduction briefly stays in the eighteenth and nineteenth centuries to further consider the spaces of the evolving geosciences.

SPACES OF GEOSCIENCE

The literature on the early nineteenth century earth sciences is marked by a division between the field (outdoor) and the museum or academy (indoor). For example, Rudwick explains that in the years before Cuvier's critical discussion of outdoor science, earth scientists were 'insist[ing] that fieldwork was indispensable, not just for collecting specimens … but for seeing with one's own eyes how the various minerals and rock

masses were spatially related to one another and to the physical topography of the areas in which they were found (Rudwick 1996: 271). Indeed, 'By the early 1800s, demonstrable fieldwork experience had become an essential qualification for any author wishing to be taken seriously in this kind of science' (Rudwick 1996: 481).

Cuvier himself, resident at the *Muséum d'Histoire Naturelle* in Paris, substantially contributed to the development of the science of geology. In the preface to his four volumes *Researches on Fossil Bones,* entitled 'Preliminary Discourse', he 'suggested that geologists could and should aspire to "burst the limits of time" by reconstructing in reliable detail what he conceived was a vast expanse of *prehuman* geohistory, dwarfing the totality of subsequent recorded human history' (Rudwick 2010: 12, emphasis original). Like Outram (1996: 259-261), Rudwick notes that Cuvier was 'an indoor museum naturalist with little firsthand experience of the relevant field evidence' (2010: 13). However, according to Rudwick, his 'picture of geohistory had come from his joint fieldwork with, his near-contemporary, the mineralogist Alexandre Brongniart (1770-1841)' (2010: 14). This, 'almost the only geological fieldwork that Cuvier ever did' (2010: 14), was of significant importance; 'Cuvier and Brongniart had recognized that their research on the Paris Basin could also play a crucial role in the reconstruction of the whole sweep of global geohistory' (2010: 15).

It was in 1808 that Cuvier and Brongniart published their *Essay on the mineral geography of the Paris region* (Rudwick 1976: 127). Therefore, according to Rudwick, even Cuvier, who had argued for the pre-eminence of indoor science just a year earlier, found fieldwork, albeit of a highly local character, to be decisive. Indeed, the geomorphologist, Michael Church, has pointed out that Cuvier recognised the importance of fieldwork. Church explains that in 1810 Cuvier echoed the emphasis on fieldwork advocated by his contemporary Saussure:

> … Horace-Bénédict de Saussure (1740–1799) [was] a wealthy citizen of Geneva who undertook to map and describe the physical geography of the Alps, culminating in August, 1787 with an ascent of Mt. Blanc with instruments, such as thermometers, barometers, and an 'electrometer' (to measure static electricity). Saussure asserted the primacy of field work in 'physical geography' which, as he and his contemporaries practiced it, could largely be equated with 'physiography' or descriptive geomorphology, notably in his influential *Agenda*, published in the Parisian *Journal des Mines* in 1796. This was a prospectus for the conduct of the science, *forcefully echoed in the 1810 report by Georges Cuvier (1769–1832) for*

> *the Emperor Napoleon on 'the progress of the natural sciences.*' Cuvier was permanent First Secretary of the Institut de France — the post-revolution successor of the Académie Royale — senior researcher at the Muséum d'Histoire Naturelle, Professor at the Collège de France, and the most influential naturalist of his day. *His report marked the triumph of field orientation in the pursuit of Earth science and the formal institutionalization of that attitude* (Church 2013: 185, emphasis mine).

It is not clear then that Cuvier's view of the value of fieldwork was as negative as has been assumed from his critical discussion of Humboldt's report (Outram 1996: 259-261). Further, many contemporary geomorphologists would contest Latour's view that the 'moment of truth' of a geoscience[1] is found on the interior space of a flat bench; Slaymaker (2009: 329), for example, has argued that 'most geomorphologists agree with' Bretz's contention that that 'field relations are the final court of appeal' (Bretz 1962 cited in Slaymaker 2009: 329, see also Powell 2008a: 550). The point, however, in attending to field sciences, is not to relocate the 'moment of truth' from academy[2] to field. Instead, it is to move to a richer conception of space that escapes the binary of field versus academy (see Lorimer and Spedding 2002: 298).

Actor-Network Theory (ANT) conceptions of space replicated the imaginary of the field remote from, and opposed to, the academy, the latter described in terms of a 'centre of calculation' (Latour 1987: 215). Later forms of ANT have sought to overcome this 'narrow imperialistic template' (Barnes 2004: 577), characterised by an 'inappropriately rigid and centred version of relations' (Law and Singleton 2005: 341). However, as argued in chapter two, these accounts have not relinquished the vestiges of Euclidean accounts and are therefore unable to do justice to the rich meshwork of relations constituting the

[1] Outram argues that Darwin was 'probably the last man of science' who combined work as a 'field naturalist' with 'theorization about the order of nature' conducted 'within built environments' (Outram 1996: 259). It is unsurprising that, as someone who identified himself as a geologist (Greene 2009: 666), Darwin undertook extensive fieldwork; nor is it strange that he combined this with work undertaken indoors. More surprising, however, is Outram's claim that he may have been the last man of science to combine indoor and outdoor science. Oreskes describes a decline in geological fieldwork occurring in the 1960s (1999: 290); presumably between Darwin's return from the Beagle expedition in 1835 and debates concerning geological fieldwork in the 1960s a good many geologists followed Darwin's lead in combining indoor and outdoor science. As seen in chapter four, in geomorphology, fieldwork continues to be a valued part of many academic careers.

[2] Within the STS tradition the dominant indoor space investigated is that of the laboratory (Latour and Woolgar 1979). However, as Livingstone points out, 'Alongside, and indeed predating, [the laboratory] were spaces of accumulation, like the museum, in which specimens were collected and organised according to the prevailing norms of the time' (2000: 288). The term 'academy' is intended to encompass both spaces.

spaces of contemporary scientific inquiry. By engaging with a field-orientated geoscience an alternative account of space may emerge; however, it is one that challenges the field/academy binary and the Euclidean account of space underpinning it.

According to Driver, the field is 'necessarily a more open and diffuse space than the study or the laboratory' (Driver 2000: 268)[3]. Similarly, it is 'inhabited differently' than its 'intramural counterparts like the laboratory or museum (Livingstone 2003: 42). However, it is problematic to assume that fieldwork is inherently more characterised by 'practices of place' than the apparently 'placeless practices of the laboratory' (Kohler 2002: 192). This is most obviously the case where the lines between the field and the laboratory, or study, are blurred; for example, laboratory simulations and numerical models may seek to replicate the particularity more usually associated with a field site, and, conversely field experiments may seek to replicate the uniformity and repeatability more obviously associated with the laboratory. However, even beyond such cases, it is false to assume the laboratory is inherently less characterised by particularity; like the field, the laboratory is potentially subject to contingencies associated with the layers of human and non-human activity that produce it as a space. This is indicated in Hacking's description of a laboratory that 'matures' as it develops 'a body of types of theory and types of apparatus and types of analysis that are mutually adjusted to each other' (Hacking 1992: 30). Indeed, as Kohler admits: 'In reality, laboratories are not a universal space: they vary, and even "standard" instruments and practices are local and particular' (2002: 194). To paraphrase Driver, the spaces of the laboratory, or of a numerical model on a computer screen, are, like the field, 'not just "there"' (see Driver 2000: 267); instead, laboratory and fieldwork are *both* 'indelibly marked by the local and the spatial circumstances of [their] making' (Shapin 1998: 6).

If laboratories are not 'universal' opposites to the spaces of fieldwork then another type of entity is deemed to be: texts. As discussed in detail in chapter four, Latour makes this assumption in his discussion of the transformations that materials undergo as they are distilled from the 'particularity', 'locality' and 'materiality' of the field to the 'text', a category listed alongside terms such as 'relative universality', 'circulation' and 'calculation' (Latour 1999a: 71). It is true that texts are portable and can circulate across physical distances whilst remaining intact; they function as 'immutable mobiles' (Latour 1987: 229). However, they are also highly specialised and will only make sense in specific situations. Again, Hacking provides an indication of the way ahead when he argues that: 'what

[3] Though see Greenhough (2006: 235) for a discussion of the genetics 'island-laboratory' of Iceland. Greenhough advocates a shift from understanding 'Iceland as a laboratory' as a 'closed space of analysis' to considering it as 'an open contested site of scientific and social exploration'.

meshes … is … a network of theories, models, approximations, together with understandings of our instruments and apparatus' (Hacking 1992: 30).

It is also important to pay attention to the work texts do when they enter specific situations. It is interesting to note that texts often come into play in the field. An anonymous editorial in *Nature Geoscience* explains that:

> When he set forth on his first voyage aboard the HMS Beagle, [Darwin] was armed with Charles Lyell's contentious book Principles of Geology, as well as his specimen collecting equipment (Nature Geoscience 2009: 81)

In a more humble example, evoking hidden[4] stories of geography, Lorimer describes a copy of a book entitled *Cairngorm adventure at Glenmore Lodge* that would be packed into the minibus for an expedition recreating a 1950s field excursion:

> The scribbles and doodles speak of placed practice and personal experience in Glenmore. Clearly this book travelled: it is a veteran of journeys made beyond the city limits. At some stage library staff were forced to make running repairs. Its spine was rebound, a new, sturdier cover fitted and numerous torn pages taped up – though little could be done to protect fragile corners worn soft by thumbs. Were these remedial measures to withstand repeated transit as the final item jammed into a full backpack? (Lorimer 2003: 281)

Finally, in an interview with Paul Merchant, explored in more detail in chapters five and six of this thesis, geomorphologist Richard Grove commented on the fact that books were taken into the field on a 'family expedition' to the Alps in 1958: 'The car was always overloaded with books as well as children and equipment and suffered accordingly' (Grove 2010: 184). Clearly texts, as well as maps (Latour 1999a: 28) and models (see chapter four) often come into play in the field. Therefore, attention needs to be paid to the work such entities do in shaping the spaces of scientific inquiry; this queries the polarisation of the 'materiality' and 'particularity' of the field and the 'universality' of the text.

A series of polarised oppositions are present in the literature: field versus museum or laboratory, outdoor versus indoor; placed versus place-less; particular versus universal; and, relatedly, material versus text. In thinking about the spaces of geomorphological inquiry this thesis questions such insidious binaries and the Euclidean account of space underpinning them.

[4] See Norcup 2015 for a discussion of another 'hidden' (p. 64) aspect of geography: an analysis of *Contemporary Issues in Geography and Education,* a 'school-teacher initiated' journal that was published from 1983 to 1991.

> The word geomorphology … literally means 'to write about (Greek *logos*) the shape or form (*morphe) of the earth (*ge)' (Gregory and Goudie 2011: 1)

Having given initial consideration to the spaces of scientific inquiry it is necessary to return to consider geomorphology and the research objects that form its focus. Geomorphology is a geoscience that studies the changing landscape of the earth's surface. To do this, as indicated in the title of a leading journal, geomorphology investigates *Earth Surface Processes and Landforms (ESPL)*[5]. As such, the research objects of geomorphology are, to borrow Law and Singleton's phrase, 'not the kind of object[s] you can drop on your toe'. The home page of *ESPL* points out the journal's 'Foci include the physical geography of our river, valley, glacier, mountain, hill, slope, coast, desert and estuary environments; alongside responses to Holocene, Pleistocene or Quaternary environmental change' (John Wiley & Sons Ltd. 2016: n. p.). The evolution of these landforms dwarf humans both physically and temporally (Church 1996: 147-148). Meanwhile, the processes responsible for landscape change, such as the erosion, transport and deposition of landscape materials, may be measured through their effects but they are not entities that can be handled.

How then to gain traction on these research objects? Despite a predominantly critical engagement with the French sociologist's writing, this thesis draws on Latour (2000: 260) to argue that research techniques may be considered as both a component of, and proxy for, geomorphological research objects. Latour argues that:

> We do not have to consider physical entities such as ferments, germs, or eggs sprouting into existence as being radically different from a context made up of colleagues, emperors, money, instruments, bodily practices, etc. … [this] has the great advantage of requiring us to stabilize neither the list of what makes up nature nor the list of what makes up context (2000: 260).

Similarly, since research objects are understood relationally, it is not necessary to differentiate between research objects and a '… context made up of colleagues, emperors, money, instruments, bodily practices, etc. …' (2000: 260).

This emphasis on the relationship between research objects and their 'context' relates to Daston's argument that research objects are 'real and historical' (2000: 3). Latour

[5] The journal of the British Society for Geomorphology (BSG).

contributes a chapter[6] to Daston's, *Biographies of scientific objects*; however, notwithstanding the title of this edited collection, there is a surprising lack of attention to the history of specific research objects or the scientific techniques associated with them. In his belated study of field scientists (1999a) Latour comments:

> Remove both [their] maps, confuse cartographic conventions, erase the tens of thousands of hours invested in Radambrasil's atlas, interfere with the radar of planes, and our four scientists would be lost in the landscape and obliged once more to begin all the work of exploration, reference making, triangulation, and squaring performed by their hundreds of predecessors. Yes, scientists master the world, but only if the world comes to them in the form of two-dimensional, superimposable, combinable inscriptions. It has always been the same story, ever since Thales stood at the foot of the pyramids (Latour 1999a: 29).

With a wave of the arm, Latour rightly points out the history of techniques encountered during participant observation. However, his curiosity regarding these histories is weak; this general statement is the only attention they receive. This study seeks to tackle this conceptual and empirical deficit; it does so by attending to the distinctive histories, not only of institutions and disciplines, but of specific research objects and associated research techniques.

INTERDISCIPLINARITY

It is noteworthy that, notwithstanding his disciplinary affiliation as a Professor of Comparative Anatomy (Sapp 2003: 11), Cuvier's research also contributed to the emerging geosciences. Rudwick explains that Cuvier and Brongniart's 1808 *Essay on the mineral geography of the Paris region* 'incorporated two innovations of great importance' (1976: 127). The first of these discoveries is described by Rudwick as follows:

> They distinguished seven major formations above the Chalk, easily separated from one another by their general lithography. However, within a single one of these, the *calcaire grossier* ('coarse limestone') … they found a constant order of superposition of individual strata over distances of more than 120 km. This remarkable constancy they were able to recognise by noting the precise nature of the fossil species in each stratum … The fossils of successive strata were not wholly distinct; but those characteristic of one stratum tended to be less abundant

[6] According to Arabatzis, Latour's chapter shares and develops Daston's 'metaphysical framework' while the latter 'is not adopted … by most of the other contributors' to the book (Arabatzis 2003: 441).

> in the next, and to be gradually replaced by a different set of species. What they had discovered, in effect, was that fossils could be used not merely to characterise in general terms a whole formation of strata, but to identify in much greater detail the individual strata within a formation (1976: 129)

The later example of Charles Darwin[7] is also instructive. According to Greene:

> Darwin 'the man' was a geologist and naturalist who travelled extensively in South America, and on his return published fundamental work on the origin and distribution of coral reefs, volcanic islands and the uplift of mountain chains, particularly the Andes. In addition, his work with fossils led to substantial contributions to vertebrate palaeontology … Darwin was always attracted to the fastest-moving aspects of geology … Thus [this work] prepared him for his shift in focus to the living world, and was an extension of the same imaginative structure and method of work that produced his best work within his scientific first discipline … Darwin was first and foremost a geologist[8], but he also made quite substantial contributions, under the general designation of naturalist, to botany, invertebrate zoology, palaeontology, entomology, primatology, psychology, soil science, and a variety of other fields that are today carefully distinguished from one another. He did all this in addition to his best-known work *On the Origin of Species,* which was subsequently appropriated by biologists (2009: 666-667)

These were scientists whose research transcended still emergent[9] disciplinary boundaries between the geo- and the bio-. Some of the scientists who participated in this study are found, more than a century later, to be reworking the spaces of scientific inquiry to

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[7] It is surprising that Outram argues that Darwin was 'probably the last man of science' who combined work as a 'field naturalist' with 'theorization about the order of nature' conducted 'within built environments' (Outram 1996: 259). It is unsurprising that, as someone who identified himself as a geologist (Greene 2009), Darwin undertook extensive fieldwork; nor is it strange that he combined this with work undertaken indoors. More surprising, however, is Outram's claim that he may have been the last man of science to combine indoor and outdoor science. Oreskes describes a decline in geological fieldwork occurring in the 1960s (1999: 290); presumably between Darwin's return from the Beagle expedition in 1835 and debates concerning geological fieldwork in the 1960s a good many geologists followed Darwin's lead in combining indoor and outdoor science. As seen in chapter four, in geomorphology, fieldwork continues to be a valued part of many academic careers.

[8] Though Secord notes that following his earliest training at Edinburgh Darwin's 'firm grasp of strata and mineral identification … could not compare with his expertise in the field of invertebrate zoology' (1991: 138); however, by the time he embarked on the Beagle Darwin had undergone further training in geology and 'letters to his sisters confirm that he identified himself as a "geologist"' (1991: 133)

[9] Though note that Secord's rejection of the idea that Geology was an 'infant science' in this period; Secord notes that it was 'in fact a specialized discipline that needed to be learned' (1991: 133)

accommodate research objects that similarly 'flout the boundaries between scientific disciplines' (Daston 2000: 12).

As Church has argued, the discipline of geomorphology 'grew out of natural history' (Church 2013: 184), with some geomorphologists referencing Hutton (1726-1797) as the founder of the yet to be named discipline (Church 1996: 149, Tinkler 1985: 42-57). As Bauer notes, 'The intellectual and academic roots of geomorphology are difficult to disentangle' (1996: 396). According to Gregory and Goudie, the term geomorphology was first used in French in 1866 and in English in 1888 (Gregory and Goudie 2011: 1). Gregory and Goudie note the use of the term at 'the International Geological Congress in 1891' and by the 'US Geological survey' at a similar time (2011: 1). Similarly, in the discipline of geography it 'received wide currency in Mackinder's lecture to the British Association … in 1895' (2011: 1) and 'became more geographically based with the contributions of W. M. Davis (1850-1934)' (2011: 1) who developed the 'geographical cycle', published in 1899; Davis's work 'remained the dominant template for landscape interpretation' 'for more than half a century' (Church 1996: 149). As Bauer explains, therefore, 'the academic disciplines of geography and geology … have had a pronounced influence on what geomorphology has become' (Bauer 1996: 396), while Slaymaker comments that 'The parent disciplines of geomorphology are geography and geology'[10]. However, Bauer perceptively argues that:

> … it is not evident that the intellectual discipline of geomorphology, with its focus on earth-surface phenomena, can or should be traced to any single geological or geographical seed or root, whether a person, institution, event, or activity. … Imposing contemporary disciplinary structures on academic thought during historical periods … ignores the malleable and evolving nature of disciplines - their cores, their boundaries, and the knowledge contained therein are subject to change through time (Bauer 1996: 408).

As such, in this thesis geomorphology is considered as characterised, throughout its history, by 'disciplinary boundaries [that] are neither fixed nor fluid'; instead 'they are relational and in formation' (Barry and Born 2013: 8). The interdisciplinary nature of geomorphology remains today since it may be found as a 'subdiscipline' (Bauer 1996: 396) of both geography and geology. Anecdotal evidence suggests that it is primarily found in geography departments in the UK (see Summerfield 2005b: 403, Sparks 1960:

[10] It should be noted that, according to Smith, in the USA geology was often the parent to geography as well as geomorphology. Smith comments 'As in so many other institutions in the U.S., geography at Harvard emerged from the study of geology 1987: 156).

4)) and geology departments in the USA. This may be due to the closure[11] of many US geography departments (Barry 2016: personal communication, see also Smith 1987 and Murphy 2007), or, as Sparks argued in 1960, because '[geomorphology's] exact affiliation probably reflect[ed] the interest shown by geographers and geologists in landforms in the infancy of geomorphology' (Sparks 1960: 4).

Geomorphology has continued to be highly interdisciplinary in its evolution through the twentieth century and into the twenty-first. Among the interdisciplinary moments described in this thesis it may be noted that Strahler turned to 'physical and engineering sciences and mathematics' (1952: 924) in proposing his 'Dynamics basis of geomorphology'; a particular influence on this work was 'Erosional development of streams and their drainage basins' by the engineer R. E. Horton (1945). Chorley noted that Horton's work, in turn, drew on a wide range of disciplines including 'soil conservation, engineering and physics' (1995: 533). More recently, as discussed in detail in chapter seven, the interdisciplinary field of biogeomorphology 'focuses on interactions between ecological and geomorphic processes' (Viles 2011: 246). Viles, in her role as Professor of Biogeomorphology and Heritage Conservation, at the School of Geography and the Environment, University of Oxford (White 2016: n.p.), also conducts interdisciplinary research applying geomorphological techniques to the challenges of heritage conservation. Murray et al. argue that 'Geomorphology is incorporating ever more interdisciplinary research, recognizing the integral couplings in landscape-forming processes between geology, hydrology, biology (from microbiology to ecology), human dynamics (from engineering to economics to sociology), geochemistry, and biochemistry' (2009: 499); indeed, they argue that 'the study of Earth-surface dynamics is inherently interdisciplinary' (2009: 497).

RESEARCH QUESTIONS

A fresh conception of space, combined with a nuanced and historical account of geoscientific research objects, leads inexorably to a consideration and reformulation of a key problematic tackled by ANT accounts: the displacement of research objects across

[11] See, for example, Smith (1987) for a discussion of the closure of Harvard's geography department in 1948. Smith notes that a year after the closure at Harvard, Yale announced the opening of a geography department; however, this 'only survived two decades' (1987: 170). Similarly, Murphy notes that 'three other leading universities—the University of Pennsylvania, Stanford, and Yale—followed Harvard's lead and dropped their geography departments in 1963, 1964, and 1967, respectively (2007: 124) and later 'The situation deteriorated further in the 1980s when formerly prestigious departments were closed at the University of Michigan (1982), Columbia University (1986), Northwestern University (1986), and the University of Chicago (1987)' (Murphy 2007: 124).

space (Secord 2004: 664). Latour and other proponents of ANT put a lot of emphasis on the work undertaken in order to securely convey objects from one place to another (Withers and Finnegan 2003: 336). Latour comments:

> If you wish to go out of your way and come back heavily equipped so as to force others to go out of their ways, the main problem to solve is that of mobilization. You have to go and to come back with the "things" if your moves are not to be wasted (Latour 1990: 26).

An example, drawn from a few years prior to Cuvier's expedition near Paris, concerns a voyage of exploration by Lapérouse, Captain of *L'Astrolabe,* that visited the Island of Sakhalin in 1787 with the aim of determining whether the land was an island or peninsula (Latour 1987: 215). Latour, in an account critiqued by Bravo (1999), describes the essential work of ensuring that the expeditions findings were safely conveyed back to a European 'centre of calculation' (Latour 1987: 215) in Versailles:

> De Lesseps, a young officer, was asked by Laperouse to carry the maps, the notebooks and the astronomical bearings they had gathered for two years back to Versailles. De Lesseps made the trip on foot and on horseback under the protection of the Russians, carrying with him these precious little notebooks; one entry among thousands in the notebooks indicated that the question of the Sakhalin island was settled and what the probable bearing of the strait was (1987: 216).

In a number of ANT accounts, the term describing objects undergoing this kind of safe displacement is 'immutable … mobiles' (Latour 1987, see also Law and Mol 2001: 611, Law 2002: 93). Law and Mol take the example of a ship traversing an ocean to elaborate on this concept. They argue that the ship has to be a stable configuration – it needs to hold its shape – in order to traverse the ocean. The details of this argument are explored in chapter two; however, for now it is enough to note simply that 'immutable' refers to the stability of a configuration, while 'mobile' refers to the ability of that configuration to undergo displacement within Euclidean space without falling apart. As such, a ship on the oceans (Law and Mol 2001: 611), or a collection of findings transported back to Europe 'on foot and horseback' (Latour 1987: 216), may be described as immutable mobiles.

However, if both spaces and object are reconceptualised new life may be found in an old question. As such, the first research question associated with this study is:

> How do research objects withstand displacement amid spaces of scientific inquiry?

This research question provides an opportunity to: reconceptualise the spaces of scientific inquiry (chapter four); consider the displacement of research objects amid the spaces of scientific inquiry (chapter five); and examine the role played by a range of actors in enabling the displacement of research objects (chapter six). However, these key ANT themes are given new impetus due to a critique of ANT accounts of space.

As seen above, research objects are understood as formed in and by the (inter)disciplinary spaces of scientific inquiry; they are 'real *and* historical' (Daston 2000: 3, emphasis original). However, such spaces may require reworking in order to accommodate interdisciplinary research objects. This leads to the formation of a second research question:

> How are the spaces of geomorphological inquiry reworked to accommodate interdisciplinary research objects?

This second question is investigated with reference to 'biogeomorphology': a sub-, or inter-discipline that, having emerged over the last thirty years, takes ecological as well as geomorphological processes into account (chapter seven). Here the analysis focuses on processes of 'reconfiguration', understood as a process 'which makes a marginal practice central', and 'articulation', understood as a process 'whereby dispersed or confused practices are brought into clearer focus' (Spinosa et al. 1995: 3).

AN ETHNOGRAPHIC METHODOLOGY

The core themes of this thesis may be summarised as follows: firstly, empirical attention will be paid to the specificity of geomorphology as a geoscience that includes a strong field dimension. Secondly, it will feature a critique of a series of weaknesses in ANT accounts, including Latour's account of the spaces of science as dominated by indoor 'centres of calculation' (1987: 215) and ANT's neglect of the history of research objects and practices.

To investigate these themes a multi-sited ethnographic methodology is employed; this enables engagement with scientists working in different locations. The methodology features two case studies and additional exploratory research. This ethnographic approach produces qualitative description capable of communicating the rich, and otherwise obscure, spaces of scientific inquiry. It is well-suited for attending to configurations composed of humans and non-humans (Whatmore 2003: 93). Finally, it

enables attention to the 'past … in the present' (Bloch 1977) through a historically sensitive analysis that traces the genealogies of techniques encountered in contemporary research settings.

The first case study examines the Rock Breakdown Laboratory at the School of Geography and the Environment, University of Oxford. This case study combines participant observation, in the laboratory and at local field sites, with interviews and text-based research. This case study provides an opportunity to attend to the interdisciplinary field of biogeomorphology. It includes a number of interviews with one of the key proponents of this field, Professor Heather Viles, as well as participant observation and interviews with early career scientists undertaking biogeomorphological research. In addition, the case study enables the study of genealogies of specific techniques, the latter understood as components of research objects.

The second case focuses on fluvial research. As the dominant area of inquiry within geomorphology it is important to examine this area of research. Emerging in the course of the research project it focuses primarily on interviews with three geomorphologists: an early career, mid-career and senior geomorphologist. Drawing on Cook et al.'s discussion of 'unsited' fieldwork (2009: 65), this second case study is partially-sited: each of the three participants have conducted formative research at the same research institution though they are now each working at different UK universities. Two have undertaken research at the same field-site and two have worked under the guidance of the same senior geomorphologist during formative stages of their career. In this way the case study has the profile of an 'exploded diagram'. Once again this case study provides an opportunity to investigate the interdisciplinary field of biogeomorphology; forming the focus for chapter seven of the thesis the theme of biogeomorphology serves to link the two case studies. In addition, this case study contributes to the investigation of field-based geomorphology; this leads to the formulation of an alternative account of the spaces of scientific inquiry.

Additional exploratory research was undertaken, in particular, at the British Society for Geomorphology (BSG) where two annual conferences were attended, along with two annual general meetings, two early career workshops and a BSG seminar on river restoration. This work was of importance in developing an appreciation of the institutional history of geomorphology as a discipline; in this way it complemented text-based research tracing genealogies of techniques and biographies of individual scientists. In addition, this

research informed on-going participant observation and interviews and provided an opportunity to recruit interview participants for the second case study.

RESEARCH OUTLINE

Chapter two engages with four key literatures: ANT conceptions of space; Science and Technology Studies (STS) conceptions of research objects; recent work on interdisciplinarity; and, finally, the geographies of science and histories of geography literatures. These literatures relate to the key theoretical themes of space, research objects and interdisciplinarity and to the empirical focus of the research project as an ethnographic engagement with the geosciences.

Chapter three sets out the ethnographic methodology adopted in this thesis. It begins with a consideration of the choice of ethnography. This is followed by a review of key influences on the research design. There is a discussion of multi-sited ethnography (Marcus 1998) and Cook et al.'s proposal of un-sited fieldwork (2009). The emergence of two case studies is then described. A fifth section examines the qualitative methodologies of participant observation, interviews, and analysis of texts. Particular attention is paid to the approach adopted in conducting and analysing interview material. A section on research ethics deals with issues of consent and anonymity. This is followed by a final section on positionality; this describes the experience of conducting this research explaining how the project is shaped by the situation of the researcher.

Chapter four, the first of four empirical chapters, considers space and 'implacement' (Casey 1998: 302) in geomorphological fieldwork. It begins by challenging the dichotomy, inherent in Latour's Circulating Reference (1999a), between embodied exploration in the three dimensions of the field site and an analytical gaze over the two dimensions of maps and other data forms; that is to say between world and word (or image). It employs the Derridean concept of 'supplement' to consider how the spaces of fieldwork are always already augmented by scientific techniques. The chapter continues, in a second section, to examine how the spaces of fieldwork are shaped, and situated, by a range of dimensions: physical, social, disciplinary, comparative, informational and temporal. The chapter concludes with a consideration of Latour's conception of reference; drawing on Derrida it seeks to 'complicate' (Kearney 1984: 124) this account.

Chapter five considers the genealogies of specific techniques and their displacement, or 'cross-appropriation' (Spinosa et al. 1995: 3) amid spaces of scientific inquiry. The chapter begins with an ethnographic vignette describing a field experiment being carried

out by a member of the Rock Breakdown Laboratory, University of Oxford. It examines existing accounts of the mid-century shift to quantifiable process geomorphology and examines what appears, at first, as a simple diffusion of ideas, mediated by human agents, from Columbia, USA to Oxford via the University of Cambridge. However, this analysis is complicated through a consideration of the biographies (Barnes 2001a, Powell 2008a: 549) of individuals and the genealogies of specific techniques. A more complex picture emerges: scholars do not fit into 'convenient camps' (Goudie 2011: 31) and techniques have intricate and 'multiple … lines of descent' (Sherratt 2006: 136).

The chapter provides a substantial theoretical and empirical contribution to the literature. Theoretically it provides a novel extension, and elaboration, of a traditional ANT theme: the displacement of research objects in space. However, both 'space' and 'research objects' are reconceptualised. Research objects are understood as relationally constituted by their contexts (Latour 2000: 260); as such, research techniques, forming part of the context from which research objects emerge, are understood as components of, and proxies for, research objects. In addition, building on the analysis in chapter four, space is reconceptualised to be shaped by disciplinary differentials as well as physical distance. The term 'research entrepreneurs' is introduced to describe the displacement, or 'cross-appropriation', of research objects; the process whereby 'one domain of practices takes over useful practices from another domain' (Spinosa et al. 1995: 3).

Following the emphasis on interdisciplinary displacement of research techniques in chapter five, chapter six examines the role of a range of actors in enabling the safe displacement of techniques amid interdisciplinary spaces. The chapter considers data gathered during interviews and participant observation with members of the Rock Breakdown Laboratory, University of Oxford; these dialogues included researchers employing geomorphological techniques to address problematics in the interdisciplinary areas of conservation heritage and biogeomorphology. The actors considered are people, equipment, texts and software. In this way the chapter once again echoes, and advances, an established ANT theme: the role of a network of actors in enabling the safe displacement of research objects amid the spaces of scientific inquiry. However, here too the terms of traditional ANT analysis are shifted: the distances amid which this displacement occurs are disciplinary rather than physical and research techniques are understood as a component of, and proxy for, research objects. The chapter concludes with a reflection on how this analysis might provide a basis for future research into 'tacit knowledge' held not only in the bodies of scientists, but also within experimental

assemblages; Srnicek's term 'cognitive assemblages' (2013: n.p.) is suggested as a means to conceptualise such a phenomenon.

Chapter seven examines the interdisciplinary area of biogeomorphology: a field of research that investigates interactions between ecological and geomorphological processes, that is found in both rock weathering and fluvial geomorphology. Part one of the chapter opens with two accounts of fluvial biogeomorphology: an early twentieth century quote from the geomorphological literature and an early twenty-first century account from interview data. It notes that, despite the similarities between the two accounts, during the intervening century such accounts are unusual. This fact is attributed, by an interviewee, to the era of quantitative process geomorphology, discussed in chapters five and six, that dominated the discipline in the second half of the twentieth century. The chapter continues, again employing the moniker 'research entrepreneurs', to examine how geomorphologists have reworked the spaces of geomorphological inquiry to accommodate interdisciplinary research objects. Drawing on Spinosa et al.'s account of entrepreneurship, it examines processes of reconfiguration, 'which makes a marginal practice central', and articulation 'whereby dispersed or confused practices are brought into clearer focus' (1995: 3). However, as Barry and Born argue, 'any analysis of the inventiveness of interdisciplinarity must attend to the path dependence of specific interdisciplines, their genealogies and multiplicity' (2013: 42, see also Sherratt 2006: 136). In order to attend to this 'multiplicity' the analysis is broadened; it considers the diverse and overlapping nomenclatures, and associated fields of research, that relate to biogeomorphology; for example, the terminology of 'ecogeomorphology' (Wheaton et al. 2011), 'zoogeomorphology' (Butler 1995, Butler et al. 2012), and 'ecosystem engineering' (Jones et al. 1994, Jones et al. 1997). Star and Griesemer's concept of a boundary object (1989) and Donaldson et al.'s 'ontological' approach (2010: 1526) to 'multiple' (2010: 1526, see also Mol 2002) objects are considered as ways of thinking about these diverse fields of research, themselves understood as constitutive of interdisciplinary research objects.

Part two of chapter seven focuses on a senior geomorphologist's experience of the development of fluvial biogeomorphology in recent decades. It considers a participant's description of a 'wave'-like movement of disciplinary becoming. The analysis continues to consider the highly specific configurations of a research group forming around a particular field site. It is argued that by refusing to differentiate between the social becomings of a research team, and the physical becomings of a landscape, it is possible to attend to the

hybrid movements whereby scientists participate in what De Landa terms ‘the open-ended becoming of the world’ (De Landa 1999: 29, see also Grosz 1999: 13)

Finally, chapter eight concludes the thesis; it summarises the principal findings of this research project and sets out avenues for further research.

Having introduced the themes investigated in this thesis, the next chapter engages with three key literatures, each of which expands upon a theme developed in this introduction: space, research objects, and interdisciplinarity. In addition it considers the sparse STS literature on the geosciences and the fleeting attention given to geomorphology in such accounts.

CHAPTER 2: SPACE, RESEARCH OBJECTS, INTERDISCIPLINARITY AND GEOGRAPHIES OF SCIENCE – A REVIEW OF THE LITERATURE

INTRODUCTION

Through the discussion in chapter one, three themes emerged. This chapter deepens this initial engagement with these themes through a consideration of the relevant literatures.

Firstly, chapter one saw an initial critique of binary oppositions between the supposed particularity of the field and the universality of the academy (museum or laboratory) and, relatedly, the text. It was noted that the conception of space underpinning this binary retained the form of an Euclidean account. This chapter continues this discussion with a detailed critique of ANT accounts of space. While acknowledging that ANT set out to critique Euclidean accounts of space (Ek 2012: 45), the research finds that both 'ANT 1.0' and 'ANT 2.0' (Ek 2011: 10) remain shaped by Euclidean conceptions. This provides a basis for the development of an alternative conception of space based on an ethnographic engagement with field-based geomorphology (chapter four). Such an alternative enables this thesis to attend to the rich meshwork of relations that shape spaces of scientific inquiry.

Secondly, chapter one introduced the idea of geomorphological research objects as 'real *and* historical' (Daston 2000: 3, emphasis original). However, it emphasised the lack of empirical research into the histories of research techniques, the latter understood as a component of, and proxy for, research objects. The second section of this chapter returns to this theme in more detail. This provides the tools for a careful conception of research objects; the latter being a key analytical focus for the research. Chapters five and six of the thesis draw on these discussions of space and research objects to address the first research question guiding this research: 'How do research objects withstand displacement amid the spaces of scientific inquiry?'. As seen in chapter one, a fresh conception of space and the nuanced understanding of research objects means this thesis brings new life to a traditional ANT concern.

Thirdly, chapter one provided an initial discussion of the interdisciplinarity of geomorphology. This chapter connects this discussion to the literature on interdisciplinarity. This begins with a clarification of the diverse terminology employed in the literature. It continues with a brief consideration of 'mode-2' research (Nowotny et al. 2001 cited in Barry and Born 2013: 9) and follows Barry and Born in rejecting the reduction of all interdisciplinarity to a concern for the relevance of research to society and

economy. Instead, adapting Spinosa et al.'s concept of 'world disclosing' (1995: 3) entrepreneurship, it proposes the term 'research entrepreneurship' to describe a broader concept of interdisciplinarity. The discussion continues to note Barry and Born's call to consider 'disciplinary boundaries' as relationally defined and 'in formation' (Barry and Born 2013: 8). Finally, it is noted that, despite Whatmore's acknowledgement of the importance of the relationship between the 'geo' and the 'bio' to geography (2013: 161, emphasis original), there is a lack of empirical studies tackling this interdisciplinary juncture. This engagement with the literature on interdisciplinarity is of significant importance throughout the thesis; this is particularly the case in chapter seven which addresses the second research question: how are the spaces of geomorphological inquiry reworked to accommodate interdisciplinary research objects?

Having considered the themes of space, research objects, and interdisciplinarity in greater depth, this chapter concludes with a section considering the sparse STS literature on the geosciences. This examines four fleeting appearances of geomorphology in the literature: Latour's account of soil scientists in the Amazon; Whatmore's account of flood scientists in the UK; Powell's discussion of Arctic geoscience and Barnes's account of the 'quantitative revolution' in geography. The case for a more detailed investigation of geomorphology is made and some methodological observations are discussed.

SPACE

INTRODUCTION

Crang and Thrift describe a move in 'current writing on space … away from the Kantian perspective on space – as an absolute category – towards space as process and in process (that is space and time combined in becoming)' (2000:3). This shift is exemplified in ANT; for example, according to Murdoch, John Law 'suggests that we should abandon topographical notions of space – in which the space of absolute and fixed coordinates is necessarily dominant – in favour of topological conceptions' (Murdoch 2006: 86). The need for such a shift can be seen with attention to an example.

Lorimer and Spedding present a discussion of a 1952 geomorphological field trip and family holiday undertaken by two geography graduates, their three-month old son, and the mother's sister. The field trip to the Parallel Roads of Glen Roy was recorded with humour in a logbook and this forms the empirical basis for Lorimer and Spedding's account. As Lorimer and Spedding point out, the geography graduates were not 'professional academics' (p. 33); one of the graduates began his career as 'Field Studies Instructor at

the recently established Glenmore Lodge Outdoor Centre … in the Scottish Highlands' (p. 16) and both worked as school teachers at different stages in their life (p. 16 and p. 30) while maintaining their interests in geography (p. 30). The fieldtrip belongs, therefore, to the 'hidden histories' (Lorimer and Spedding 2002: 300) of geography that don't always get told (see also Norcup 2015).

From the point of view of this study, the principle interest in Lorimer and Spedding's paper pertains to their account of the spaces of fieldwork. They note that 'it is important to include those aspects of space and place, as understood in contemporary geographical thought, that confound cartography' (2005: 33). As such the 'local is never just local' (p. 31); Lorimer and Spedding comment:

> It is a simple task to pinpoint the 1952 expedition in terms of its absolute spatial and temporal coordinates, *but these are of little help to us in locating the richer historical geography of science that we envisage*. To appreciate this we need to examine the temporal sequence of events as well as the spatial arrangements of people, places, objects, ideas and social relationships associated with these events, in other words the wider network that brings specific instances of 'the local' into existence as the products of other locals coalesce. The field science that Robin and Catriona did at Glen Roy was also, in part, the science of Aberdeen, Sheffield (thanks to David Linton), Glenmore, Kingussie (home of the Badenoch Field Club) and so on. In this sense, the local is a feature that is both at-a-point and strung out across time and space (p. 31, emphasis mine).

As Lorimer and Spedding make clear, a Euclidean account of space is insufficient for explaining the richness associated with the spatial configurations of geomorphological research.

This section begins with a consideration of the distinction between space and place. It continues to examine critically conceptions of space employed in early Actor-Network Theory, referred to here, following Ek, as 'ANT 1.0', and in the more recent theorisations of 'ANT 2.0' (2011: 10).

SPACE, PLACE AND 'SPLACING'

Ek summarises the difference between space and place in 'traditional Western metaphysical thought', with space 'as unbounded three-dimensional distance' and place as 'bounded distance' (2012: 41). In contrast, Barnes argues for a reconceptualization of places not as 'abstract, static, self-contained sites, sealed by rigid boundaries, but [as] material, dynamic, open, and defined by their interrelationships with other places' (Barnes 2004: 591). Doel goes further; he is 'incredulous about the polarization of place and space' and argues that 'if it were not such an inelegant neologism, [he] would be tempted

to say that there is nothing but splace, taking splace – splacing' (1999: 9, emphasis original). Indeed, reconceptualising space as relational arguably renders the opposition between space and place redundant. Places are understood, as seen below, as 'socio-material events' (Ek 2012: 39). Similarly, spaces are no longer understood 'simply as container[s] of heterogeneous processes' but instead as 'made by heterogeneous relations' (Murdoch 2006: 4), with the latter understood as 'mixtures of the natural and social and the human and the non-human' (Murdoch 2006: 3). The eventful, heterogeneous (natural and social), nature of spaces and places indicates that the two terms may be used interchangeably in a post-structuralist account (see for example Ek 2011: 7). Ek argues, further, that 'place and space could … be seen as verbs rather than as nouns' and that 'in sum, there is no space, only spacing, no place, only placing' (2012: 42) – or, in Doel's awkward neologism, 'splacing' (1999: 9).

However, the proverbial baby must not be jettisoned with the bathwater. An important aspect of place, that is of *specific* spaces, is particularity. A key achievement of science studies is to emphasise the role of place, or of specific spaces, in the production, and circulation, of scientific knowledge (Livingstone 2003). Although, particularly in its 'ANT 1.0' guise (Ek 2011: 10), science studies at times relied on a 'narrow imperialistic template' (Barnes 2004: 577), it has successfully described the ability of science to act at a distance and, relatedly, to withstand displacement across space. In Barnes's terms, it has examined how science accomplishes its 'trick of making place disappear', bringing to light the 'enormous effort [required] to undo geographical distance' (Barnes 2014: 210). In challenging the traditional distinction, between place as a bounded site and space as unbounded, three dimensional, and Euclidean, it is important to recognise that it is still possible to talk about *specific* spaces; it is the understanding of space that is to be challenged, *not* the particularity of given spatial configurations.

SPACE IN ACTOR-NETWORK THEORY

ANT provides a way to describe 'the often enormous work needed to produce knowledge, as well as to capture its fragility and contingency' (Barnes 2014: 217). It also 'emphasiz[es] distributed agency spread across many actors, human and non-human' and 'encourag[es] the inclusion … of material actants – books, machines and devices' within its narratives, 'recognizing their agency' (p. 217). According to Powell, ANT is 'the most successful posthumanist approach' (2007a: 317). Similarly, Barnes comments that ANT is 'the best-known variant [of science studies and] certainly the version most commonly found in geography' (Barnes 2014: 207).

SPACE IN TRADITIONAL ANT: REGIONAL AND NETWORK SPACE

In a valuable discussion of ANT's accounts of space, Ek explains that early ANT incorporated two 'topologies', or accounts of space: regional space and network space (2012: 45). The topology of 'region' is closest to traditional, Euclidean accounts of space; indeed, Ek comments that: 'the spatiality of this topology is the Euclidean conception of absolute and relative space' (2012: 45). The topology of network space is, conversely, 'not measured in physical distance' but 'in how the elements of the network hang together'.

These accounts of space as region and network are not mutually exclusive: John Law and Annemarie Mol provide a tangible example of how spaces of region and network inter-relate in their description of a ship crossing an ocean (2001, see also Law 1986, Law 2002): 'The mobility of the Portuguese ships only exists in Euclidean space. Here they move through an orthogonal box defined by X-Y-Z co-ordinates – a box in which there is a long distance between Lisbon and Calicut' (2001: 612). Conversely 'in network space there is nothing mobile about the vessel. It holds its shape. *But it also holds its position* in that space. It does not displace itself [in network space]' (2001: 612, emphasis original). Law and Mol explain that 'the immutable mobile', in this case the ship, 'achieves its character by virtue of participation in two spaces: it participates in both network and Euclidean space. And such is Latour's trick. To talk of an "immutable mobile" is to elide the two' (2001: 612). Elsewhere, Law explains that:

> Vessels are spatially or topologically multiple, inhabiting both Euclidean and network spaces. They are also homeomorphic within each of the forms of space, holding together physically in the one, and functionally or syntactically in the other. However, they move only within Euclidean space, remaining immobile within network space. (If there is rupture in the relations between the components in network space then they are no longer a network object.). At the same time it is this immobility within network space which affords their displacement within Euclidean space, that allows them to sail successfully from Calicut to Lisbon (2002: 95-96).

Despite this shift, 'old ANT' (Law 2002: 96) remains significantly concerned with how objects, whilst held stable in network space, withstand displacement across physical, or Euclidean, space; such objects are termed 'immutable mobiles': they are stable in network space and mutable in regional, or Euclidean, space (Law 2002: 96). As such, despite Ek's contention that ANT's conception of space 'started out as a critique of

Euclidean space' (2012: 45), the reality of Euclidean space remains unchallenged in the accounts of 'old ANT'; network space does not replace Euclidean space – it co-exists with it. For example, the Latourian description of 'Pasteur's lab in Paris' as a '"centre of calculation" indicates 'a discrete space able to act on many other dispersed spaces' and 'able to act at a distance as long as the network … remain[s] in place' (Murdoch 2006: 64-65). The discrete-ness of the space is demarcated within Euclidean space as is the 'distance' across which the 'centre of calculation' is able to act. Similarly, listed among the 'features of centres of calculation' is their ability to '"bring home" relevant features of the places and peoples of concern' and to 'reach back out to the multitudes of micro-locales upon which it might act' (Murdoch 2006: 65, see also Latour 1987: 218). In the case of Pasteurisation, the 'anthrax bacillus' is 'refined in the field' and then 'transported back to Paris' (Murdoch 2006: 65). Again, 'home', 'reach' and 'micro-locales' are Euclidean designations, or in the terms of ANT they are located in the topology of 'region'; the field and Paris are understood as located in, and separated by, Euclidean space. A further example of Euclidean space at work in an ANT analysis has already been seen in Law's description of a ship traversing an ocean (Law 2002: 95-96). Yet another example may be discerned in Latour's account of *Circulating Reference* where samples are gathered in the field and transformed, in a series of steps, from field to laboratory; a displacement that includes a significantly Euclidean dimension (Latour 1999a).

Proponents of 'ANT 2.0' (Ek 2011: 10, Ek 2012: 44) identified another criticism of earlier ANT accounts: they sought to improve on what they saw as the 'inappropriately rigid and centred version of relations' (Law and Singleton 2005: 341). Indeed, Law and Singleton argue that:

> ANT became too managerialist in its early versions as it thought about objects. Its intuition about the importance of relations was right, but it got itself too concerned with standardization, with the rigidities of immutable mobiles that, if they exist at all, exist within rather specific and rigid networks that try to reach out over long distances and achieve centralised control (2005: 339).

Responding to these criticisms, 'ANT 2.0' scholars, such as John Law and Annemarie Mol, sought to 'open up to a spatial imaginary more ontologically complex than "ANT 1.0"' (Ek 2011:10).

A third criticism of early ANT is related obliquely to the second: the hefty logic of ANT 1.0 does not only result in overly 'managerialist' accounts of scientific spaces and practices; it's highly theoretical analysis runs the risk of 'overdetermin[ing] [ethnography] before it begins' (Marcus 1998: 17). As Marcus argues:

> Ethnographic projects that are heavily motivated by and cast in culture theory terms must be allowed to 'breathe', especially in terms of their descriptive accounts of things, before the theory kicks in (1998: 18).

ANT provides an important attempt at challenging the dominance of Euclidean accounts of space – though, as argued above, fails to go far enough in this direction. A new generation of ANT analyses, sometimes termed 'ANT 2.0' (Ek 2012: 44), sought to respond to some of these issues. The next section critically examines the concept of space proposed by 'ANT 2.0'.

SPACE IN 'ANT 2.0': TOPOLOGY, FLUID AND FIRE SPACE

According to Ek, 'John Law and Annemarie Mol', the leading proponents of ANT 2.0, have 'investigated several topological constellations beyond the Cartesian logic of Euclidean space' (2012: 44). This section sets out the spatial ideas associated with this shift beyond traditional ANT before examining the extent to which they overcome the criticisms of early ANT discussed above.

In order to examine Law and Mol's responses to the deficiencies they perceived in earlier versions of ANT it is necessary, first, to examine the term 'topology' more carefully. Law comments that:

> My question is: what is an object if we start to think seriously about alterity? I choose to tackle this topic spatially, and more particularly topologically (Law 2002: 92).

'Topology' may simply be understood as an account of spaces; it is sometimes counter-posed with 'topography', a term closely connected to traditional, Euclidean, accounts of space (Ek 2011: 3, Murdoch 2006: 12). The roots of the term 'topology' are found in mathematics where it denotes 'the character of objects in space' (Law 2002: 94, Ek 2012: 45). Law explains that:

> … topologists think about spatiality by asking questions about the continuity of shapes: the properties that the latter retain while they are also being deformed … [they] invent and explore different possible spaces or (this amounts to the same thing) different possible circumstances in which objects may be deformed without being broken (2002: 94).

As seen above, diverse topologies, or 'spatialities' (2002: 94), may coexist. Here in its adoption from mathematics there is a subtle sleight of hand: topologists describe *singular spaces* with *multiple dimensions*, whereas Law states that 'spaces are not self-evident and singular, but … there are *multiple forms of spatiality'* (Law 2002: 92, emphasis original). In mathematics, an object is topologically homeomorphic if it can be deformed without being broken. However, this topological homeomorphism is dependent on how many dimensions there are in the space the object occupies. An object that does not retain homeomorphism when deformed in a space of only two dimensions may retain homeomorphism when undergoing the same deformation in a space of three dimensions (see Law 2002: 94). In the ANT account, or analogy, of topology, if an object is considered only in terms of network *space* it is deemed 'immutable'. However, if it is considered in *a multiple space composed of the spaces of both network and region*, it becomes an immutable mobile; it is stable in network space but may move in the space of the region, that is in Euclidean space. The sleight of hand consists of a shift from talking about objects that are homeomorphic in spaces of a given number of *dimensions* to discussing objects that remain homeomorphic in spaces with a given number of *types of space* (regional, network, fluid, fire).

FLUID SPACE

Extending this reasoning along lines that are 'highly abstract' and 'almost cryptic' (Ek 2012: 48), but that seek to respond to critiques of 'ANT 1.0' (Ek 2011: 10), proponents of 'ANT 2.0' (Ek 2011: 10) argue that an object may be mobile in both Euclidean (or regional) and network space if it is stable in a third space, or 'topological system': 'fluid space' (Law and Mol 2001: 614). This is analogous to the topologist's argument, explained above, that an object that cannot retain homeomorphism when deformed in two dimensions may retain homeomorphism under the same deformation within a space that has three dimensions. This complex reasoning is made clearer with an example. The proponents of 'ANT 2.0' (Ek 2011: 10) explain with reference to a Zimbabwean bush pump. This is a pump whose inventor, having decided not to patent their design, encouraged users to adapt it, or to carry out non-standard repairs, in order to enable the applicability of the pump in a wide range of situations and with a wide range of materials (Law 2002: 100). Law and Mol explain that:

> Within Euclidean and network space alike, the bush pump is an object that changes shape. It looks different from one village to the next, and it works differently from one set-up to the next … the bush pump shows configurational variance. It is a mutable mobile … Is it the same in two places? A network analyst would say no. And yet it makes sense to say that it is 'the same pump'. It is the 'Zimbabwean bush pump' that moves to so many places in rural Zimbabwe and that moves (so runs the argument) precisely because it is not an invariant shape either in network or in Euclidean space. It changes, it is different. (2001: 613)

Despite this difference, and the variance of the object in both network and regional, or Euclidean, spaces, Law and Mol emphasise that it is 'the same pump' (2001: 613). For this to be the case it must be invariant in one spatiality but, as already seen, it is variant in both network space (it 'shows configurational variance' (2001: 613)) and regional, or Euclidean, space (it 'moves to so many places in rural Zimbabwe' (2001: 613)). Where then is its invariance located? Law and Mol propose that the bush pump is invariant in 'fluid spatiality' (2001: 614). They comment:

> So what defines shape invariance in a fluid topology? No doubt there are various ways of thinking about this. However, one particular feature is crucial. This is that while the connections which make a shape invariant in fluid space change shape, they do so *gradually* and incrementally' (2001: 614)

The bush pump is therefore understood as invariant in fluid space: it changes shape (in Euclidean and network space) but does so 'gradually and incrementally' (2001: 614) - that is in a 'fluid' way. It could be argued therefore that the bush pump is an 'invariant mutable mobile': it is invariant in fluid space, mutable in network space and mobile in Euclidean space.

FIRE SPACE

> The fire topology … harmonizes with [a] post-structuralist critique of the metaphysics of presence (Ek 2011: 12)

Mol and Law argue, however, that some objects move but not 'gradually' or 'incrementally' (2001: 614) way but in 'abrupt and discontinuous movements' with a '*flickering relation between presence and absence*' (2001: 615, emphasis original). They give the example of a mathematical formula for the 'gust response' or 'G' of an aircraft – that is its susceptibility to turbulence. The formula includes a number of variables such as velocity, lift slope, wing loading, weight and size of wing. Both these independent variables and the dependent variables (G, or gust response) pull on things that cannot be actually present on the page, and yet are present in their mathematical form. Law and Mol argue that:

> All the terms of the expression achieve their stability by virtue of the simultaneous absence and presence of a range of other materials, situations. They achieve their stability in the continued enactment of discontinuities (which are also continuities) with those Other materials and contexts. And what is the case for components of the expression also applies to the expression as a whole. It is held in shape and given constancy as a result of the discontinuities of conjoined alterity (2001: 618).

Such an object is deemed mobile in Euclidean, network and fluid space but holds its shape in a fourth spatiality, or topology: 'fire space' (2001: 615). Here, 'as with fluid constancy, movement rather than stasis is crucial. Without movement there is no consistency. The difference is that while in fluidity constancy depends on gradual change, in a topology of fire constancy is produced in abrupt and discontinuous movements' (2001: 615). Mol and Law continue: 'topologically, then, our argument is that in fire space a shape achieves constancy in a relation between presence and absence: *the constancy of object presence depends on simultaneous absence or alterity* (2001: 616, emphasis original, see also Law and Singleton 2005: 342-343).

This complex argument becomes slightly more intuitive in an example given by Law and Singleton (2005): alcoholic liver disease. The disease is difficult to pin down; Law and Singleton write that:

> The textbook … by Sherlock (1989), which includes a chapter on alcohol and the liver, is cautious in almost every respect. Indeed, it offers no definition of 'alcoholic liver disease'. Instead there are numerous questions, and answers to those questions, and all are surrounded by caveats … And even the 'hard science' part of the description, about the oxidation of ethanol in the hepatocyte, is full of ifs and buts (2005: 339-340).

They continue:

> What of the name-changing that we talked about earlier? What of the slippage between 'alcoholic liver disease', the formal topic of our enquiry, and the other related topics that we found ourselves talking about? … we were dealing with, and learning about, a single-albeit shape-changing-object (2005: 340).

This leads to the conclusion that alcoholic liver disease is a fire object; specifically, Law and Singleton adopt the metaphor of the 'bush fire' rather than a 'domestic fire' (2005: 347):

> In this way of thinking, alcoholic liver disease becomes an object that jumps, creatively, destructively and more or less unpredictably, from location to location. It is an object in the form of a dancing and dangerous pattern of discontinuous displacements between locations that are other to (but linked with) each other … it is also, or so we believe, much more dynamic, more sporadic, less predictable, and, yes, more *discontinuous* than is suggested by the metaphor of flow. This is why, for us, it is a fire object: it lives in and through the juxtaposition of uncontrollable and generative otherness (2005: 347, emphasis original).

According to Mol and Law the idea of topology 'extends the possibilities of mathematics far beyond its original Euclidean restrictions by articulating other spaces' (Mol and Law 1994: 643, original emphasis). To what extent has ANT 2.0 succeeded in carrying this impetus across to their non-mathematical account of space?

Ek makes reference to the 'highly abstract, almost cryptic reasoning' (2012: 48) associated with a fire topology in what he terms 'ANT 2.0' or 'ANT and after' (2012: 44). Indeed, the elaborate and protracted logics associated with descriptions of multiple, interacting spatialities of ANT 2.0 render such accounts clumsy and lacking the elegance presumably associated with their source of inspiration in mathematical accounts of topology. As Powell, citing Berlin, points out, ANT is in danger of becoming 'a theory festooned with *ad hoc* hypotheses to account for each specific deviation from the norm' and will as such 'soon cease to be useful' (Berlin 1960: 14 cited in Powell 2007a: 318).

ANT 2.0 does succeed in its objective to open up space for alterity in its accounts; in the place of 'managerialist' (Law and Singleton 2005: 339) accounts, of earlier ANT, its descriptions of 'fluid' and 'fire' space allow a greater diversity. 'Fluid' space renders it possible to think about objects that are mobile, or variant, in both network and regional space, but stable in 'fluid' space; incremental differences are acknowledged. Meanwhile, 'fire' space allows a way to think of objects that have abrupt, or 'flickering' (Mol and Law 2001: 615) differences.

However, this increased acceptance of alterity does not exhaust the aims of the proponents of ANT 2.0; Law comments that his objective is 'to denaturalise network-space and network-objects by showing that these too are enacted' (2002: 102). The term 'denaturalise' is important; it relates closely to Marcus's call to allow ethnographic accounts to 'breathe' (Marcus 1998: 18). One of the objectives of both ANT and ANT 2.0 is to strip back the philosophical baggage that invisibly frames analysis. These efforts are highly valuable. However, there is a danger that, no sooner than the dominance of a Euclidean account has been challenged, a new hefty theoretical apparatus is hauled into view; once again this runs the risk of 'overdetermin[ing]' (Marcus 1998: 17) empirical materials gathered during an ethnographic study.

Finally, and most importantly, to what extent does ANT 2.0 succeed in 'investigating … spatialities … beyond the Cartesian logic of Euclidean space' (Ek 2012: 44)? Like ANT 1.0, the proponents of ANT 2.0 do not undo a Euclidean account of space; they merely add to it. The diverse proposed spatial topologies of 'fluid' and 'fire' *as well as '*region' (Euclidean) and 'network' do allow a fresh way to think about scientific spaces and

objects; however, this does not succeed in shaking off the vestiges of a Euclidean ontology. An alternative conception of space is necessary to enable the apprehension of the complex entanglements that shape spaces of scientific inquiry.

OBJECTS

There is a strong relationship between space and research objects. In traditional ANT there is a preoccupation with the question of how research objects withstand displacement across (Euclidean) space. In later ANT accounts, objects are understood as intersections between the different spatialities of network, region, fluid and fire (Law 2002: 102, Ek 2011: 12). This section considers the literature on research objects; it begins with Daston's designation of research objects as 'real *and* historical' (Daston 2000: 3, emphasis original), before considering objects that are dispersed; Star and Griesemer's concept of boundary objects (1989); and Mol's concept of objects as multiple (2002, see also Donaldson et al. 2010).

REAL AND HISTORICAL RESEARCH OBJECTS

As seen in chapter one, Daston makes the vital claim that 'scientific research objects can be simultaneously real *and* historical[12]' (Daston 2000: 3, emphasis original). Her focus is on 'how a heretofore unknown, ignored, or dispersed set of phenomena is transformed into a scientific object that can be observed and manipulated, that is capable of theoretical ramifications and empirical surprises, and that coheres, at least for a time, as an ontological entity (Daston 2000: 5). In the same volume, Latour ponders 'why should adding the strange assumption of the historicity of things to the historicity of humans simplify the narratives of both?' (Latour 2000: 260). His response is that 'we do not have to consider physical entities such as ferments, germs, or eggs sprouting into existence as being radically different from a *context* made of colleagues, emperors, money, instruments, body practices'; as such we are required 'neither to stabilize the list of what makes up nature nor the list of what makes up context' (2000: 260).

Building on this analysis, only a small step needs to be taken to propose that a research object is *constituted by* its context. This accords loosely with an ANT answer to the

[12] It is important to emphasize that a consideration of scientific research objects as real and historical does not suggest that the modes of scientific inquiry investigated are 'wrong, or [are] without utility, or [have] no bearing on the material world, or especially that anything goes' (Barnes 2001a: 413)

question ‘what is an object?’ (Law 2002: 91): ‘*objects are an effect of stable arrays of network relations …* [they] hold together so long as those relations also hold together and don’t change their shape’ (2002: 91, emphasis original); here, however, this is rephrased to say that objects are understood, relationally, as an effect of heterogeneous configurations and hold together so long as those configurations remain intact. This is supported by the ‘material semiotics’ of Law and Hetherington (2000: 3, cited in Barnes 2004: 576); this is the crucial idea that ‘entities take their form and acquire their attributes as a result of their relations with other entities’ (Law 1999: 3 cited in Barnes 2004: 576). Though, as seen below, research objects are not always as tangible as ‘entities’, this approach to considering ‘entities’ is closely connected to the ANT 2.0 (Ek 2012: 44) conception of objects and spaces. Ek comments that: ‘Objects can thus be ontologically understood as intersections between spatialities that in their turn enact different social topologies as Euclidean, network and fluid topologies’ (2011: 12). Similarly, Law states that:

> Objects are topologically multiple, existing as intersections or interferences between different spaces including regions, networks, and fluids. As a part of this I have argued that objects may be understood as an intersection between different versions of shape invariance, Euclidean, network, and fluid (Law 2002: 102)

However, as seen above, while ‘ANT 2.0’ (2011: 10) provides an interesting response to the perceived weaknesses in the spatial ontology associated with ‘ANT 1.0’ this thesis adopts neither of these two ANT alternatives and seeks to develop an alternative account of the spaces of geomorphological inquiry. Nonetheless, the accounts of research objects offered by Law and proponents of ‘ANT 2.0’ provide a valuable point of departure. The analysis of this thesis seeks to adopt the assumption that research objects are constituted by their context, indeed that they may be understood in some sense as constituted by spatial intersections, without assuming all the baggage of ANT (Latour 1999b).

DISPERSED OBJECTS

Returning to the analysis of Daston cited above, it is interesting to note her emphasis on ‘dispersed … phenomena’ in her attention to the way ‘a heretofore unknown, ignored, or dispersed set of phenomena is transformed into a scientific object that can be observed and manipulated’ (2000: 5). This sparks two avenues of inquiry relevant to this study. Firstly, as noted in chapters five and seven, Spinosa et al. describe three processes associated with the ‘disclosing of new worlds’ through what they term ‘entrepreneurship’ (examined in this thesis as ‘research entrepreneurship’): reconfiguration, cross-appropriation and articulation (1995: 3). Of these three processes, articulation is described as a process whereby ‘dispersed or confused practices are brought into clearer

focus' (1995: 3). Within this study, this provides one avenue for exploring the question of how the spaces of geomorphological inquiry are reworked to accommodate interdisciplinary research objects. Secondly, attention to the way 'dispersed' phenomena are 'transformed into a scientific research object' is particularly salient to the research objects of geomorphology, particularly following the quantitative revolution and the inculcation of 'process geomorphology' since the middle of the twentieth century. Notwithstanding the large number of small and large blocks of rock encountered in the laboratory and field sites investigated in chapters five and six of this study, many of the objects of geomorphology are 'not the kind of object[s] you can drop on your toe' (Law and Singleton 2005: 331). Instead, like the 'alcoholic liver disease' encountered in Law and Singleton's study (2005: 331), the research objects of geomorphology are often dispersed or, more accurately, diffuse. Such objects are rendered visible only within an assemblage of equipment and (disciplinary, or interdisciplinary) research practices that have evolved over time. Such assemblages, or configurations, form a key focus for this research project.

BOUNDARY OBJECTS AND MULTIPLE OBJECTS

Star and Griesemer argue that 'it is normally the case that the objects of scientific inquiry inhabit multiple *social* worlds, since all science requires intersectional work' (1989: 392, emphasis mine). Star and Griesemer accept the Latourian view that 'the important questions concern the *flow* of objects and concepts through the *network* of participating allies and social worlds' (p. 389, emphasis original). However, they reject the 'Callon-Latour-Law model of translations and *interessement*' (p. 390) since they see this as '*necessarily*' describing what happens, as objects and concepts are displaced, 'from the point of view of one passage point – usually the manager, entrepreneur, or scientist' (p. 390, emphasis original). Star and Griesemer argue that this has the effect of 'funnelling' (p. 390) the analysis; they explain that 'the *n-way* nature of the *interessement* (or let us say, the challenge intersecting social worlds pose to the coherence of translations) cannot be understood from a single viewpoint' (p. 389). Instead they argue for, and adopt, an approach characterised by 'many-to-many mapping, where several obligatory passage points are negotiated with several kinds of allies' (p. 390).

Star and Griesemer introduce the concept of a '*boundary* object' (p. 393, emphasis original) in order to think about how objects function amid this 'many-to-many mapping'; they define boundary objects as being 'both plastic enough to adapt to local needs and the constraints of several parties employing them, yet robust enough to maintain a

common identity across sites' (p. 393). Star and Griesemer explain that 'these objects may be abstract or concrete' and that 'they have different meanings in different social worlds but their structure is common enough to more than one world to make them recognizable' (p. 393). Their conception of boundary objects resembles the Simmelian concept of 'form and content' (Rapport and Overing 2000: 136): shared (cultural) forms enable coherence and communication, yet diverse 'content', or meanings, enable difference – thereby providing a way to negotiate the theoretical dilemma of (social) structure versus (individual) agency (see Barnes 2001a: 415).

Drawing on Star and Griesemer's work, Donaldson et al. consider the idea that 'many interdisciplinary objects can be [considered as] boundary objects' (2010: 1532). However, they point out that 'a boundary object, by definition, is essentially singular – it retains its form across different fields' (p. 1532). The difficulty that Donaldson et al. identify with this proposition is that it allows only an 'epistemological', as opposed to an 'ontological' consideration of interdisciplinary research objects. The 'epistemological' approach assumes that there is a single object, with multiple perspectives, or understandings, of the research object associated with different disciplinary points of view. In contrast, the 'ontological' approach assumes multiple overlapping research objects, since the research objects cannot be entirely separated from the concrete techniques and practices of the diverse disciplines that engage with them (see Mol 2002). Donaldson et al. argue that 'sometimes the best way to tackle the mess of an interdisciplinary object is to acknowledge its multiplicity and not to try to reduce that through synthesis' (2010: 1533-1534).

SUMMARY

This section has considered research objects as (i) real and historical (Daston 2000: 3), as (ii) boundary objects and as (iii) multiple objects. An important corollary emerges from the assumption that research objects, *constituted by* their contexts, may be understood relationally - that is as an effect of real and historical, heterogeneous configurations. Such configurations include the historically specific assemblages of techniques and equipment that render diffuse objects tangible. It is therefore possible to consider research techniques as a component of research objects. This leads to the formulation of the first research question: 'How do geomorphological research objects withstand displacement amid the spaces of geomorphological inquiry? As seen above, the ability of research objects to withstand displacement across space is a traditional concern of ANT.

INTERDISCIPLINARITY

A third literature of relevance to this study turns on the question of interdisciplinarity. The boundaries and genealogies of geomorphological practices are not assumed in advance; instead, 'disciplinary boundaries are [understood as] neither entirely fixed nor fluid; … [but as] relational and in formation' (Barry and Born 2013: 8). As seen in chapter one, geomorphology is a highly interdisciplinary field of research. This thesis investigates the cross-appropriation of research techniques and examines how the spaces of scientific inquiry are reworked to accommodate interdisciplinary research objects. As such it makes a substantial empirical and theoretical contribution to the literature on interdisciplinarity. This section examines recent literature on this theme; it begins by considering the proliferation of terminology associated with this research area.

CLARIFYING TERMINOLOGY

A number of interrelated prefixes to the term 'disciplinary' have developed in the literature; for example, Osborne lists 'inter-, multi-, trans-, de-, anti-, in-, meta- and post-' (Osborne 2015: 2). Barry and Born explain that 'Commonly, a distinction is made between multidisciplinarity, in which several disciplines cooperate but continue to work with standard disciplinary framings, and interdisciplinarity, in which there is an attempt to synthesise perspectives from several disciplines' (Barry and Born 2013: 8). Meanwhile, they note 'Less clear distinctions are made between interdisciplinarity and transdisciplinarity, and in practice their meanings are often conflated' (p. 8). The term 'interdisciplinarity' is 'dominant' in the 'Anglo-American academy' while 'the idea of transdisciplinarity … has wider currency in the French and German speaking worlds' (p. 8). Barry and Born provide a useful summary of the potential differences between interdisciplinarity and transdisciplinarity; however, notwithstanding the critique of Osborne (2015: 4), this thesis follows them in their conclusion 'neither to define nor to arbitrate between the two terms', but to 'take "interdisciplinar-ity" to be a generic expression whilst recognising that interdisciplinarity and transdisciplinarity are indigenous concepts with variable significance in particular circumstances' (Barry and Born 2013: 9-10).

AMONG DISCIPLINES

Another important prefix to the term 'disciplines' is proposed by Donaldson et al., who suggest that 'thinking of interdisciplinarity as being "among disciplines" rather than between them' (2010: 1527), 'draws attention away from interdisciplinary research as something that occurs at the interface of disciplines, and directs it towards a wider terrain

of exploration' (2010: 1522). They also argue that this pushes attention towards the objects of research rather than the 'interface between disciplines'. This important insight is valuable in considering an alternative conception of the spaces of geomorphological inquiry, and in considering how research objects withstand displacement *among*, or amid, these spaces.

ENGAGING WITH (INTER-) DISCIPLINARY FORMATIONS

Klein describes the development of an 'undeniable momentum' around interdisciplinarity in the second half of the twentieth century (1990: 38). She sees this 'momentum' as having roots in the expansion of the 'hyphenated sciences' in the fifties (p. 38), following what she identifies as a catalytic effect of the Second World War (p. 32). This was followed by what she terms a 'watershed era' in the sixties and seventies (p.35), with an Organisation for Economic Co-operation and Development (OECD) seminar in 1970, the results of which were published in 1972, followed by a second major publication in 1979, the development of two professional associations and a further international conference in 1980 (p. 37).

Klein's attribution of 'historical novelty' to an analysis of interdisciplinarity is seen as problematic by Barry and Born who argue it is false to imagine 'that in the past knowledge production has primarily taken place within autonomous and unified disciplines, and that it no longer does' (2013: 3). Instead, 'if the appearance of interdisciplinarity is a historical constant', these authors argue that 'what is novel is the contemporary sense that greater interdisciplinarity is a necessary response to intensifying demands that research should become more integrated than before with society and the economy' (2013: 4).

It is this integration with society and economy that provides the central concern of Nowotny et al. who argue that there has been a shift towards what they term 'mode-2' research (Nowotny et al. 2001 cited in Barry and Born 2013: 9): studies orientated by relevance to society and economy. According to Barry and Born such research is animated by two logics: a 'logic of accountability' to society and a 'logic of innovation' through which academic work engages with the economy (2013: 14). However, Barry and Born are critical of arguments that reduce interdisciplinarity to these twin imperatives; they comment: 'interdisciplinarity is equally associated with the development of fields, initiatives and sites in which new types of autonomy are created and defended against a reduction of research to the imperatives of accountability or innovation' (2013: 4). This insight provides impetus for this research to engage with interdisciplinary work in geomorphology and related fields without assuming that it is carried out solely with an aim

to engage society and economy. This deepened concept of interdisciplinary may be examined with reference to Spinosa et al.'s concept of 'entrepreneurship' as having a 'world disclosing' potential. As seen briefly above, according to Spinosa et al., this is characterised through 'cross-appropriation', 'in which one domain of practices takes over useful practices from another domain' (Spinosa et al. 1995: 3); 'reconfiguration', where marginal practices become central; and 'articulation', 'whereby dispersed or confused practices are brought into clearer focus' (1995: 3). Building on the ideas of Barry and Born (2013) and Spinosa et al. (1995), and following a consideration of the spaces of geomorphological inquiry in chapter four, chapters five and seven of this thesis will seek to understand the ways in which geomorphologists act as 'research entrepreneurs', with the latter understood in terms of its world-disclosing potential (Spinosa et al. 1995: 3).

Barry and Born also argue that both interdisciplinary fields of research, and established disciplines, may be sites of autonomy and inventiveness; however, neither have any inherent claim to these traits (2013: 42-43). Similarly, they argue that 'interdisciplinary research can involve closure … on the other hand disciplines themselves are often remarkably heterogeneous or even internally divided' (Barry and Born 2013: 7). Attention is therefore directed towards 'the path dependence of specific interdisciplines, their genealogies and multiplicity' (p. 42). As Tamboukou and Ball have argued (2003), ethnography provides a well-suited methodology for investigations sensitive to such nuance. Rather than treating geomorphology, a priori, as a discipline, sub-discipline, or inter-discipline, since it manifests in all these disciplinary formations at different, specific times and places, it is important to approach geomorphology without prior assumptions about its distinctive disciplinary formations. In doing so, this study adheres closely to Barry and Born's call to 'understand interdisciplinarity less as a unity and more as a field of differences, a multiplicity' (2013: 5); indeed, it extends this approach to the consideration of all disciplinary formations, interdisciplinary or otherwise, encountered in the course of the research. As such it concurs with Barry and Born's observation, noted above, that 'disciplinary boundaries are neither entirely fixed nor fluid; they are relational and in formation' (2013: 8).

STUDIES EXAMINING INTERDISCIPLINARY RESEARCH AT THE JUNCTURE OF SOCIAL AND NATURAL SCIENCES

The reordering of 'relations between the natural sciences, the social sciences and the arts and humanities' is 'at the heart' (Barry and Born 2013: 42) of Barry and Born's edited collection *Interdisciplinarity* (2013). For example, Weszkalnys and Barry (2013) undertook a study of three interdisciplinary institutions, each of which gather researchers

from the natural and social sciences, in order to tackle environmental challenges: the Öko-Institut in Germany, the Tyndall Centre for Climate Change Research in the UK and the Earth Institute at Columbia University, New York. In introducing these case studies, they note that 'for some researchers, the environment has come to be understood not merely as an interface or zone of interaction between the natural and the social, but as a domain that problematizes the distinction between nature and society' (p. 179, see also Whatmore 2013). Weszkalnys and Barry argue that 'environmental research' is an example of what Daston calls an '"applied metaphysics"'; it 'contributes to and forms part of, as well as analysing, the world that it envisages' (p. 196). This complicates the relationship between world and word – that is the question of reference (see also Latour 1999a and chapter four of this thesis). Not only this, but, according to Weszkalnys and Barry, environmental research 'concern[s] itself with' and 'has come explicitly to interrogate its own entanglement in the world that it analyses' (p. 196). In addition, Weszkalnys and Barry argue that 'calls for the involvement of affected populations in research on environmental problems' derive not only from 'a sense of the potential contribution of non-experts' (p. 196) but also 'from a conviction that environmental problems are not objectively given in nature, independently of the multiple ways in which they are encountered, created experienced and valued' (p. 196). As such, 'the object of environmental research could be understood as *a different kind of object* from those typically encountered in the natural sciences' (p. 197, emphasis mine); indeed, the authors cite Greco's comment that the environment may 'in fact … no longer be characterised as an object at all' (Greco 2013 cited in Weszkalnys and Barry 2013: 197).

This attention to the sometimes complicated relationship between subject and object accords with the critique, set out in chapter four, of the strangely linear Latourian account of reference in *Circulating Reference* (1999a). However, while it provides impetus for examining the (inter-) disciplinary research objects of geomorphology, it also highlights an empirical gap in the literature. Barry and Born's collection attended to the relation between the natural and social sciences – a disciplinary conjunction regularly problematized and agonised over in geography (Whatmore 2013); however, relatively little work has considered interdisciplinary research occurring between the geosciences and the life sciences. This is the case notwithstanding Whatmore's comment that 'the vital connections between the *geo* (earth) and the *bio* (life) are arguably the most enduring of geographical concerns' (Whatmore 2013: 161), before going on to focus on the interface between natural and social sciences. Chapter seven of this thesis addresses this

empirical gap through a consideration of the interdisciplinary field of 'biogeomorphology' (Viles 1988).

SUMMARY

This section has examined recent literature on interdisciplinarity. It began with a discussion of the wide range of terminology employed in the literature. This was followed by a consideration of 'mode-2' research (Nowotny et al. 2001 cited in Barry and Born 2013: 9); the analysis concurred with Barry and Born in rejecting the reduction of all interdisciplinarity to a concern for the relevance of research to society and economy. Adapting Spinosa et al.'s concept of 'world disclosing' (1995: 3) entrepreneurship, the term 'research entrepreneurship' was proposed; this enabled a deepened conception of interdisciplinarity. A further insight from Barry and Born was acknowledged in their call to consider 'disciplinary boundaries' as relationally defined as 'in formation' (Barry and Born 2013: 8). Finally, the analysis concurred with Whatmore's observation regarding the importance of links between 'the *geo* (earth)' and the '*bio* (life)' in geography (Whatmore 2013: 161); however, it has noted that key previous work has focused instead on interdisciplinarity at the interface of the social and natural sciences. As seen in chapter one, this empirical gap sparks the second research question guiding this thesis: 'How are the spaces of geomorphological inquiry reworked to accommodate interdisciplinary (bio-geo) research objects?'. This question is tackled in chapter seven of this thesis.

GEOMORPHOLOGY IN THE STS LITERATURE

As Wainwright makes clear (2012), the STS literature on geosciences is sparse. Reference points are provided, however, by Rudwick's magisterial analyses of the history of geology (1976, 1985, 1996, 2005, 2008, 2010); Secord's discussion of the Geological Survey of Great Britain (1986), his analysis of Darwin's early geological work (1991), and his recent work on the Cambrian-Silurian controversy over geological strata (2014); and Oreskes's studies of *The Rejection of Continental Drift* (1999) and *Plate Tectonics* (2001). Additional work is provided by Richard Powell's ethnographic study of Arctic geoscientists (2007a; 2007b; 2008a; 2008b); Nigel Clark's conceptually rich, though empirically and historically underdeveloped discussion of the earth sciences (2010); Edwards (1999, 2010) and Yearley (2009) on climate science; and Trevor Barnes's analysis of the quantitative revolution in geography (2001, 2004, 2008a, 2008b). Finally, Wainwright (2011) provides a useful review of recent sociological literature on climate change and sets out a research programme that resonates strongly with the aims of this research, particularly when he asks: 'How does the culture of the lab and the field shape the

practice of geoscience?' (p. 177). This section examines four fleeting examples of geomorphologists appearing in the STS literature. It concludes with a consideration of methodological points of departure indicated by these literatures.

ENCOUNTERS WITH GEOMORPHOLOGISTS: LATOUR, WHATMORE, POWELL AND BARNES

As Wainwright points out 'there has been little use of STS ideas to explore the science of physical geography' (2012: 786). Four instances of geomorphology surfacing in the literature may be seen in (i) Latour's seminal discussion of scientists investigating Amazonian treeline advance (1999a); (ii) Whatmore's discussion of a RELU funded research project bringing together fluvial geomorphologists, human geographers and members of the public to tackle a flood prevention challenge (2013, see also Donaldson et al. 2010; Landström, Whatmore and Lane 2013; Whatmore and Landström 2011); (iii) Powell's discussion of 'geographical scientists' (2008a: 550) involved in the Canadian Government's 'Polar Continental Shelf Project during the late 1950s and 1960s' (2008a: 549); and (iv) Barnes's discussion of the quantitative revolution in geography (2001, 2004, 2008a, 2008b, 2009, 2014) - including a short case study following the geomorphologist Arthur Strahler (Barnes 2008a: 14). In the first three cases the geomorphologists are involved in interdisciplinary teams, while the fourth case features what Strahler himself termed a 'remarkable interdisciplinary transfer of information from hydrology ... to geomorphology' (Strahler, 1992: 69 cited in Barnes 2008a: 14).

Latour presents an ethnographic study of soil scientists, including a geomorphologist, who are investigating treeline advance in the Amazon (1999a). In his 'characteristically iconoclastic' (Hacking, 2002: 11) style of analysis, Latour seeks to examine the question of reference through a study of the transformations of materials from the field, through the laboratory, and eventually into an academic publication. The analysis is therefore typical of what Law calls 'old ANT'; it concerns itself with the displacement of scientific research objects across Euclidean space, and the stabilisations in network space required to achieve such feats. Considering the flamboyant title, *Circulating Reference,* Latour's analysis is surprisingly linear; each step of the chain of transformations he describes 'belongs to matter by its origin and to form by its destination' (1999a: 56). Chapters four and five of this thesis engage with Latour's account of space, research objects and reference in order to (i) provide an alternative conception of space to that articulated by proponents of ANT and ANT 2.0 and (ii) to consider how geomorphological research objects withstand displacement amid such redefined spaces.

The second STS account of geomorphology is, as mentioned above, described by Whatmore (2013), Donaldson et al. (2010), Landström, Whatmore and Lane (2013), Lane (2011), and Whatmore and Landström (2011). This research featured a highly original research design; undertaken under a UK Research Council funded programme, 'Rural Environment and Land Use (RELU)', the research involved an innovative collaboration of physical geographers (fluvial geomorphologists), human geographers and groups of local people who, together, formed 'competency groups' (Whatmore 2013: 170) tasked with finding solutions to flooding in 'two localities in the UK' (Whatmore 2013: 167). A requirement of the RELU funding indicated that the research needed to meet the criteria of constituting a collaboration between social and natural scientists. All of the project members undertook 'an intensive induction and training programme … in the working methods of the proposed research activities' (Whatmore 2013: 167). Whatmore explains that:

> These included (i) the ethnographic methods of science and technology studies (STS) being used to interrogate the modelling practices which inform flood management in the UK … ; (ii) the hydraulic modelling practices of the project team's 'in-house' flood scientists … ; and (iii) the philosophy of science informing the design of 'competency groups' (CGs), a methodological experiment involving extended collaborations between the whole project team (social and natural scientists) and people affected by flooding in two localities in the UK (2013: 167).

The methodology adopted for the flood risk research project was highly experimental. Arguably, at its heart there was a process of dialogue – something that continued from the project's inception 'during one of the regular sessions at the annual conference gatherings of British geographers dedicated to "bridging the human/physical [geography] divide"' (p. 165). The importance of such dialogue cannot be overstated. This point is also made by Barnes in his discussion of his research into the quantitative revolution in human geography; he explains how he learned to allow 'the interviewee more space to ruminate and stray, to be less fixated on my prepared questions' (Barnes 2014: 211). As a result, his 'interviews became progressively less interview-like, turning into conversations about a life lived, an oral history' (p. 211). The research therefore contributes to, or constitutes a moment in, ongoing discussions among physical and human geographers (Harrison et al. 2004; Harrison et al. 2008; see also Barnes 2009), and among 'geographers who can count, and those who cannot' (Barnes and Hannah 2001: 383).

A further instance of geomorphology surfacing in science studies may be seen in Powell's history of the 'Government of Canada's Polar Continental Shelf Project during the late 1950s and 1960s' (2008a: 549). Here he includes 'geomorphology' among what he terms the 'geographical science[s]' (p. 551). Powell notes that among his interviewees were

'geomorphologists', as well as other 'geographical scientists', 'who felt that they became geographers during fieldwork' (Powell 2008a: 550).

A particularly interesting section of Powell's analysis, from the point of view of this study, concerns one participant's reflections on the nature of fieldwork in geography. The participant, Bea Alt, expressed a measure of antipathy or regret at the manner in which Arctic science has evolved from its earlier more rugged forms of practice:

> Well now you have the Modeller who never sets foot in the Arctic and yeah, models the Arctic, has no idea what ice looks like, you know (Powell, 2008a: 559).

Present-day experimental work did not escape criticism either, as Alt remarked:

> And you have the person who goes up with some incredibly delicate instrument and measures in one little spot some really, esoteric thing (Powell, 2008a: 559).

Alt was keen, however, not to be seen as too critical of such approaches. She continued:

> Which is important, I'm not saying it's any less important. But, it doesn't, it doesn't have the, you won't spent fifty percent of your time trying to stay alive. You have much better equipment. Most people stay, only stay, for a very short time. You don't go and stay for three months. (Powell, 2008a: 559).

Powell's interviewee gave a striking sense of the embodied experience of doing fieldwork in a previous time, commenting:

> It was more of an adventure. It *was* more exciting. It was hands-on, you didn't have a bunch of instruments, you went out and looked at the thermometer. You know? You walked across the tundra and mapped by hand, you picked up rocks. You weren't spending half your time pouring over satellite pictures. You were discovering things (Powell, 2008a: 559, emphasis original).

These extracts from Powell's analysis highlight the relationship between modelling, measurement and embodied practice in fieldwork. Chapter four of this thesis provides new empirical material gathered in the course of this research project; together with a fresh theoretical approach this enables a detailed consideration of this theme.

A final example of geomorphology is given in Barnes's 'geo-historiograph[y]' (Barnes 2014: 202) of what he terms the 'quantitative revolution' (2001: 409) in geography. While his account centres on human geography (2004: 578), there are several points of particular relevance to the histories of geomorphology discussed in chapters five and six of this thesis. For example, Barnes notes Strahler's comment about a 1945 paper by Horton, commenting that the paper led to a 'remarkable interdisciplinary transfer of information from hydrology ... to geomorphology' (Strahler, 1992: 69 cited in Barnes 2008a: 14). Elsewhere, Barnes notes that J. A. Steers, then head of the Department of

Geography at the University of Cambridge, reacted with scorn to a paper presented by Peter Haggett at an RGS conference and featuring a 'multiple regression equation' (2004: 579). Points of connection between physical and human geography are also apparent. For instance, Barnes notes that David Harvey, who later became a critic of quantitative approaches to human geography, worked as a demonstrator on an undergraduate course in Cambridge (2008a: 15); run by Chorley and Haggett, it was the first programme in the Geography department to feature statistical and other quantitative methods. This thesis builds on these intriguing hints in Barnes's account; tracing the genealogies of techniques employed in a contemporary geomorphological laboratory, chapters five and six of this thesis provide additional nuance and detail to these histories of quantitative techniques in geography.

Barnes links the quantitative revolution to the cold war (2008a, 2008b). This is criticised by Johnston et al. who argue that 'the "revolution's" origins were more diverse and messy than the overly deterministic process that Barnes presents' (2008: 1802). Barnes provides a brief summary of Arthur Strahler's participation (Barnes 2008a: 14) in what he terms, following Pickering, the 'World War II regime' (Pickering 1995 cited in Barnes 2008a: 4). He notes that Strahler was principally funded by the Office of Naval Research (ONR) as were the nine PhD students, including Richard Chorley, that he supervised in the period 1950-1960. Barnes also describes what he terms the 'Pauline' experience (Barnes 2008a: 14) of Strahler in reading Horton's (1945) paper on a 'hydrophysical approach to quantitative morphology'. Finally, Barnes notes that when Chorley returned to Cambridge in 1958 he began, along with Peter Haggett, to teach quantitative techniques, commenting 'Chorley brought back from Cold War America a very different conception of physical geography' (2008a: 15).

Barnes comments that 'although the focus of a biographical approach is on the lives of individuals, it is still quite possible to include discussion of the wider political, social, and cultural context' (2001: 414). Given the potential for ethnography to attend to the non-human (Whatmore 2003: 93), a hybrid ethno-biographical[13] approach may further broaden the scope of a study to attend to the physical, embodied configurations at work in the generation of science. Powell provides a useful review of the use of biography in history of geography, arguing that there has been 'a "biographical" turn in histories of geography' (2008a: 549). Biography, like ethnography, '…gives us a way to tie together

[13] This term draws inspiration from Lorimer's neologism, 'geo-biographical', 'where biography becomes a spatial formation emerging through the multifarious spaces and landscapes produced in a life' (Lorimer 2015: 52)

the parallel currents of history at the level where events and ideas occur' (Hankins 1979: 5 cited in Barnes 2001a: 414); it finds its subject matter 'embedded in lived contradictions' (Young 1988: 123 cited in Barnes 2001a: 414). In this way an ethno-biographical approach blends well with a genealogical approach (as discussed in chapter three); this enables the telling of rich 'geographical stories' 'about people, about things and about texts' (Barnes 2004: 577).

Powell notes Livingstone's comment that an 'intellectual community with no awareness of its own story is … like a person suffering the loss of memory' (Livingstone 2007: 43 cited in Powell 2008a: 550). Conversely, however, Barnes discusses Barnett's contention that geographers should 'forget about the past and … act instead with no regard for what has gone before' (Barnett 1995: 419 cited in Barnes 2014: 204). He states that many histories of geography provide no rebuttal to Barnett's criticism (p. 205) and goes on to make a case for investigating the history of geography. Barnes draws on Faulkner to argue that, contrary to Barnett's account, 'the past is never dead. It's not even past' (Faulkner 1951 cited in Barnes 2014: 206); indeed, according to Barnes 'we carry the burden of the past into the present, bringing with us what went before' and 'present knowledge necessarily rests on past knowledge' (p. 206). This is indeed a strong rationale for historical studies of geography; however, Barnes's methodology does not reflect his acknowledgement of the connection between past and present. Barnes finds his research in archives to be characterised by 'a liveliness and emotional resonance' – indeed to have a 'pulse' (p. 221); indeed, he argues that archives are not to be understood as 'a fixed passive object … but as a set of distinctly embodied geographical and historical processes' (p. 224). However, while this allows a movement towards capturing the presence of the past in the present, a further step forwards may be found in applying a historically-sensitive, ethnographic methodology. In this way, it is possible to attend to the 'past … in the present' (Bloch 1977) as it shows up in the material configurations of present day geomorphological inquiry. This form of inquiry begins with feet firmly planted in the present, but with an awareness towards traces of the past – an interest in tracing the genealogies of techniques and practices and material configurations encountered in the present.

This section has seen four examples of STS research featuring brief references to geomorphology. It has also highlighted methodological considerations arising from this literature. It is clear that an ethnographic examination of geomorphology can build on these accounts, offering a powerful contribution to the sparse STS literature on the geosciences.

CONCLUSION

This literature review has examined key literatures each of which provide important points of departure for the study. Firstly, it critically examined the conceptions of space at work in 'ANT 1.0' and 'ANT 2.0' (Ek 2011: 10) arguing that both accounts retain the vestiges of a Euclidean spatiality. Secondly, it considered the literature on research objects, including the consideration of the foci of scientific research as (i) 'real *and* historical' (Daston 2000: 3, emphasis original), (ii) dispersed, (iii) boundary objects, and (iv) multiple objects. Thirdly, it considered the literature on interdisciplinarity. The term 'research entrepreneurs' was introduced, drawing on Spinosa et al.'s concept of the 'world-disclosing' character of entrepreneurship (1995: 3). Following the insight of Barry and Born (2013), it was noted that this conception of research entrepreneurship does not indicate that interdisciplinarity can be reduced to the relevance of research to economy and society. It was also noted that disciplinary boundaries should be understood neither as 'fixed nor fluid' but as 'relational and in formation' (Barry and Born 2013: 8). Finally, it was noted that there is an empirical need to investigate interdisciplinary work that occurs amid a 'geo' / 'bio' juncture.

CHAPTER 3: AN ETHNOGRAPHIC STUDY - METHODS, ETHICS AND POSITIONALITY

INTRODUCTION

This ethnographic research project was organised around two key case studies; these focused on rock weathering geomorphology and fluvial geomorphology respectively. The methodologies employed for the case studies included participant observation, interviews and analysis of relevant geomorphological texts. This was supported by additional work, including participant observation at meetings of the British Society for Geomorphology and interviews distributed across a wider range of participants; the latter was important both in (i) contextualising the core case studies and (ii) allowing the second case study to emerge as both relevant and viable.

This chapter begins with a consideration of ethnography as the methodology chosen for this research project. It continues with a section explaining key influences on the research design. This is followed by a consideration of multi-sited (Marcus 1998) and 'un-sited' (Cook et al. 2009: 65) ethnography. Following this there is a discussion of the three methodologies employed in this research project: participant observation, interviews and analysis of texts. While participant observation and analysis of texts are discussed briefly there is a detailed discussion of interviews explaining specific aspects of the approach taken in the study. A section on research ethics discusses consent and anonymity. Finally, there is a discussion of the researcher's positionality in the research project; unlike the rest of the chapter this section is written in the first person.

WHY ETHNOGRAPHY?

Ethnography provides the core methodology for this thesis. This choice of methodology has several advantages. Firstly, it offers rich qualitative description that renders available a sense of otherwise arcane lifeworlds. Secondly, of all the methodologies available to the social sciences, ethnography is particularly suited to attending to human - non-human configurations across a range of sites. As Whatmore argues, ANT has employed ethnographic methods to great effect, 'amplif[ying] … currents in this body of research practices' (2003: 93). It has applied multi-sited (Marcus 1998) rather than single-sited or 'un-sited' (Cook et al. 2009: 65) ethnography. Further, it has adopted 'a "symmetrical" approach … that redistributes attention from exclusively human actors … to the host of non-human devices, codes, bodies and instruments that are active parties in "doing" or practicing science' (Whatmore 2003: 93). While this thesis engages critically with the

accounts of space advocated by actor-network theorists, it takes inspiration from its valuable adoption of ethnographic research.

A third advantage of ethnographic research relates to its capacity, in a historically sensitive guise, to attend to the 'past … in the present' (Bloch 1977). This provides an opportunity to expand on the process of attending to the 'host of non-human devices, codes, bodies and instruments that are active parties in "doing" or practicing science' (Whatmore 2003: 93); it stimulates the tracing of the genealogies of techniques, practices and material configurations encountered in the present.

The ethnographic approach adopted in this thesis shares some common ground with discourse analysis in that it 'tries to explore how the socially produced ideas and objects that populate the world were created in the first place and held in place over time' (Phillips and Hardy 2002: 6); put more simply, it traces the genealogies of geomorphological research objects and attends to the role of discourses and texts in this process. However, it does not consider the 'ideas and objects that populate the world' (Phillips and Hardy 2002: 6) as being solely socially produced; nor does it adopt a 'strong social constructivist epistemology' (Phillips and Hardy 2002: 2). Instead, there is attention to the hybrid nature of research objects as social *and* natural entities (see Daston 2000).

INFLUENCES ON THE RESEARCH DESIGN

> Thought is like a path, says Heidegger, a way into and through the world, a movement toward a clearing (Jackson 1989: 2).

A key influence on the research design, as well as on the analysis of research materials, was found in Michael Jackson's (1989) emphasis on a Heideggerian approach to ethnographic research understood in terms of 'paths towards a clearing' (1989, see especially p.18). Here ethnographic inquiry is depicted as a process shaped by the exploration and opening out of pathways, as though walking through a forest, rather than relying on a view from above (1989: 8) – understood here as a research design entirely mapped out in advance (Mabry 2008: 216). This distinction is further elucidated by Mabry who distinguished between 'large-scale quantitative studies' that 'sample from broad populations and produce grand generalisations' and 'case studies' that 'provide deep understanding about specific instances' (2008: 216). Mabry argues that:

> Rather than carefully adhering to a design specified at the outset, when relatively little is known about a case, a qualitative case researcher is expected to improve on the original blueprint as information emerges during data collection. For example, if unexpected sources of data become apparent or if unanticipated aspects of the case come to light, the researcher is expected to capitalize on the

> new opportunities and *progressively focus* the study on the features of the case which gradually appear to be most significant (2008: 216).

This approach is echoed by Lenzo who draws on what Lather calls 'situated methodologies' (Lather 1993 cited in Lenzo 1995: 17); these imply that 'our practices arise from the specificities of our situations and cannot be prescribed ahead of time' (1995: 17).

A related influence on the research design is associated with Rivoal and Salazar's discussion of the role of serendipity in ethnography (2013). As Rivoal and Salazar, referencing Pertti and Pelto (1978: 185), comment:

> There has always been a tendency in anthropology for the 'field' to shape the research design, rather than the other way around. Accidental sagacity has been intimately bound up with innovation in anthropology and beyond ... It may seem ironic, but disasters often offer great potential for what Pertti and Gretel Pelto have called 'serendipity, disguised as catastrophe' (Rivoal and Salazar 2013: 180).

There is a considerable emphasis, in post-structuralist writing, on the 'becoming of the world' (Grosz 1999: 13; see also Clark 2003: 37) and on the 'agency of human-nonhuman assemblages' (Bennett 2009: xvii). As ethnographers it is important to design research attentively at the outset of the research project (see 'making the cut' below) and to cultivate ongoing opportunities for research proactively as the project evolves. Equally important, however, is a more receptive attitude: embracing the challenges and potential of the many aspects of research that are not in the immediate control of the researcher – whether these stem from the exigencies of the field or from the researcher's own circumstances (see 'positionality' below). As seen above, the nascent literature on 'serendipity' in research provides a valuable point of reference in this regard (Rivoal and Salazar 2013) as does Clark's discussion of 'The Play of the World' (2003: 28). However, balancing this, it is important to note Barnes's wry observation that 'the very last thing my data did was self-organise' (Barnes 2014: 203); this extends beyond analysis to data collection. Although themes emerge and research may be progressively focused the researcher's role in driving and shaping the research process remains vital.

Research may be understood in terms of an 'event' (Clark 2003: 40); it occurs at the intersection between the demands and lacunae of given theoretical, empirical, and methodological literatures; given research communities; the specificities of field sites; and the biography, training, and particularities of a researcher or team of researchers. A reflexive awareness of the ways a researcher's own biography and person shape the research process is vital to ensuring the validity of richly empirical qualitative research.

Again, this is returned to below in the section on 'positionality'. It is important to note that the researcher is not separated from the cases under study; as Jackson comments:

> Unlike traditional empiricism, which draws a definite boundary between observer and observed, between method and object, radical empiricism denies the validity of such cuts and makes the *interplay* between these domains the focus of interest … A radically empirical method *includes* the experience of the observer and defines the experimental field as one of interactions and intersubjectivity (Jackson 1989: 3-4).

In a related point, Coffey's discussion of the body in ethnography (1999) provides a toolkit for considering how the embodiment and positionality of the researcher shapes the research process; this is discussed further in the section on 'positionality' below.

A final influence on the research design is associated with George Marcus's emphasis on the 'need for ethnography to contextualise itself' (1998: 13), rather than deferring to other disciplines for contextualising narratives. This is reflected in the decision to contextualise the case studies with additional interviews. Since the focus for this research project is geomorphological inquiry, this point also accords with Barry and Born's call to consider 'disciplinary boundaries' as relationally defined as 'in formation' (Barry and Born 2013: 8). The next section considers multi-sited ethnography through an engagement with Cook et al.'s critical commentary and advocacy of 'un-sited fieldwork' (Cook et al. 2009: 65).

MULTI-SITED ETHNOGRAPHY

Cook et al. argue that multi-sited fieldwork was originally predicated on the idea that the objects of ethnographic research are single wholes that require investigation from multiple perspectives. They contest this idea, arguing that:

> … from the outset … we have doubted that it would make any sense to regard our proposed field locations – or indeed any set of locations we might propose – as parts which we could aim to piece together in order to arrive at an adequate description of some greater whole, the understanding of which would in turn contextualize and explain the several instances we observe (Cook et al. 2009: 50).

This is a powerful argument. Just as there is no single global Buddhism, underlying the many local Buddhisms encountered by Cook et al., there is no single 'geomorphology' underlying the multiple geomorphologies encountered in this research project.

Cook et al. progress their argument to state that '…there can still be good reasons for undertaking the kind of spatially extended study proposed by the multi-sited research programme' (2009: 58). Indeed, multi-sited fieldwork, just as single-sited fieldwork, can study multiple phenomena without assuming holism. This study adopts the terminology of

'geomorphological spaces of inquiry'; however, the prefix 'geomorphological' does not imply that these spaces all pertain to a single disciplinary phenomenon. The disciplinary objects of an ethnographic study can be just as 'multiple' as the scientific research objects (Mol 2002) encountered therein. Borrowing Barry and Born's phrasing (2013: 5), the discipline of geomorphology can be approached with a willingness to see it 'less as a unity and more as a field of differences, a multiplicity' (2013: 5).

In arguing against a concept of multi-sited fieldwork that assumes its objects to be single wholes, Cook et al. state that it is necessary to 'detach' (2009: 48) the concept of the ethnographic field from the concepts of 'space in two dimensions' (p. 64) and from the idea of a '"whole" cultural formation'[14] (p.64). They propose instead that 'the boundary of a field of study can be drawn to incorporate – by design – contrasts and comparisons that are germane to the theoretical questions that drive the research' (2009: 58; see fig. 1). In this way the locatedness of an ethnography is designated conceptually, by differences germane to the research questions, rather than in terms of a physical location in Euclidean space. They support this view with the important insight that:

> Even if working in a single village (more obviously so in a city) a researcher has to make decisions about what to include in the study and what to omit. One makes those decisions on the basis of the questions that are motivating one's research (2009: 65).

As argued in this thesis, the assumption of Euclidean space, as a container of social activity, is flawed; this applies as much to the spaces of ethnographic investigation as it does to the spaces of geomorphological inquiry that form the focus for the study. However, an account of space that is non-Euclidean can examine the role played by physical distance in defining the spaces enacted by cultural, and disciplinary, formations. The extent to which physical location shapes the spaces of geomorphological practice should be investigated through the research, rather than assumed at the outset. As Marcus points out, 'the contours [of a cultural formation] … cannot be presumed but are themselves a key discovery of ethnographic inquiry' (1998: 117).

This discussion of the multiplicity of the objects of ethnographic research, and the potential complexity of their relationship with physical locations, raises an important question articulated by Candea: '… if "the field" is a framing cut out of a seamless reality,

[14] This idea of a 'whole cultural formation' is termed a 'place' by Cook et al. (2009: 64); they explain that: 'Even groups such as men, women, the young and the old, and communities of all sorts would qualify as places in our terms insofar as those groups are specifically imagined as sharing certain characteristics that they do not share with members of other groups, that is, insofar as they are found inside a boundary, whether or not that boundary is a spatial one' (2009: 60)

how does one make the cut?' (Candea, 2009: 29). The next section considers this question and introduces the cases that formed the key foci for this research project.

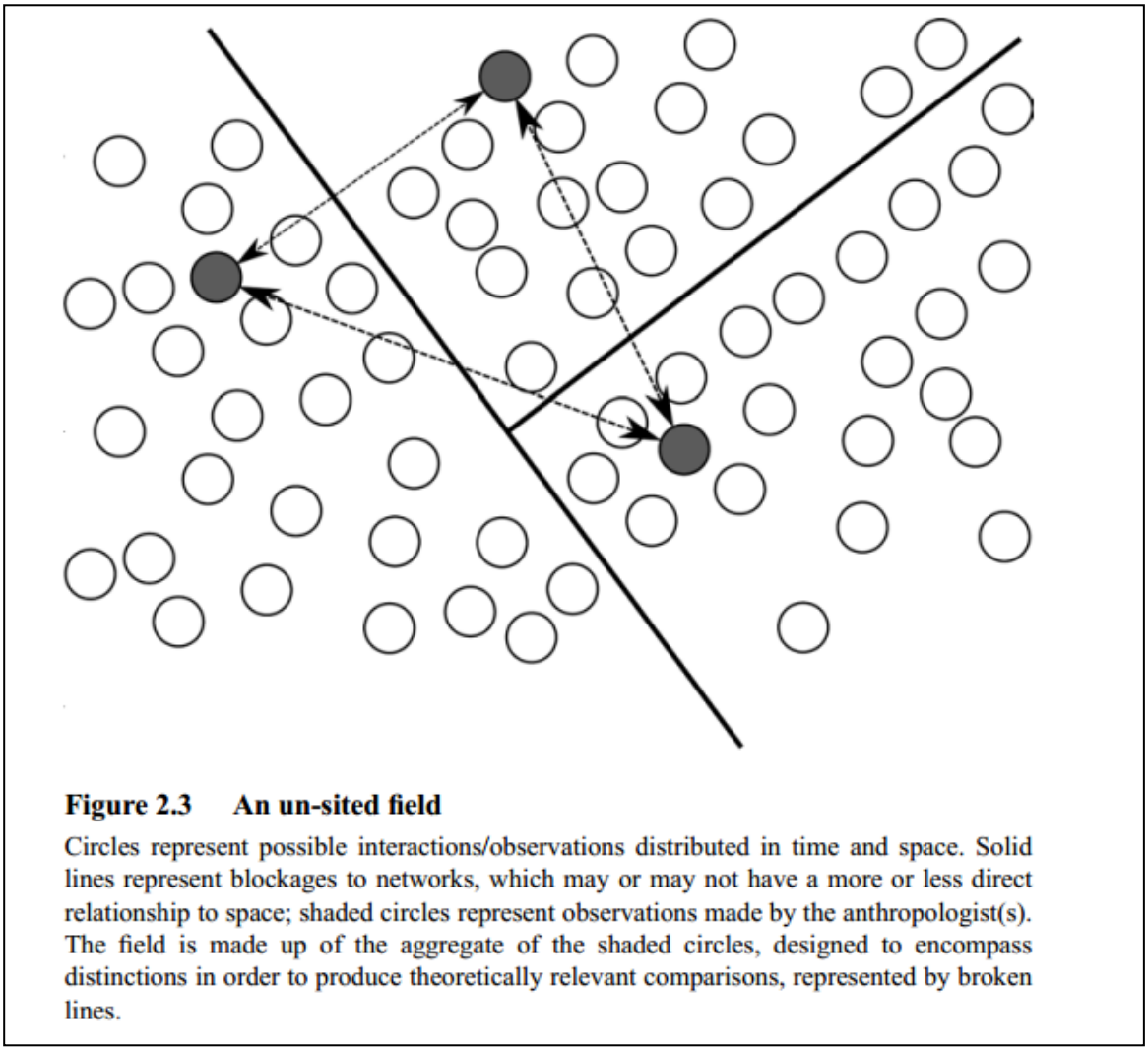

Figure 2.3 An un-sited field

Circles represent possible interactions/observations distributed in time and space. Solid lines represent blockages to networks, which may or may not have a more or less direct relationship to space; shaded circles represent observations made by the anthropologist(s). The field is made up of the aggregate of the shaded circles, designed to encompass distinctions in order to produce theoretically relevant comparisons, represented by broken lines.

Figure 1: An un-sited field (Cook et al. 2009: 65)

MAKING THE CUT: TWO CASE STUDIES

As Clark points out, however, embracing the play and eventfulness of the world in research does not absolve a researcher of the need to make decisions: 'the simple fact of being part of a world of endless possibilities means that we must choose some paths at the expense of others' (2003: 42). To achieve this the study did not insist that all these choices are made in advance; as seen above, the roles of serendipity and contingency in the opening out of the research-event were acknowledged. However, from an early stage the research worked with the objective of focusing on two case studies: fluvial and rock

weathering geomorphology. The cases were anchored in, but not reducible to, two institutional research sites. Both the cases and the sites were understood relationally rather than atomistically; this is reflected in the analysis, which does not hesitate to incorporate discussion of other related disciplines, techniques and sites, where relevant, in order to explore potential avenues of enquiry, trace specific threads, attend to contrasts and contextualise the analysis.

The cases and additional research chosen for this research project were not selected in order to shed light on an assumed 'single' or 'whole' discipline of geomorphology (see Cook et al. 2009); instead, they were selected since they enable analysis of specific geomorphological spaces of inquiry as 'multiplicities' (Barry and Born 2013: 5) and 'in formation' (Barry and Born 2013: 8). The research began close to home following the feminist injunction to start 'where you are' (Dombroski 2011: 21). The research was undertaken iteratively, in cycles that combined reading of both theoretical material (e.g. STS and geographies of science) and empirical materials (such as peer-reviewed geomorphological literatures, social media and histories of geomorphology), ethnographic research (participant observation and interviews), and writing (see Fig. 2). As such the research conformed to the description of Cook et al.:

> The ethnographer chooses a base, has certain research questions in mind, finds points in networks of people and places whose comparison promises to shed light on those questions, and then visits some of those points (people, events, and so on) in a sustained and concerted way, in the process, of course, revising the questions with which he or she began. It is only by means of a persistent (if productive) fiction that this activity could be construed as an attempt at a complete description of a bounded site (Cook et al. 2009: 58).

Figure 2: Iterative research process combining reading, research and writing throughout each year of research.

The ‘base’ selected as the starting point for research was the Rock Breakdown Laboratory associated with members of the Landscape Dynamics research cluster at the School of Geography and the Environment, University of Oxford. In this way, rather than seeking to escape the positionality of the researcher as an apprentice academic (doctoral student-geographer-ethnographer), in search of a 'neutral' point of departure, the research was anchored amid this positioning. The associated potentiality and challenges were teased out during, and provided impetus to, the research process. Marcus argues that:

> Working from very deeply probed affinities of varying sorts has often been the way in which the most interesting research proposals have been generated among students. The projection of these affinities from the realm of the more personal … is the key move which gives a project substance and force, and also more legitimacy in the mainstream tradition of social science writing (1998: 15).

An analogous ‘key move’ provided a departure point for this research. Following a departmental seminar, I intentionally attended the ‘wrong’ research cluster meeting; instead of joining the human geographers at the ‘technological natures’ or the ‘transformations’ cluster meetings, I went along the corridor to the meeting of the ‘landscape dynamics’ cluster. Here I joined an informal group of strangers viewing a series of research posters displayed in one of the teaching rooms where I had undertaken my MSc training. This provided an opportunity to strike up conversation with members of the research cluster, and, in particular, with Professor Heather Viles - who became an important, and extremely helpful, ‘gatekeeper’ and ‘key informant’. Following on from this encounter, I was generously given the opportunity to present a summary of my research to a second meeting of the research cluster. I was then added to the group’s email list, given access to the laboratory, and invited to talk with and negotiate participant observation and interview opportunities with groups (see table 1) and individuals (see table 2). I participated in events exploring new equipment and undertook participant observation at the laboratory and associated local field sites.

Table 1: Group sessions - Participant observation and interviews with members of the Rock Breakdown Laboratory and other members of the Landscape Dynamics research cluster at the School of Geography and the Environment, University of Oxford.

What	Where / Who with	When
Attended (participant observation)	Posters session by members of the Landscape Dynamics research cluster (encompassing members of the Rock Breakdown Laboratory)	January 2012
Presented my research aims and invited participation in the research	Meeting of Landscape Dynamics research cluster	February 2012
Participant observation	Demonstration of new 3D imaging equipment at Rock Breakdown Laboratory	July 2012
Attended	Andrew Goudie retirement symposium	November 2012
Participant observation	Visit to local field site	February 2013
Participant observation	Demonstration of endoscope equipment at Rock Breakdown Laboratory	March 2013
Participant observation	Visit to a local field site	April 2013

Table 2: Individual sessions with members of the Rock Breakdown Laboratory at the School of Geography and the Environment, University of Oxford

What	Where / Who with	When
Repeated in-depth interviews	Professor Heather Viles	February 2012 May 2012 September 2014 October 2014
In-depth interview	Professor Andrew Goudie	October 2014
Participant observation in laboratory and at local field sites, unstructured and semi-structured interviews	Early career researcher based at Rock Breakdown Laboratory	February 2012 April 2012 July 2012 August 2012 February 2013 March 2013 April 2013 (3 visits) June 2013 July 2013 June 2014
Participant observation in laboratory	Early career researcher based at Rock Breakdown Laboratory	February 2013 March 2013 (2 visits) April 2013 (3 visits)
Participant observation and unstructured interviews	PhD student based at Rock Breakdown Laboratory	December 2012 January 2013 July 2013 June 2014
Unstructured interview, informal meetings, ethnographer proofed three chapters of doctoral thesis	PhD student based at Rock Breakdown Laboratory	February 2012 March 2013 December 2014-January 2015 (proofing thesis) January 2015 (skype call) April 2015
Semi-structured interview	PhD student based at Rock Breakdown Laboratory	April 2013
Semi-structured interview	MPhil student	February 2012
Participant observation	Laboratory manager	July 2013

I also interviewed geomorphologists who, although members of the Landscape Dynamics research cluster, were not associated with the Rock Breakdown Laboratory (see table 3). Again this provided the opportunity to undertake exploratory work and to get a sense of the differing spaces and practices associated with geomorphological inquiry of various kinds.

Table 3: Individual sessions with other members of the Landscape Dynamics research cluster at the School of Geography and the Environment, University of Oxford

What	Where / Who with	When
Interview	Mid-career geomorphologist	December 2011
Interview	Mid-career geomorphologist	February 2012
Participant Observation	PhD student	July 2012

The 'at hand' nature of research was not exhausted by physical proximity; proximity could take other forms. For example, early in the research process interviews were carried out with geomorphologists who had already collaborated with human geographers, engaging in conversations 'across the divide' (Whatmore 2013: 164); such interviewees were academically, rather than physically, proximate[15].

Building on, and overlapping with research carried out at the Rock Breakdown Laboratory and associated field sites, the researcher attended two annual conferences, annual general meetings and early career researcher workshops organised by the British Society for Geomorphology (BSG) (2012 in Nottingham and 2014 in Manchester, see table 4). The researcher also attended a BSG meeting on flooding and climate change (2014 in Newcastle, see table 6). This provided an important opportunity to develop a 'network of contacts loosely based around the germ of your project' (Crang and Cook 1995: 14). The three meetings, in Nottingham, Manchester and Newcastle, were important both in the recruitment of participants, in providing sites of informal participant observation, and for the conduct of both formal and informal interviews.

[15] See chapter four for a discussion of different kinds of proximity and their relationship to a revised concept of space.

Table 4: Participant observation at meetings convened by the British Society for Geomorphology (BSG)

What	Where / Who with	When
Participant observation	BSG 2012 Annual Conference, Nottingham: 'Are we putting the model cart before the data horse?' Including early career workshop and annual general meeting	3 day event – June 2012
Participant observation	BSG 2014 Annual Conference, Manchester Including early career workshop and annual general meeting	3 day event – September 2014

The British Society for Geomorphology was, it turned out, connected to Oxford as well as other UK and international academic institutions; key informants held committee positions at the BSG during the period of research. Research undertaken at the BSG also provided an opportunity to contextualise the study at the Rock Breakdown Laboratory. However, this occurred without assuming that the discipline of geomorphology forms a coherent 'whole' (Cook et al. 2009: 50), of which the science undertaken at the Rock Breakdown Laboratory could, unproblematically, be seen as a (representative) example or part. Instead, both the science encountered at the BSG, and that encountered at the Rock Breakdown Laboratory, were apprehended with the expectation that they pertain to interconnected, but multiple and fragmentary, disciplinary formations.

The final research case emerged, through a process of exploration and sifting, from the work undertaken at BSG meetings and during additional interviews and research (see table 6). It featured 'depth interviews' (Bloor and Wood 2006: 104) with three individuals: an early career, mid-career and senior fluvial geomorphologist (see table 5). All of these scientists had trained, or undertaken formative research, at the same UK academic institution. Two of the interviewees had been supervised or employed by the same individual while two had undertaken formative (PhD or early career) research at the same field site. In addition, the ethnographer had the opportunity to visit the aforementioned field site during the course of the doctorate. A further reason for this selection related to

work two of the interviewees had undertaken in biogeomorphology – a sub-, or inter-, discipline already encountered in the Rock Breakdown Laboratory and discussed in detail in chapter seven.

Table 5: Individual meetings with fluvial geomorphologists for case study two

What	Where / Who with	When
Depth interview	Senior fluvial geomorphologist	February 2015
Depth interview	Mid-career fluvial geomorphologist	October 2014
Depth interview	Early-career fluvial geomorphologist	August 2015

This selection of interviewees reflected the assumption that physical distance *may or may not* be of primary importance in the definition of (i) the spaces of geomorphological inquiry and (ii) the ethnographic field. In this way it was a partially-sited, partially 'un-sited' (Cook et al. 2009: 56), case study. It therefore benefited from the advantages described by Cook et al.:

> What was so productive in the original multi-sited research programme was the opportunity to liberate ourselves not so much from a bounded site, as from the idea that the field of ethnographic research could ever be coterminous with, or the same thing as, a geographically bounded location or area. Abandoning such an idea of a necessarily sited field makes it possible to admit that it never was possible to achieve a complete description of any area or group of people; but in exchange for acknowledging that fields are always constructed out of a too-rich reality, we would gain the freedom to determine their boundaries explicitly, in relation to our research questions (Cook et al. 2009: 58).

However, this research project didn't go as far as Cook et al. (2009) in severing the inevitable situated-ness of ethnographic research from a physical location. Instead, it adopted the purview of an exploded diagram; all the participants having worked or studied at the same institution during formative periods in their careers, but having each now moved on to establish careers influenced surely by this formative work but also by innumerable other factors.

Table 6: Additional exploratory and contextualising research

What	Where / Who with	When
Interview	PhD student in geography	November 2011
Interview	Mid-career geomorphologist	December 2011
Interview	Senior fluvial geomorphologist	January 2012
Interview	Senior glacial geomorphologist	February 2012
Interviews	PhD student in Earth Sciences (volcanology)	February 2012 April 2013
Participant observation	Flooding and climate change workshop	June 2014
Interview	Senior fluvial geomorphologist (consultant)	September 2014
Interview	Early-career fluvial geomorphologist	September 2014
Semi-structured interview	Mid-career ecologist investigating interactions between ecological and geomorphological processes	October 2014
Participant observation	Field visit – Ecology	June 2015

METHODOLOGIES EMPLOYED: PARTICIPANT OBSERVATION, INTERVIEWS, AND ANALYSIS OF TEXTS

> … qualitative methods prominently feature three data collection techniques: observation, interview, and the review and analysis of site-generated or -related documents (Mabry 2008: 218).

Participant observation included repeated visits to the Rock Breakdown Laboratory, to associated local field sites, and to the meetings of the British Society for Geomorphology discussed above. In relation to the Rock Breakdown Laboratory, visits were arranged via email, or the ethnographer would call in to the laboratory; sometimes a visit would be sparked by an email or text from a participant suggesting the ethnographer might be interested to join participants for a particular workshop or experimental activity. Participant observation at the Rock Breakdown Laboratory was complemented with informal interviews and dialogues occurring as participants undertook experimental work, or at various locations close to the laboratory: a participant's college, a common room, a participant's office, a classroom or a nearby park.

Text-based research had two roles in this research project. Firstly, geomorphological literatures, including the rich materials discussing the history of the discipline, provided an empirical data source in themselves. Secondly, reading geomorphological texts provided important preparation for interviews and participant observation. Such work enabled the researcher to conduct interviews and participant observation at a sufficient depth to provide useful data and, importantly, to secure the respect of interviewees. As the research design adapted to circumstances, text-based research took on a greater importance in the research project; this enabled the researcher to probe the genealogies of techniques and research objects encountered during participant observation.

INTERVIEWS

The interviews undertaken for this research ranged from loosely structured 'depth' interviews (Bloor and Wood 2006: 104) to conversations carried out in the process of participant observation. This section discusses the approach taken in conducting these interviews.

CONSULTATION DOCUMENTS

Significant preparation was carried out in advance of formal interviews in order for the researcher to familiarise themselves with the interviewee's field of research and written work. Following this preparatory work, the researcher developed a 'consultation document' (see appendix 1) – an A4 document with information about the research project and several questions designed to engage the participant's interest and enthusiasm for the research and to elicit additional information. While generic consultation documents were prepared for use during ad hoc research encounters (for example for informal conversations conducted on the fringes of the BSG annual conference), the majority of consultation documents were carefully tailored for individual interviews and emailed to participants ahead of the interview. These documents served to gently structure interviews: they demonstrated the seriousness of the researcher and acknowledged the value of the participants' time as well as attempting to acknowledge key themes associated with the participants' professional work and identity. In addition to the consultation documents the researcher usually prepared a more extensive list of questions; these acted like a shopping list: the process of making the list was as important as referring to it while the interview was underway.

Consultation documents, together with preparation held in written notes and in the researcher's memory, provided a 'sea anchor' to the interviews – tethering them flexibly to the core themes of the research but allowing plenty of scope for spontaneous departures.

The researcher would rarely refer to the consultation document in the course of the interview, except very occasionally as a prompt, helping her to maintain the flow of the interview. Instead, after starting off with an initial question the researcher would allow the interview to develop as a conversation (see Barnes 2014: 211). On several occasions, however, and sometimes to the surprise of the ethnographer, an interviewee would refer to a question mentioned on the consultation document – sometimes having annotated the latter in advance of the meeting. This might occur in the course of a discussion about a separate point – the interviewee recalling a point they wished to make. Alternatively, towards the end of the interview an interviewee would bring the conversation back to a point on the interview consultation document, anxious to make a point in relation to a particular question that had not yet been covered.

Barnes, similarly, notes that he learned to 'allow the interviewee more space to ruminate and stray, to be less fixated on my prepared questions'; in addition, he explains that 'the interviews became progressively less interview-like, turning into conversations about a life lived, an oral history' (2014: 211). Barnes continues to state that 'people were so enthusiastic to talk. In some cases, it was as if they had waited their whole life to be asked' (2014: 211). This quality of 'intersubjective bridge' (Bloor and Wood 2006: 104) is, however, an achievement of both interviewee and ethnographer; it demands labour and engagement on both sides. Participants are often extraordinarily generous in sharing highly reflexive accounts of their working lives. Barnes notes that 'I gathered material without asking questions' (2014: 211); however, this does not indicate that preparation is unrequired. The ethnographer, for their part, must endeavour to prepare with diligence and care; to make efforts to ensure that they warrant the trust shown by interviewees as they share their stories.

AD HOC INTERVIEWS

In the course of this research some opportunities to conduct informal interviews occurred spontaneously. The researcher developed a generic 'consultation document' for use during participant observation at geomorphological conferences. On occasion this resulted in an 'ad hoc' interview carried out, with minimal time for specific preparation, on the margins of the event. Again this does not imply that preparation is forgone. Instead, it simply indicates that another mode of preparation is in play; in such a situation the researcher may draw on prepared generic questions and, importantly, on all the knowledge gathered during the course of the research to date.

AUDIO RECORDING

The recording of interviews was introduced in the course of the research project. Bloor and Wood (2006) note that Wilkie (1963, cited in Bloor and Wood 2006: 16-17) 'provides an early example of a comparison between audio-recorded data and data in which the interviewer maintained handwritten notes in the context of social worker and client interviews' (Bloor and Wood 2006: 16-17). They note that:

> It was only when the social workers' notes were compared to the full transcripts that additional information about the clients' situations emerged. Wilkie also makes the point that by audio-recording the practitioner has an opportunity to reflect on and improve his or her interviewing techniques (Bloor and Wood 2006: 17).

The researcher certainly found that very important material, not recalled from the interview itself, was captured in the recordings. As seen below the researcher used Sonocent's *Audio Notetaker* software to aid transcription and analysis.

In addition to recording interviews, on one occasion, with the participant's consent, the researcher audio recorded not only an informal interview carried out on the bus trip to a participant's field site, but also left the recorder running during participant observation at the field site. This enabled the researcher to capture rich contextual materials. This also served to elicit clearer memories of the research encounter when writing up field notes following the research trip.

INTERVIEW ANALYSIS

As discussed above, interview findings were treated as narrative co-productions of interviewee and interviewer. In keeping with this understanding of the data, the analysis of material gathered during interviews was engaged with through a process of listening (during the interview and, importantly, afterwards), transcription, and review. Themes emerging from the research materials were identified and an index of the empirical materials built with page numbers and extracts included; this took the form of a simple word document with tables for each theme.

SOFTWARE

Markle et al. comment that 'in the process of transcription some meaning is lost and eventually forgotten' (2011). They argue that 'qualitative researchers have evolved their methods continually, often due to technological breakthroughs' and they 'assert that the qualitative research community is currently on the precipice of another such change, specifically in the transcription of audio and visual data'. They argue that '[p]erhaps even more important than the tools researchers use to analyse ... data are the current

technological advances that enable readers to view multimedia data alongside a traditional text narrative' (Markle et al. 2011).

One example of such technology is Sonocent's *Audio Notetaker* (see fig. 3 and fig. 4). This software was developed for use by students with dyslexia. It provides a visual representation of audio materials: small bars represent short phrases - approximately the length of individual sentences. These chunks can be colour coded; for example, to highlight particularly important sentences. They can also be grouped into chronological sections, corresponding, for example, with a particular question, and the interviewee's response.

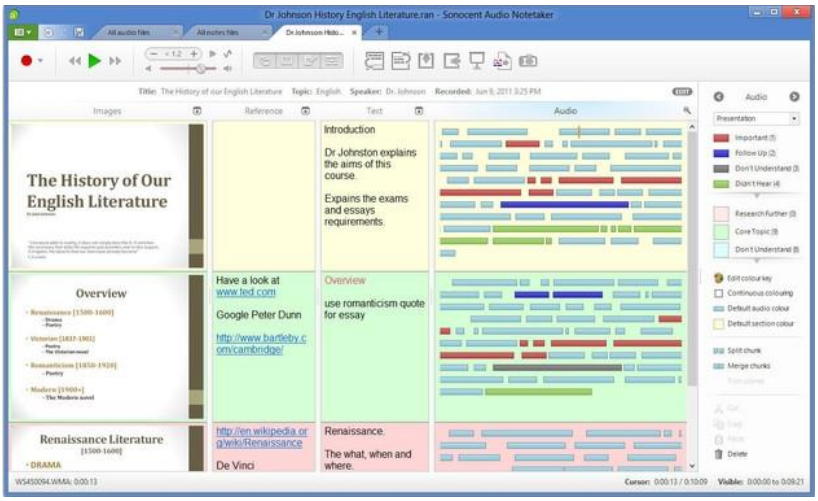

Figure 3: Screenshot from Sonocent Audio Notetaker (ETC Educational Technology Connection (HK) Ltd, 2016)

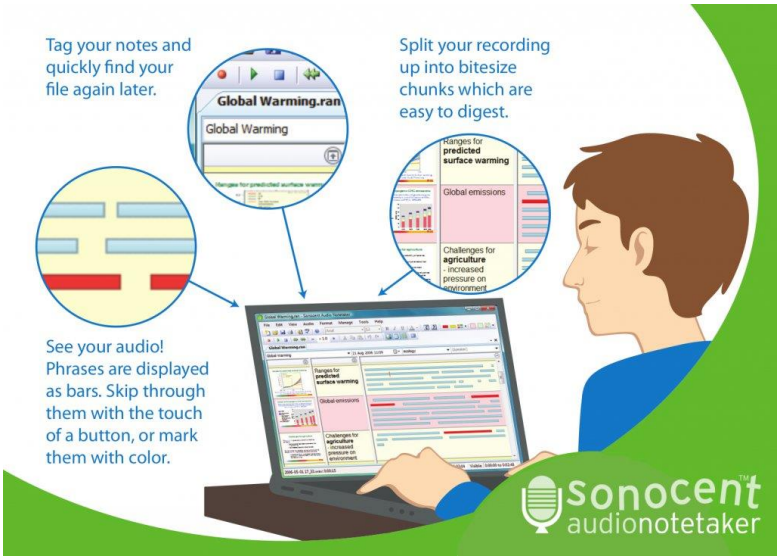

Figure 4: Diagram explaining Sonocent Audio Notetaker (ETC Educational Technology Connection (HK) Ltd, 2016)

These characteristics are extremely valuable in 'enabl[ing] readers to view multimedia data alongside a traditional text narrative' (Markle et al. 2011: n.p.). This makes it very easy to return and listen again to a particular section of an interview. While in practice it is still usually necessary to transcribe interviews fully to enable detailed analysis, the software does allow a researcher to break an interview down into sections quickly and to transcribe only the relevant material in order to do an initial analysis. Images, for example non-identifying photos taken in the field, or scans of handwritten interview notes, can also be added to individual sections of the text. Similarly notes on the text can be added to individual sections – identifying, for example, emerging themes. This software proved extremely valuable to the process of transcribing and analysing audio recordings of interviews and additional recordings made during fieldwork. It not only 'improve[d] the efficiency and authenticity' of the research, but also added to the ability of the researcher to 'stay immersed in the data' (Markle et al. 2011: n.p.); the audio recordings are valuable in triggering a broader and richer memory of the research setting once the researcher is removed from this environment during the analysis and writing up process.

RESEARCH ETHICS

CONSENT

> Ethnographers typically think of data as a gift from their informants, with all the implications of reciprocity that gift exchange implies (Falzon, 2009: 1).
>
> Participants have a stake in the accuracy of how they are presented and in whether case accounts are flattering or damning … [they] may suffer at the hands of an external other whom they have allowed into their communities (Mabry 2008: 221).

Participants taking part in participant observation at the Rock Breakdown Laboratory, or in formal interviews carried out throughout the research project, were given an explanation regarding the nature of the research and were asked to complete a consent form (see appendix 1) detailing whether they preferred to remain anonymous or to be named in the written up research. In some cases, participants who had indicated that they were happy to be named were in fact treated as anonymous in order to protect the anonymity of other participants.

During research conducted at the British Society for Geomorphology, the researcher spoke openly with attendees regarding the research she was conducting – including on one occasion addressing all participants at an early career workshop in order to inform them about the research and request participation. Similarly, she carried a generic 'consultation document' describing the research project and handed these out during

discussions with conference attendees. This sparked many informal conversations, which together with participant observation at the conference, informed the research project. However, such material was not cited directly in the write up since it relied on tacit rather than formal consent. In two cases, formal interviews were arranged on the margins of the BSG conferences. One of these interviews was pre-arranged while another was spontaneous; however, in both cases formal consent was requested and given.

ANONYMITY

Given the institutional and physical proximity between research participants and the primary audience for the research, additional sensitivity was required in the course of the research. In relation to research undertaken at the Rock Breakdown Laboratory, anonymity was adopted for early career research participants and doctoral students; however, with their agreement, anonymity was not given to more senior participants as they would have been too recognizable in the accounts. Early career researchers and doctoral students at the Rock Breakdown Laboratory were also given the opportunity to read early drafts of the sections of text discussing their input to the project prior to this research being seen by their more senior colleagues. This provided a helpful opportunity for discussion and feedback and in one case a participant kindly responded by providing additional references and photocopied articles to contribute to the ongoing research. More senior members of the Rock Breakdown Laboratory were also given access to some early drafts of written work, though due to the timeframes of the research project this was not possible for all the material. Again this provided a valuable opportunity to discuss the research, benefitting from thoughts and comments on the early drafts.

Some fluvial geomorphologists requested anonymity at the start of the interview – though with an understanding that they may, at times, be recognisable in the text given the relatively small communities of geomorphology and of geography. Names of people and places were changed in the text to protect anonymity where relevant.

POSITIONALITY

The obvious type of positioning of the researcher in relation to this project was as a graduate student at the School of Geography and the Environment. Throughout the early stages of my doctorate, my supervisor's office was adjacent to the office of a key informant and gatekeeper. This had, to my knowledge, no material impact on the research but it brings home the importance of Cook et al.'s observation that 'ethnographic fieldwork is something that could be done no less if one were to travel no further than half

an hour's walk from one's study than if one were to travel all over the world' (2009: 65). This positioning as a student was important in my participant observation and interviews with senior, mid-career, early career and PhD student geomorphologists. This is reflected on further below. First, however, it is necessary to reflect on the 'embodied' and affective nature of ethnographic research; my own peculiar circumstances at the time of the research shaped this research project as much as any initial research design or the contingencies of fieldwork (Rivoal and Salazar 2013: 179). As Coffey comments:

> We cannot divorce our scholarly endeavours from the bodily reality of being in the field (1999: 68).

In the course of a doctorate several years of life ensue; such a time may be eventful for a researcher and for me this was certainly the case. I lived throughout the doctorate with an, initially undiagnosed, traumatic brain injury; this impacted directly on my research – affecting my ability to structure my ideas into written work, to organise my time and to process information. The period prior to diagnosis was extremely stressful and saw me struggling to work within the frameworks of my programme of study; this impacted on mine and others' perception of myself. Whether or not reality bore this out, I felt, for a time, like a persona non grata in the environment of the School of Geography and the Environment (this could also be understood in terms of an accentuated case of 'imposter syndrome' (LFPS Lincoln Film Production Society 2012)). My doctoral studies were also affected by a sudden bereavement, a diagnosis of bi-polar disorder and, connected to the stresses of this time, the development of a chronic pain condition. Through these challenging times my general fitness levels were also significantly impaired. While the medical conditions were all long term, in the short term, corresponding to the period of my doctoral studies, I experienced the surfacing of each problem, diagnosis, and gradual steps to managing new challenges, as conforming, approximately, to a Kübler-Ross change curve (see Fig. 5). At first my levels of confidence and morale were steady but these fell significantly as each condition took effect; a turning point was often associated with diagnosis and this was followed by a gradual return of confidence and morale. Due to the staggered, overlapping impact of each condition these change curves had a summative effect; this made for a bumpy ride indeed.

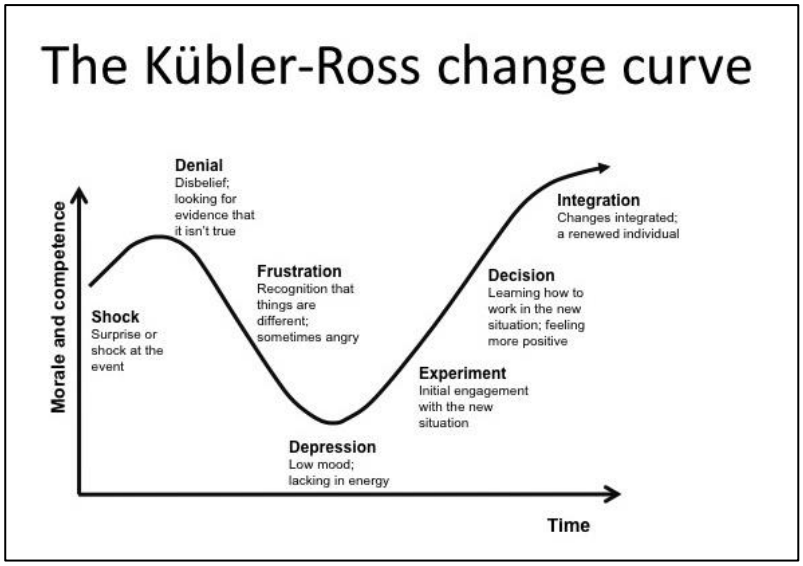

Figure 5: The Kübler-Ross change curve (Marlow, 2014)

In ethnographic fieldwork, the body is a key tool for research. Coffey comments on the role of her body during research:

> During my own fieldwork … I was extremely conscious of the need to manage and produce an acceptable body to the fieldsite. I was concerned with presenting a personal front which mirrored that of the social actors in the field. I attempted to dress 'like an accountant'. This in itself was based on my assessment of the acceptable body in accountancy – as smart, self-assured, confident and well managed (Coffey 1999: 65).

The maintenance of an 'acceptable body' (Coffey 1999: 65) is key in the context of both participant observation and in, what Ezzy calls, the 'embodied emotional performance' of 'qualitative interviews' (2010: 163). My experience during the period of my research was that of a body in turmoil; this made it more difficult to present as 'self-assured, confident and well managed' (Coffey 1999: 65) – traits as important when positioned as a graduate student in an academic setting, at academic conferences and during depth interviews and participant observation, as in the accountancy context Coffey describes. Further challenges were associated with my perception, partially reinforced and partially confounded in the course of fieldwork, that physical geographers, and those accompanying them into their field sites, would need to be very physically fit or would risk to lag behind noticeably, potentially becoming a nuisance in the field (though see Bracken and Mawdsley 2004 for 'a critique of the dominant image of physical geography fieldwork as essentially a masculinist endeavour' and an attempt to 'encourage a wider view of the challenges and pleasures of fieldwork for women in geography' (2004: 280)).

The solution was to shift the balance of research to incorporate additional text-based research; this enabled greater attention to the genealogies of geomorphological techniques and to histories of geomorphology. By reducing the intensity of participant observation and interviews, spreading these out over a longer time period, I was able to undertake detailed preparation and to present myself professionally for fieldwork; this resulted in a very high quality of dialogue and interaction. Returning to conduct repeat interviews, and participant observation, over a longer period enabled a longitudinal engagement that would normally be difficult to accommodate in a doctoral study. In addition, interviews and participant observation were deepened by the fact that I had been able to engage more deeply with geomorphological literatures. In this way, the particular circumstances associated with my disability presented as 'serendipity, disguised as catastrophe' (Pertti and Pelto 1978 cited in Rivoal and Salazar 2013: 185).

Indeed, the thoughtful and receptive response of participants, during both interviews and participant observation, did much to boost my morale, and my sense of identity as a professional person. As mentioned above, the effect of my health difficulties on my sense of belonging in the School of Geography and the Environment, and in academia more generally was profound. The department had been the site of significant personal challenges; as a result, for a time just entering the department was enough to trigger symptoms of anxiety. It was necessary to overcome this anxiety to conduct effective research – and similarly, conducting research was part of my means to overcome this anxiety since it promoted, as mentioned above, a positive and professional sense of self. I developed a strong rapport – typical of peers and colleagues - with key informants at the Rock Weathering Laboratory; I remain indebted to them as they would most likely not have been aware how their warmth and genuine response to my research also helped to heal a battered sense of confidence.

In order to differentiate between roles and experiences of the School of Geography and the Environment, I found myself using different entrances to the department for different purposes. I tended not to use the main entrance, which I had used throughout my MSc to enter the building, and instead entered by the side entrance adjacent to the Rock Breakdown Laboratory in order to conduct interviews and participant observation. Conversely, when visiting my supervisor, I entered by a side entrance at the opposite end to the building – at the foot of a stairwell leading to the third floor offices.

A further aspect of my positionality when conducting research at the School of Geography and the Environment, University of Oxford, is connected to shades of what Doreen Massey describes in the following passage:

> I had felt the same thing while at Oxford University and had the same wrestle there, agog at the beauty of Radcliffe Square in the lamplight at night and angry and alienated at what it stood for. Feeling like a space invader. These are the wrenchings of a displacement effected by class (Massey 2010: n.p.).

This sense of class-based positioning doubtless impacted on my recruitment of research participants; while I was professionally and personally used to working across differentials of class, ethnicity, and gender, I tended, particularly during the period of personal difficulty described above, to gravitate towards participants who, for reasons of gender, class, nationality, or personal disposition, did not appear to conform to, or represent, the establishment described by Massey. In this regard, I found the ethos at the Rock Weathering Laboratory, in the School of Geography and the Environment, to be highly inclusive in disposition; this was of significant importance to the success of the research project.

HOW BELONGING TO SCHOOL OF GEOGRAPHY AND THE ENVIRONMENT IMPACTED ON THE REST OF MY RESEARCH

My positioning as a Geography graduate student was likely to have been important in securing access. I occasionally referred, during interviews and participant observation, to the fact that my undergraduate degree had been in social anthropology (in fact it had been in social anthropology and theology but I mentioned only anthropology as being the most relevant to my current study); this positioned me as an insider-outsider. In common with other Oxford students, no doubt the 'brand-name' of Oxford was also of some help in opening research avenues; indeed, the logo of the university was included on both the consent forms and 'consultation documents' discussed above.

My positioning as someone who had conducted research at the Rock Breakdown Laboratory was perhaps of some importance in the context of some of the other interviews and participant observation I conducted; indeed, one participant inquired as to whether Professor Heather Viles, a key informant and gatekeeper in the research project, was my academic supervisor. However, although this may have subtly shaped interactions, it did not do so in a way that was obvious at the time; certainly, no negative consequences of such positioning could be detected.

EMBODIED ANALYSIS

The role of embodiment and positionality, discussed here in relation to the research process, also extended into the analysis of empirical materials. As seen previously, where audio recordings were made, these were either partly or fully transcribed using Audio Notetaker; this enabled an easy movement back and forth between transcriptions and the original audio material during the process of analysis. Additional fieldnotes were made during and following research activities, sometimes in notebooks and sometimes in Microsoft Word or OneNote documents depending on the medium at hand. In addition, and importantly, 'writing trials' (Latour 2005: 134) were undertaken at intervals during the period of empirical research. As Latour comments:

> It is impossible to imagine that one would gather the data for a period of time and only then begin to write it down. Writing a report is too risky to fall into this divide between enquiring and reporting … ideas, paragraphs, metaphors, and tropes might come haphazardly during the course of the study. If they are not allowed to find a place and an outlet, they will either be lost or, worse yet, will spoil the hard work of data collecting by mixing the meta-language of the actors with that of the analysts. So it is always good practice to reserve a separate space for the many ideas that may spring to mind even if they will only be used years later (2005: 134-135).

This process of making 'writing trials' was particularly important given the incremental research process which combined interwoven stages of reading, research and writing (see fig 2). In some cases, for example the first half of chapter seven, a 'writing trial' was developed early in the research process, and evolving versions of it were shared with two key research participants, benefiting from their comments and responses. It was then, as Latour indicates, put to one side for a considerable length of time, while additional research was ongoing, then returned to towards the end of research for editing and incorporation into a final empirical chapter.

This leads to the question of how analysis of empirical materials was undertaken. This itself was an embodied, situated, process; the researcher's cognitive capacities and subjectivity, shaped by individual experiences as well as years of academic training, was an important tool in the analytical process just as the researcher's body was important in the process of collecting data. In general, the analytical process was not marked by the systematic coding of empirical materials, although a thematic index of key interviews was produced and played a role in identifying potentially fruitful avenues for 'writing up'. More generally, however, the approach taken was to spend time immersed in the field and in review and analysis of the data while, as seen above, producing 'writing trials' around key emergent themes; some of these 'trials' then provided the core idea for chapters or

subsections of chapters. This drew on some wise advice I was given for overcoming obstacles in the writing process: I was reminded to 'expose' my mind to the problematic and leave it to stew; invariably, and largely unconsciously, ideas would emerge. As Psychologists Dijksterhuis and Meurs comment:

> One could say that unconscious thought is more ''liberal'' than conscious thought and leads to the generation of items or ideas that are less obvious, less accessible and more creative. Upon being confronted with a task that requires a certain degree of creativity, it pays off to delegate the labor of thinking to the unconscious mind (2006: 145).

This process of engagement with empirical materials, has the advantage of a deepened, more creative analysis that brings into dialogue the full cognitive toolkit of a researcher rather than only a thin layer of (conscious) analysis. As Latour indicates, ideas must be allowed to 'come haphazardly'; themes must be allowed to emerge. It should be noted that a more positivist perspective could criticize such an approach as suffering too extensively from the biases and idiosyncrasies of a particular researcher. While the limitations of this 'embodied' approach to analysis must be acknowledged, so must its valuable capacity for original insight. Here, the need for reflexivity, and the awareness of positionality, in the place of a search for an unbiased god's eye view, continues through the research process into analysis.

As noted above, on occasion draft 'writing trials' were shared with participants. This process of inviting participants into the writing process is in contrast to Latour's wariness of 'mixing the meta-language of the actors with that of the analysts' (p. 135). The final text, together with any errors or omissions, is that of the researcher rather than a co-produced text (such as that of Down and Hughes 2009). However, although occurring only in a small way, this openness to opportunities for involving participants in the writing process proved valuable on a number of levels: it created trust and transparency during an ongoing research process; it provided an opportunity to ensure that the final text was as sensitive as possible to any concerns or clarifications that participants raised; and it enabled the researcher to benefit from a deepened understanding which fed back into the overall research process as well as the development of a particular writing trial.

The process of analysis did not stop here, however. As well as bringing ideas into dialogue with the researcher's subjectivity, their cognitive and affective toolkits and limitations, empirical materials were also brought into dialogue with theoretical resources (see Pryke et al. 2003). As one participant insightfully commented, the style of analysis employed in this study differs significantly from their own hypothesis testing approach;

theoretical materials and empirical materials are allowed to collide in a productive fashion. 'Writing trials' therefore become experimental: there is a process of testing out ways of bringing empirical materials and theoretical resources into dialogue.

In discussing the 'embodied' process of analysis, reference is made to the 'cognitive' toolkit of the researcher. As seen above, the researcher suffered a brain injury prior to commencing the research which impacted directly, if subtly, on the researcher's 'processing' of information. This rendered the cognitive dimension of analysis more troublesome than it had been previously and doubtlessly shaped the style and content of the final research output.

CONCLUSION

This chapter explored the methodology adopted in this research project. It explained the rich empirical material generated (Whatmore 2003: 94) through an ethnographic approach. Key influences on the research design were examined. The role of Jackson's adoption of the Heideggerian approach to thought as a 'path towards a clearing' rather than a view from above was discussed and related to Mabry's advocacy of a progressively focused approach to research. Rivoal and Salazar's discussion of 'serendipity' in ethnographic research was described, with their argument that the 'field' shapes the research design as much as the researcher (2013: 180). This was related to a post-structuralist emphasis on the 'agency of human-nonhuman assemblages' (Bennett 2009: xvii) though this was balanced by Barnes's observation that 'the very last thing my data did was self-organise' (Barnes 2014: 203); while opportunities and themes emerge during the collection and analysis of data – requiring a receptive attitude on the part of the researcher - this does not negate the key role of the researcher in driving the research. As Cook et al. comment:

> The ethnographer chooses a base, has certain research questions in mind, finds points in networks of people and places whose comparison promises to shed light on those questions, and then visits some of those points (people, events, and so on) *in a sustained and concerted way*, in the process, of course, revising the questions with which he or she began. (Cook et al. 2009: 58, emphasis mine).

A final influence on the research design was Marcus's discussion of the 'need for ethnography to contextualise itself' (1998: 13). This led onto a discussion of multi-sited (Marcus 1998) and un-sited (Cook et al. 2009) ethnography and a brief discussion of the relationship between ethnography and Euclidean space. It was noted that the holism of ethnographic research objects should not be assumed any more than their boundedness in physical space. As such multi-sited fieldwork is not a means of gaining multiple

perspectives on a single phenomenon. Cook et al. argue, as a result, that ethnography should be detached from (physical, or Euclidean) space with its boundaries drawn conceptually, in relation to research questions rather than physically in relation to research sites. However, it was argued that the assumption of Euclidean space, as a container for social activity, is itself flawed and that the role of physical location in shaping the spaces of geomorphological practice should be investigated through the research not assumed at the outset. This echoed Marcus's comment that 'the contours [of a cultural formation] … cannot be presumed but are themselves a key discovery of ethnographic inquiry' (1998: 117).

Following the discussion of multi-sited and un-sited ethnography, a section entitled 'Making the cut' set out the case studies that formed the foci for this research project. The first case study found its beginnings in following Dombroski's ethnographic adoption of the feminist exhortation to 'start[] where you are' (Dombroski 2011: 21); it began, along the corridor, at the School of Geography and the Environment's Rock Weathering Laboratory in Oxford. Further participant observation was carried out at the British Society for Geomorphology; in addition to informing the research project, this provided an opportunity to recruit participants for a second case study focused on fluvial geomorphology.

The discussion of the case studies, that provided the key foci for the research project, was followed by a discussion of the key methodologies adopted: participant observation, interviews, and analysis of texts. The discussion of participant observation and analysis of texts was relatively brief. However, the approach taken to the conduct of interviews is examined in depth; this reflected the novelty of the approach adopted rather than the relative importance of interviews in comparison with the other methods adopted. The role of 'consultation documents' in gently structuring the interviews was discussed. This was followed first by a discussion of audio recording and then by a discussion of analysis and transcription using Sonocent's *Audio Notetaker* and simple indexing. The value of Sonocent's *Audio Notetaker* in enabling the researcher to move easily between sections of transcribed data and the relevant sections of audio recording was discussed.

A section on research ethics discussed the role of consent and anonymity in the research project. This was followed by a final section examining the positionality of the researcher. The role of the body in ethnographic research was examined and the impact on the evolving research design of conducting research, while the researcher's body is struggling, was explored. The resulting changes to the research design were discussed

in terms of Pertti and Pelto's phrase 'serendipity, disguised as catastrophe' (Pertti and Pelto 1978: 185 cited in Rivoal and Salazar 2013: 180). The chapter concluded with a reflection on the embodied nature of analysis in this research project.

CHAPTER 4: SUPPLEMENTARITY, SYMMETRY AND SPACE IN GEOMORPHOLOGICAL FIELDWORK

INTRODUCTION

Fieldwork, as an object of theorisation, is extensively engaged with in both human and physical geography (Powell 2002). Rose argues that the 'gaze' in geographical approaches to the landscape represents a 'masculinism' (1992: 9) and concomitant 'feminisation of nature' (p. 17) associated with 'geography's "erotics of knowledge"' (1992: 17). Powell (2002) argues for a consideration of fieldwork across physical and human geography as a basis for 'conversation within the discipline over joint conceptualisations' (2002: 267). He also highlights Phillips's recognition of the role of 'experience, intuition, tacit knowledge, and other deeply individuated ways of knowledge' within geomorphological fieldwork. Baker argues for an alternative epistemology of fieldwork: he describes it in terms of a 'semiotics' of 'inference laden signifiers' enabling geoscientists to 'converse[ ] with the earth' (Baker 2000: 2) in an enterprise of 'earth-' (as opposed to 'theory-') 'directed science' (Baker 2000: 3).

This chapter begins with a consideration of fieldwork undertaken on foot and employing the senses to engage with a field site. The first section examines the dichotomy between the analytical gaze, associated in Latour's 'Circulating Reference' with abstract representations of the landscape, and other, kinaesthetic, and haptic senses, associated in Latour with (field-based) hesitant, exploratory modes of investigation. It seeks to challenge this dichotomy, noting the presence of measuring tools and 'abstract' data sources during embodied exploration. Lund's concept of a 'touching eye' is employed to argue that a playful, exploratory and multi-sensory mode may be utilized by geomorphologists investigating so called 'abstract' data sources. Section one concludes with a consideration of the Derridean concept of 'supplement' – this is employed to examine how fieldwork is always already augmented by scientific techniques. Section two builds on this analysis to examine the concept of space associated with fieldwork. It examines how fieldwork is always already shaped, and situated, by a range of dimensions: physical, social, disciplinary, comparative, informational and temporal. The chapter concludes with a critical engagement with Latour's concept of 'Circulating Reference' (1999a). It argues that the empirical materials examined in this chapter provide evidence of a need to complicate reference, ensuring that the symmetry of 'nature' and 'culture' is adequately reflected in the analysis of fieldwork.

> *Immigrant rocks*: the rocks of Skiddaw are immigrant rocks, just passing through here, like my sister and me only rather more slowly, and changing all the while. Places as heterogeneous associations. If we can't go 'back' home, in the sense that it will have moved on from where we left it, then no more, and in the same sense, can we, on a weekend in the country, go back to nature. It too is moving on … In the end there is no ground, in the sense of a stable position, and to assume there is (sic) to fall into those imaginations [that] celebr[ate] a mobile culture while holding (or trying to hold) nature still (Massey 2005: 137, emphasis original).
>
> The possibility that landscape might be understood in terms of *event* remains unarticulated by ANT (Wylie 2006: 522, emphasis original).

Echoing Latour's emphasis on the field as 'origin' (1999a: 56), three interviewees, each at different stages of their careers, emphasized the primary role of fieldwork; this was seen to be related closely to their identity as geomorphologists (Powell 2008a: 550):

> The absolutely crucial thing for me is going out into the field and looking – and looking properly … It's me reading the landscape and thinking - when I see something – and thinking … well how did it get there? And it's the sort of geomorphological tradition really. You know it's what they used to do a hundred years ago. (Aidan: senior fluvial geomorphologist interviewed February 2015).
>
> Because I've always been catchment focused I don't think I've been a good kind of reductionist … I've always needed to know my field site … it's not just fully about being a deductive scientist – it's because you have the inductive experiences that lead towards the research hypothesis that you then try to tackle in a deductive way … You've got to have that idea and generate that idea- and I get my ideas often walking around and you know seeing something and going huh why's that working like that – and you know I side with that perspective. You can crunch as many numbers as you want but you need to go out there and look at the site and get some sense of why things are happening (Finn: mid-career fluvial geomorphologist interviewed October 2014).
>
> When I was doing my sort of training – first starting out – it kind of goes back to – I did a Masters – the thing I kind of took to heart is that to understand any system the first thing you have to do is to go there and if you're able to kind of either take field photographs or kind of sketch or stuff like that so I'd be kind of – because my answer's quite kind of old fashioned – the first thing would be that site specific knowledge of walking up and down … I don't think I could do anything unless I'd been to a site and just wandered around and kind of had a look at how things fit together because in terms of starting to formulate research questions for me it almost all comes erm from sort of walking around and looking at things and that's where the kind of germs of ideas – I wonder how that works … It is something that is more maybe a dominant kind of thinking in geomorphology so some of … even the bigger projects I've been involved in … it tends to be these initial site reckies built into the thing often before grants are applied for and so people go out (Kumara: early career fluvial geomorphologist interviewed August 2015).

This emphasis on fieldwork was also found in the geomorphological literature. For example, in a discussion of gender in physical geography, co-authored by a geomorphologist, Bracken and Mawdsley comment that:

> At the outset, we should state that we both believe in the value of field-based research in both human and physical geography, and the importance of grounded empirical investigation in the development of rigorous ideas and arguments (2004: 281).

In geomorphology, the importance attributed to fieldwork may be related to Deleuze and Guattari's discussion of 'implacement' in disciplines, including hydrology[16], that they term 'nomad':

> Only in the imperialist perspective of a royal science such as Newtonian physics does implacement supposedly become a matter of indifference: the law of gravity is presumed to be universal and to operate between any two bodies found anywhere in the physical universe. Gravitational forces are schematized in parallel laminar lines that are determinable metrically. But in the very different perspective of a nomad science such as the hydraulics of flood control … the role of place is pervasive and not to be ignored. Here the material forces move not in perfectly straight lines in a grid like space but in spiral and cortical motions in concrete places, for example in the coursing of floodwater … How water moves is a direct reflection of *where it is* … If the geometry of gravitation is still Euclidean … that of hydraulic motion is vectorial, projective, and most especially topological – thus a function of the place it is in (Casey 1998: 302-303, emphasis original).

This chapter seeks to reconsider what this 'implacement' means, examining how geomorphological fieldwork is situated amid 'heterogeneous associations' (Massey 2005: 137) that always already include the 'supplement' (Derrida 2001: 366) of scientific understanding. The chapter is primarily built around a sustained engagement with the three interviews cited above although additional material is incorporated where relevant. These interviews were conducted with three fluvial geomorphologists, Aidan, Finn and Kumara,[17] who, at the time of interview, were at different stages of their careers: senior, mid-career and early-career scientists. Each had, at different times, undertaken formative work in geomorphology at the same UK institution, though they were now based at different institutions. They were also connected through having either studied the same field site or having worked under the guidance, either as PhD students or as early career scientists, of the same geoscientist.

[16] A discipline closely related to fluvial geomorphology.
[17] Names of the geomorphologists and place names associated with their research have been changed to maintain anonymity.

SECTION ONE: EXPLORING AND ANALYSING THROUGH MOVEMENT AND VISION

> Through walking … landscapes are woven into life, and lives are woven into landscape, in a process that is continuous and never-ending (Ingold 2011: 47).

> What happens to the original, embodied encounter with space, the walking, pacing, the use of hands and measuring tools? (Paterson 2007: 60)

CIRCULATING REFERENCE: HESITANT WALKING AND A DOMINATING GAZE

In *Circulating Reference,* Latour juxtaposes two pictures, presented with images and texts (1999a: 24-29). In the first a group of scientists, including a geomorphologist, are in the field to study treeline advance in the Amazonian forest. In the second, the same scientists take a break from this fieldwork and gather around two maps. Latour states that:

> In the [first] picture our friends were immersed in a world in which distinct features could be discerned only if pointed out with a finger. Our friends fumbled. They hesitated. But in [the second] picture they are sure of themselves. Why? Because they can point with their fingers to phenomena taken in by the eye and susceptible to the know-how of their age-old disciplines: trigonometry, cartography, geography (1999a: 29).

Latour emphasises the 'novelty' of the second picture, commenting: 'here are four scientists whose gaze is able to dominate two maps of the very landscape that surrounds them'. In Latour's view this ability rests on the 'two-dimensional' (p.29) nature of these representations, as opposed to the three-dimensional space of the field. As seen in chapter one, there are parallels here with Cuvier's opposition (1807 cited in Outram 1996: 259) between 'the field naturalist' and 'the sedentary naturalist'.

These contrasting images are echoed by Paterson who describes how 'a forgetting of touch and the bodily senses … forges a set of idealized, abstracted visual representations' (2007: 60). This is examined in terms of an opposition, challenged in other parts of Paterson's text (2007: 74), between the 'detached eye' and the 'embodied hand' (2007: 9); Paterson comments that 'detachment is of the eye, we might say, whereas the hands and feet draw us into the world' (2007: 76)[18]. Similarly, the two

[18] This might also be considered in terms of an opposition, highlighted by anthropologist Michael Jackson, between exploratory wandering along Heideggerian 'paths towards a clearing' and an analytical view from above (Jackson 1989).

pictures described by Latour bear resemblance to the smooth and striated spaces articulated by Deleuze and Guattari. Smooth space 'is never a matter of point of view or distance in any metric sense - even landmarks become proximal presences – it must be experienced by actions at "close range", for example, by "legwork", by walking, hearing, and more generally by various haptic modalities' (Casey 1998: 306-307). By contrast in striated space 'even the "immersion in an ambient mileau" that we have seen to be indispensable to smooth space is reduced to a set of positions on a grid or map' (Casey 1998: 308). However, anticipating the argument of this chapter, it is important to note that these two forms of space are not polarised by a uni-linear set of transformations. Instead Deleuze and Guattari comment that:

> Smooth space is constantly being translated, transversed into a striated space; striated space is constantly being reversed, returned to a smooth space (Deleuze and Guattari 1987: 474 cited in Casey 1998: 308).

Deleuze and Guattari's discussion of smooth and striated space is returned to towards the end of this chapter. First, however, it is necessary to examine more carefully the practices of geomorphological fieldwork.

WALKING THROUGH THE LANDSCAPE

Returning to the geomorphologists' comments cited above, two remarks are particularly salient:

> I get my ideas often walking around and you know seeing something and going huh why's that working like that (Finn: mid-career fluvial geomorphologist interviewed October 2014).

> The first thing would be that site-specific knowledge of walking up and down … I don't think I could do anything unless I'd been to a site and just wandered around and kind of had a look at how things fit together because in terms of starting to formulate research questions for me it almost all comes erm from sort of walking around and looking at things and that's where the kind of germs of ideas – I wonder how that works (Kumara: early career fluvial geomorphologist interviewed August 2015).

There is a rich vein of recent phenomenological (Matos 2005), 'post-phenomenological', (Wylie 2005: 245), and non-representational (Merriman et al. 2008) work exploring movement, place and landscape. It is worth also noting Ingold's thoughtful genealogy of the 'booted European foot' and the 'striding gait' (2011: 48). Wylie discusses the 'spectral' dimension of walking (2005: 237), describing landscape 'in terms of the entwined materialities and sensibilities *with which* we act and sense' (p.245). Paterson emphasises

'kinaesthesia' as 'a sense of movement that utilizes a range of nerve information including that of muscular tension and balance' while Matos comments that 'while *walking* one intimately senses and thinks *place.* One perceives and develops a feeling for places' (2005: 6, emphasis original). This literature draws attention to the haptic aspect of fieldwork. However, it is important to note that, even in analyses that oppose 'embodied hand' and 'detached eye' (Paterson 2007: 9), the field scientist is rarely *empty*-handed.

WALKING THROUGH THE LANDSCAPE WITH MEASURES AND MAPS

> What happens to the original, embodied encounter with space, the walking, pacing, the use of the hands *and of measuring tools*? (Paterson 2007: 60, emphasis mine).
>
> Our own body with its organs is the instrument we carry about in space. Now it is the hand, now the leg that serves for a compass, while the eye turning in all directions is our theodolite for measuring arcs and angles in the visual field (Helmholtz 1971: 259 cited in Paterson 2007: 72).

Referencing the work of Merleau-Ponty, Paterson writes: 'the creation of contexts or situational knowledges … is a dynamic, kinaesthetic framework that includes the motile body *but also measuring equipment* (e.g. Merleau-Ponty 1992: 143), even if the equipment fosters two-dimensional inscriptions, as Latour outlined' (2007: 74, emphasis mine). The mid-career fluvial geomorphologist cited above gave an interesting response to a question about the 'kit bag' taken into the field. Amid a list of items they commented:

> If you want an amusing answer to that question – which is a little facetious – but a wooden ruler … of thirty centimetre length and one meter length – would be a kind of go to equipment really (Finn: mid-career fluvial geomorphologist interviewed October 2014).

Underlying the proximity of measure and embodiment they explained:

> Plastic rulers are not much use in the field – they break very easily … so if I ever come across wooden rulers – the same with … meter staffs because you can use a meter staff as a third leg in a river – you can use it as a walking stick (Finn: mid-career fluvial geomorphologist interviewed October 2014).

This comment provides the beginnings of 'a way beyond the distinction between transcendent, geometric, or abstracted space on the one hand, and immanent, situational or phenomenal space on the other' (Paterson 2007: 74). Here the kinaesthetic interaction of the geomorphologist with the field environment includes measuring equipment, the latter enabling their exploration and acting as a 'third leg' enabling the embodied movement of the scientist.

WALKING THROUGH THE LANDSCAPE *WITH* MAPS, PHOTOS AND REMOTELY SENSED DATA

Another item in the geomorphologist's kitbag is 'a large scale aerial photo' described as 'always an undervalued piece of field kit' (Interview with mid-career fluvial geomorphologist). While Latour opposes the gazes 'able to dominate two maps of the very landscape that surrounds them' (1999a: 29) to a 'fumbl[ing]', 'hesitat[ing]' walk through the landscape, an alternative perspective refuses this opposition between eyes and feet. This is hinted at in Lorimer and Lund's discussion of a hill-walker's use of maps:

> Of specific interest are the walker's attempts to *see* and *feel* the route as it emerges through the map before any embodied action occurs *in* the landscape … While the novice might look at the map from a position on high, and only see a confusion of lines, markings and numbers, the more experienced walker learns to use the map as a way of looking *into* the landscape, and seeing how the topography rises and falls. By steadily examining the route it is possible to visualize landscape and speculate about the types of terrain likely to be encountered … It is important to recognize this more complexly layered process of cartographic visualization, when the map becomes a medium between the anticipating body and the still distant landscape (Lorimer and Lund 2003: 137, emphasis original).

Echoing this account, the early career researcher responded to a question about how they prepare for a first visit to a field site:

> The other thing I personally would probably do is … sort of play around with some remote sense datasets because they tend to be fairly freely available – my kind of skillset lets me kind of just mess around with them – play around with them fairly easily – I don't have to think about it too much so just kind of things like that – spending a few hours looking at er say google earth or downloading land sat imagery … The project I am starting in Kiliw - there is a lot of … groundwater components to it - kind of questions to it - so before I went out there I was downloading some data from a satellite called Grace that maps the amount of groundwater in fairly coarse grids so people tend to use it for things like sort of drought monitoring and ice in the polar icecaps – things like that – kind of quite – er – sort of coarse scale assessments – but I just wanted to look at how the groundwater varied over the year and it was just something I could do in like you know a sort of half hour or so – just download it and mess around with it in a geographical information system – a GIS package – So yeah – things like that erm they are … *again like walking around the field in a way* – it's like exploring the site remotely and it's it not – I wouldn't be at that stage really – trying to get anything kind of concrete process data … it's just kind of playing around – just trying to develop a feel for … what sort of things … could be investigated – what sort of things might be interesting to look at (Kumara: early career fluvial geomorphologist interviewed August 2015, emphasis mine).

To understand the 'touching eye' at work, even as a geomorphologist works with maps, aerial photographs, remotely sensed data, or models, it is helpful to consider Lund's description of mountain walkers pausing at the top of a mountain. Lund argues that

although ‘sensational engagement may alter as the bodily postures change’, ‘the “lived body” continues to move in the sitting position rather than exclude haptic forms of sensation’ (Lund 2005: 33). This focuses attention on a ‘touching eye’ and on a body that, while sedentary, engages with a field site in a manner that does not ‘exclude haptic forms of sensation’ (p. 33). As Clark remarks ‘new understandings’ are emerging ‘of the way that viscera, skin, posture and gesture are all implicated in the processing of information’ (2003: 43). This image of a pausing mountain walker provides the beginnings of an alternative way to consider how sedentary geomorphologists may at times interact with maps, photos, and remotely sensed data in a playful and exploratory manner, while at other moments, including during fieldwork, they may adopt the abstracting, analytical approach more conventionally associated with the gaze. In this way, interaction with apparently abstract entities may not be devoid of the ‘hesitan[cy]’ and ‘fumbl[ing]’ Latour describes witnessing in the field (Latour 1999a: 29) and fieldwork may not be devoid of abstraction.

Another example of the way apparently distant ‘representations’ may come alongside, and enter into interaction with fieldwork, is seen in the comment of the senior geomorphologist, who self-identified as being primarily fieldwork-orientated; they noted an increasingly two-way dialogue between fieldwork and modelling:

> I've found it's been really productive working with certain modellers both in - both the physical modellers in the lab - the flume type people - and also the people who construct the numerical models - I've found it’s been really productive … working with them … and I think that sort of work … has really developed in the last say ten to fifteen years … before that - people tended to go their separate ways and … so if you were a numerical modeller - ok you would take some data from somebody's paper - or they would give it to you … to enable you to develop your model - but it wasn't really a two way relationship like it is now … so I think things have really moved on in the last fifteen years probably … the people I’ve worked with best … yes we tend to - they'll come out in the field … and then I'll - you know we'll go out in the field and we'll look at everything - and they'll say what their model does and I'll point out things I see - and then from that you know we can move forwards - and sometimes you know they'll try to model things slightly differently … once they've heard my view about how certain features form … so yeah it's you've got to have the right people … and if you get the right combination of people you can go a long way (Aidan: senior fluvial geomorphologist interviewed February 2015).

It is important to note that the model is unlikely to be specific to one field site in its evolution. The mid-career geomorphologist commented that:

> There won't be an infinite number of models - you aren't just going to walk off and develop one easily and those that pre-exist it'll be far more cost-effective - within the realms of a set PhD - which is where most of the advances happen in the

> world - it'll be more within the realms of a particularly PhD to sort of tweak a pre-existing model than start from scratch … you are not easily going to build a catchment model with the level of complexity that that involves within a PhD and then test it to do something with it smart that your PhD is supposed to be about within the realms of your 3 years - you know it makes much more sense that you maybe take that model and maybe tweak it a bit - erm - you know particularly as it's not a commercial entity - it's there for using - the code's available -there are forums and blogs that talk about it and so on … so I suspect you'll see that you know the two or three catchment models that exist around the place used for quite a long time and I mean maybe they'll eventually be almost unrecognisable from the original model but they'll become the stepping stone – the kind of coding - there'll be a series of lines in there presumably which stand the test of time and become the basis for people doing you know a lot of different tests you know from those models … so getting back to this idea of kind of taking the model or creating something new - I suspect ... as we get more detailed understanding and require models of a greater sophistication then you need to take pre-existing models and try and make them work for your purpose either by adjusting them or just getting smart about which parameters to – you know dials to turn on the model (Finn: mid-career fluvial geomorphologist interviewed October 2014).

This success in bringing fieldwork into dialogue with numerical modelling is indicative of a symmetrical ontology whereby the texts (e.g. coding, images) of a model may be brought alongside a landscape, producing an augmented, informed (Barry 2005) space of fieldwork. This point echoes Hinchliffe et al.'s (2005) observation on the role of field guides in ecological fieldwork. Hinchliffe et al. describe the use of field guides in looking for the 'traces' of water voles, or what they term the 'water vole writing' (p. 647). They comment:

> Learning water vole writing involves rapid movements between texts, descriptions, field signs, conversations, comparisons, finding similarities, explaining differences, and so on … The role of texts is important to grasp here. The field guides as textual inscriptions of footprints and other characteristics were not equivalent to footprints in the field. But nor were they either rich or poor depictions. They were, instead, sensitising devices, diagrams, that made water voles more rather than less real for those who started to use them (Hinchliffe et al. 2005: 648).

This interweaving of textual resources, including models, and non-humans in the production of the spaces of fieldwork points to an alternative conception that does not oppose three-dimensional movement to a gaze over two dimensional representations.

SUMMARY: SUPPLEMENTARITY

This section has found that exploring the field on foot is an important, and for some geomorphologists, crucial aspect of doing geomorphology. However, it has challenged the dichotomy, inherent in Latour's *Circulating Reference,* between embodied exploration in the three dimensions of the field site and an analytical gaze over the two dimensions of

maps and other data forms. This links to the opposition, explored in chapter one, between 'indoor' and 'outdoor' science. It allows a consideration of the way that:

> The field … is not just 'there': it is produced and reproduced through both physical movement across a landscape *and other sorts of cultural work in a variety of sites* (Driver 2000: 267).

What happens then, to the concept of 'place' and to the (territorial) specificity of a field site? Might this be reconceptualised as a hybrid space without losing, but rather enriching, the crucial sense of emplacement?

The Derridean concept of the supplement[19] is helpful here. Derrida argues that the signifier supplements the signified – it is both an 'addition' to, and 'supplements a lack on the part of the signified' (2001: 366). Drawing on this account, it is possible to conceive of the 'supplement' of the scientific (the signifying) as representing an addition to the landscape (the signified), but crucially also as indicating a lack therein. This echoes the way that, elsewhere, Derrida plays with Rousseau's use of the term *supplement* in the latter's discussion of culture as supplementary to nature. While one meaning of the term supplement 'suggests that the second term is inessential, merely adding to the first term, which is primary, full, self-sufficient', a second meaning, surfaced by Derrida, sees the supplementary term 'mak[ing] up for deficiencies in what was supposed to be the perfect and complete entity to which they are in an ancillary relationship' (Attridge and Baldwin 2004).

Concurring with Massey's conception of 'places not as points or areas on maps, but as integrations of space and time; as *spatio-temporal events'* (Massey 2005: 130, emphasis original), supplementarity indicates that the 'spatio-temporal events' of fieldwork are understood as 'hybrid' (Whatmore 2002); they are therefore characterised by the ontological symmetry that Latour argues for in *We Have Never Been Modern* (1993) but inadequately articulates in *Circulating Reference* (1999a). To signal this approach, the term 'fieldwork' is opposed to 'field site' throughout this chapter, with the physical location of a field site understood as just one dimension among the many that shape and define the spaces of fieldwork.

[19] Note that Gail Davies also employs the Derridean concept of supplement in her rich analysis of post genomic research (2013).

SECTION TWO: SITUATING FIELDWORK IN SPACE

> It would be a mistake to think that the field is simply a site that just 'is there' and can be taken for granted (Livingstone 2003: 47).
>
> … the field is constituted as such by academic projects and the activities of scientific investigators, including the stories scientists tell about it (Jellis 2008: 1)

This section argues that the spaces of fieldwork are always already even richer than the meshwork of non-human processes that geomorphologists attend to. It begins with a consideration of the location of fieldwork, relative to the academy, in physical space. It then proceeds to complicate this analysis, layering differing dimensions that shape the spaces of fieldwork.

FIELDWORK AND PHYSICAL PROXIMITY OR DISTANCE

Geomorphological field sites are often to be found at locations that are remote from the home universities of geomorphologists. However, another species of field exists: the field site that is close to home. Bracken and Mawdsley comment that:

> We can also question the extent to which the dominant image of expeditionary fieldwork (involving long trips of several weeks, even months, in distant places) represents the norm. Many physical geographers choose to work on sites which are relatively close by, utilize automated data collection methods, and may only need a day's visit every couple of weeks to change equipment, download data or survey location change. For process-driven investigation, frequent visits to nearby places make a great deal of sense, in terms of being able to make more visits over a longer time period, and allowing funds to be directed towards equipment and other requirements rather than travel (Bracken and Mawdsley 2004: 283).

The mid-career geomorphologist explained that, following their return to the UK after several years based overseas, they wanted to select a field site close to their home university:

> I still do stuff outside Chayde but here – I want something close by – so I thought well what are people not doing … erm there are very few classic alluvial rivers around here – fully alluvial – we've got alluvial rivers that flow off Clearacre and they're all important salmon fisheries and things like that and I started thinking about ok I want to - that's gonna be what I can bring back over from Chayde (Finn: mid-career fluvial geomorphologist interviewed October 2014).

Similarly, seen in chapter seven, the senior geomorphologist was appointed to a lectureship early in their career and they selected a field site within easy reach of their university, commenting:

> It was sheer luck that when I was appointed to Edgewick I needed to set up a catchment so I could take students out and say look this is a flow gauging station, this is a rain gauge and so on – and then it was sheer luck that I chose the

> Faircrest to do it in'. (Aidan: senior fluvial geomorphologist interviewed February 2015).

The site this geomorphologist selected was important, as seen in chapter seven, since at this site the interaction of fluvial and ecological factors was unaffected by prevalent river management practices; this enabled observations that proved decisive in the development of fluvial biogeomorphology.

In these examples, the specificity of a particular field site may be understood as a function of the location of the site along a dimension of physical proximity or distance. As seen in more detail in chapter seven, this type of located-ness is important to the evolution of the discipline of geomorphology since 'the random, the accidental, that which befalls an individual entity, becomes an essential ingredient in the history and development of that entity and all that follows it' (Grosz 1999: 5). The chance selection of a field site, for reasons of its proximity to a given university, proved decisive in the evolution of a given area of research; without this, its potential role in shaping the discipline, and its research objects, may have remained unrealised. It is therefore possible to answer in the affirmative to a question articulated by Livingstone: 'Is any significance to be attached to the locations chosen as sites for scientific pursuits' (2003: 20).

Having noted the role of physical distance or proximity, it is of value to consider other forms of spatial proximity or distance that position geomorphological fieldwork.

SOCIAL PROXIMITY

Another form of relation or spatial positioning may be provisionally described as 'social': field work may be understood as situated along a social vector of proximity or distance. For example, as seen in chapter seven, the senior geomorphologist mentioned above explained that later in their career their focus had shifted from the field site described above to a new field site:

> Then what happened was … as a result of another, you know, series of connections with people – I ended up looking at this river in Ulelicia which I do work on a lot now called the Ananes (Aidan: senior fluvial geomorphologist interviewed February 2015).

> Work on the Ananes was promoted by a very eminent ecologist who was working in Tresti … and [they] came from the US to Europe and wanted a naturally functioning river to work on – and [their] postdoc – who's now probably … one of the most eminent [ecologists] in the world … [they] found the Ananes and because we all knew one another we all ended up in a workshop on the Ananes - wandering around and saying 'Cor – this is a good place to do all of these things'. So it actually was that work started by an ecologist – even though I was involved

> and another geomorphologist – but I'd already been doing ecologically related things – without realising it … It's just sheer chance – you have to be with the right people (Aidan: senior fluvial geomorphologist interviewed February 2015).

These excerpts from the interview with Aidan indicate how fieldwork is also located along (and relationally constituted by) a vector of social proximity and distance; that is to say within academic networks of people. Although this fieldwork was more remote in physical terms, it was proximate in terms of academic networks of people. The specificity of this factor was emphasised. Aidan continued:

> So on the Ananes, well, there were three ecologists … all of whom … were quite unusual for being able to think big as well as small … so it was having those three people together who were ecologists … who were interested in the bigger scale, along with myself and someone called Seumas... so we were two geomorphologists who just happened to be interested again in quite … large-scale fluvial geomorphology. And that little group together we were able to go into the field and actually talk in a way where we could communicate with one another (Aidan: senior fluvial geomorphologist interviewed February 2015).

Again the particularity of the spatial location, along a social dimension, is important, as is the emphasis on chance evoked in these comments.

The early career researcher remarked on the value of time alongside other researchers in the field. They commented:

> I think if … at all possible it's always good to go [to the field] with people who have done research in the landscape before because often what makes it into the academic papers and textbooks is not always the whole story about particular landscapes – it's often about a particular facet of a kind of landscape and so going you know walking around a particular site with someone who's done research – I suppose even you'd say it's someone who's just familiar with the landscape – a kind of local person whose kind of like landscape manager or someone like a gamekeeper – someone like that who has that kind of innate familiarity with the landscape – you get a lot of interesting information and interesting context talking to people like that (Kumara: early career fluvial geomorphologist interviewed August 2015).

Companions in the field can open out new possibilities for fieldwork– expanding the range of interactions between a researcher and an environment. This analysis leads to an understanding of how (i) field work is located in spaces defined along social as well as physical dimensions of distance and proximity and (ii) interpersonal companionship in the field that can open out the range of interactions between a researcher and a field site, extending the spaces of fieldwork and deepening a researcher's apprehension of the way a system functions.

DISCIPLINARY PROXIMITY

Another vector of proximity and distance, indicated in the interview with the senior geomorphologist cited above, is that of disciplinary distance or proximity and the ability to communicate among particular individuals trained in different disciplines. This factor was also emphasized in an interview with an ecologist who had discovered, during studies of treeline advance[20], the importance of geomorphological factors. The ecologist described how spending time in the field with people from other backgrounds enabled a different experience:

> My colleague there gave me the opportunity of just getting lost [in the field] and walk[ing] around to get an idea – that really helped me a lot to escape the classical biological point of view – definitely no doubt – because very [few] people I was working with had purely biological background so I definitely changed my point of view – through interaction with all these people – and it's been probably the most enriching thing I've done in my research life so far – which is erm – learning to see the world differently than I had been initially trained to do. (Sascha: mid-career ecologist interviewed October 2014).

This excerpt recalls Hamilton et al.'s discussion of different disciplines coming together in the field (2008). However, it shows a process of change with a different experience of the field occurring as a researcher allows a combination of other people's perceptions and time spent 'getting lost' in an environment to shape their understanding and their evolving researcher-self.

A similar episode was described by Aidan, the now senior geomorphologist, describing a shift of identity from being a theoretical hydrologist to becoming a geomorphologist for whom field work is of primary importance. This occurred when a senior geomorphologist in the same department left and a number of projects that had been a focus for collaboration required completion; the researcher commented:

> When Sean left I was left with a whole load of projects – which were geomorphology projects – which I could either say 'well they're just finished' or I could take them on – and I looked at it as an opportunity – so I became a geomorphologist overnight ...That's when I started working with Ranna because I did all this geomorphology but I wasn't a geomorphologist – and I'd been out and looked at things and I thought you know this is interesting – that's interesting – and I thought I'd better get somebody with some street cred in geomorphology <laughs> to come out and look at what I'd found out – to say yeah that's a sensible idea or it isn't … that's when we started working together because she said oh this is really interesting (Aidan: senior fluvial geomorphologist interviewed February 2015).

[20] Coincidentally, this is an example of interdisciplinary work examining treeline advance; a similar study, in a different region of the world, was the subject of Latour's *Circulating Reference* (1999).

While Donaldson et al. describe research objects that are 'more interdisciplinary' than the researchers themselves (2010: 1522), these interview excerpts evoke the importance of a strong interpersonal rapport among individuals, from different disciplines, working alongside one another in the field.

COMPARATIVE DISPLACEMENTS: THE PRESENT ABSENCE OF OTHER FIELD SITES

Another element in geomorphological field work is the presence and absence of other field sites. Livingstone comments that 'to fieldworkers it was presence ... that underwrote their claims to authority' (2003: 41); however, it is valuable to attend to the interplay, or in 'freeplay' (Derrida 2001: 369) of presence and absence inherent in fieldwork. This enables a suspicion towards any conception that the field site as 'origin' or 'signified' is characterised by what Derrida calls 'full presence, the reassuring foundation, the origin and the end of play' (p. 370). The senior geomorphologist commented that:

> I suddenly go to this big braided river in Ulelicia … and again – you know – the way you observe it is based on what you know from the UK so immediately I could look at that river and I could come up with a set of hypotheses which if I'd just been local and only ever looked at that river – I wouldn't have thought of – so it is very important to have these core sites – even if you then move as I have done from the Faircrest to the Ananes … because that just gives you a way of … looking at other rivers. (Aidan: senior fluvial geomorphologist interviewed February 2015).

This presence-absence also recalls Law and Singleton's discussion of 'fire objects' (2005: 341). Law and Singleton argue that 'an object is a pattern of presences and absences' (p. 343). They comment:

> An object is a presence. It is present, here and now. But whatever the form of its presence, this also implies a set of absences. The present object implies realities that are *necessarily* absent, that *cannot* be brought to presence; that are othered (2005: 342-343).

In a similar way, the spaces of fieldwork are characterised by presence and absence. The role of other field sites in the ontology of fieldwork is also emphasised by the mid-career researcher mentioned above. They commented that:

> There's something about a – I think – a visual imprinting on your brain as well as about field sites – the more … these days invariably when I go to a new field site pictures in my mind spring up of the closest similar environments in which I've worked and I start thinking about what is transferable information from those environments and what is different because ok the physical laws are still the same – but then there's that environmental space - hydro-climatic terrain geology type

> space … and the history – environmental history – what's the same – what's different – and of course that's handy because compare and contrast is a fundamental way that we do a lot of science – so you know – I'm always thinking about that (Finn: mid-career fluvial geomorphologist interviewed October 2014).

This presence-absence of other field sites, as a dimension shaping the space of field work, is again of key importance: the existing field work experiences of a researcher shape the space in which future field work occurs. It should be noted that such experiences also shape the researcher; as Livingstone argues, the 'arenas' of scientific inquiry 'are active in producing the kinds of subjects humans are in those spaces' (2003: 86).

INFORMED FIELDWORK: FIELD SITES, TEXTS AND REMOTELY SENSED DATA

While chapter six will consider the role played by texts as actors enabling the safe displacement of techniques, here texts are understood as a dimension shaping the spaces of fieldwork. Rather than polarising field (as 'origin') and text (as 'destination') (Latour 1999a: 56), fieldwork is seen as always already *informed* (Barry 2005). The process of engaging with texts prior to and following visits to a field site provides a researcher with an additional opportunity for engaging with the objects of a study; the spaces of fieldwork are thereby opened out. For example, asked what preparatory work would be conducted prior to field work, Kumara commented:

> I think I'd definitely try and get … research that has been published on that site or similar sites – do kind of background reading to familiarise myself with the type of system it is and then try to see what other research has been done – personally I don't try and do that to an exhaustive level – I just try to give myself a general feel – erm – for what's going on (Kumara: early career fluvial geomorphologist interviewed August 2015).

As seen in section one, the informational dimension of the space in which fieldwork occurs is also expanded through engagement with remotely sensed data. Again this functions to expand the space for engaging with objects of research, enabling a researcher to apprehend aspects of a landscape that are otherwise unavailable in a field visit. This may include processes that are difficult to engage with as they occur out of sight, on a different timescale, or on a different physical scale. The early career researcher commented on the way remotely sensed data enabled them to 'extend' their knowledge of a site in a way not possible during a field visit:

> Yeah I think it's … all about just really trying to understand – understand the system – understand the different sort of contexts that individual sort of sites exist in – erm – so I would tend to use remote sense data and GIS to really I suppose

> extend the sort of spatial knowledge of a particular site because it's easier to see things in much larger scales than it is on the ground – so that's typically what I would be using it for – just to sort of see how things sort of fit into a wider context – which isn't possible on the ground – rather than trying to do the same thing and be – it's the same kind of idea I guess – sort of – mentally wandering around the landscape if you like through satellite data. (Kumara: early career fluvial geomorphologist interviewed August 2015)

This comment affirms Byrne's insight that: 'data manipulation and image generating capacities of computer based technologies of investigation can be understood, at least in part, as extensions of our ability to perceive the complex world' (2002: 163)

The researcher was asked whether they would visualise the remotely sensed data whilst in the field and they responded that:

> Erm – that would depend on the site – I think so in terms of – erm – I would be sort of visualising particularly in the large scale – kind of visualising what's sort of over the horizon because I work a lot in kind of forests and trees – the horizon can often be quite close – you can maybe only see a few tens of metres – erm – so – yeah – it would be helping me to understand that which direction the mountains or high grounds in which direction if it's a river basin – which kind of direction the sort of flow is even – if I am not near a river – it's how the particular place I am in – how that fits into the kind of drain – the wider drainage basin – and kind of understanding context so I would be using that kind of remote sense data in a kind of abstract way so I would go 'well that way's north – so that's where the higher ground is and it's draining down to the right' or whatever. It would be kind of walking around – kind of making those sort of wider connections which sort of helps you understand what you are looking at when you are down on the ground-particularly when you don't have very long kind of lines of sight (Kumara: early career fluvial geomorphologist interviewed August 2015)

These extracts from interviews show the way remotely sensed data can enable a broadened dimension of fieldwork through 'play' and 'visualisation' and 'mentally wandering around the landscape'. Again, as seen in section one, this implies that an analytical approach is not always employed to remotely sensed data; at times a 'wandering' exploration may be the primary mode of engagement.

TEMPORAL STRUCTURING OF THE SPACES OF FIELDWORK

Project timeframes represent another dimension that shapes the spaces of fieldwork. Finn commented that it is becoming difficult, within the timeframes of a given research project, to undertake work on a field site that has never been studied:

> I think it's kind of intriguing whether you can have people just walk out into the field now - start from scratch in an unknown area and kind of come up with something that's kind of research worthy at the end of their topic - I mean it ought to be as long as they're transferring over hypotheses that are sufficiently well advanced but if you're going to need a lot of data to kind of get to that point then you're starting

> to really constrain what you can do - I think that's part of that big science big data change that we're seeing and we're going to see people using pre-existing data sets more I suspect as the basis for at least the early phase of investigations - and maybe that doesn't hold within some of the quaternary world so much where you can actually have a kind of more kind of exploratory type of PhD but erm I suspect in some of the process stuff it's going to become increasingly difficult to just develop something that's sufficient in sophistication soon - not now probably but in ten years' time - if you don't base it off a pre-existing dataset just to get yourself up and running (Finn: mid-career fluvial geomorphologist interviewed October 2014).

The same interviewee commented on the way they had cultivated a nested body of research associated with a particular field site, with each project containing the beginnings of further projects:

> The first Masters student I had on the Clearwater… I got her to look at the catchment scale - at limitations of the salmonoid environment up and down the Clearwater knowing full well that it would related to sediment and I told her you're doing the first thesis on this and it's my expectation then people will sort of tier off your thesis in future years using the background knowledge you've got - you've identified the critical locations or whatever … and will do more detailed work thereafter - and that's actually happening now - the MRes student who's just finished - has actually taken - she's had long enough in it to actually start at a catchment scale - and then go off in a number of other directions – well that's because she's had longer and I'm hoping that once I read her thesis I'll have … a suite of ideas for this year's masters cohort of geomorphologists - so yeah … and we just had a PhD student come in now and there are a couple of things happening on the Clearwater and all this pre-existing data which is now being generated by passive sensors in bedload monitoring and them doing some tagging of things - and it'd be stupid for her not to start by looking at - and she can decide after a year or so whether she wants to carry on – rather than that - the old idea when you read for three months when you started your PhD - and fall asleep at your desk regularly as kind of I did - before you generated ideas - I think there's a way of starting a PhD now which is much more kind of experiential and interactive in that yeah do the reading but also get out there and measure things - you know and I guess you know this is almost like having field centres - you've got a corpus of knowledge there that you can take and run with and almost just experience what it's like to kind of feel that data, use it, analyse it and hopefully that will actually help your process of learning and help you derive you know meaningful research hypotheses, or at least ones that are less likely to fail, or at least ones that have a greater resonance with the research community thereafter rather than going up blind avenues or something like that so yeah I mean that's exactly - that's exactly what - how I - started which is why I direct people to do things in the Clearwater (Finn: mid-career fluvial geomorphologist interviewed October 2014).

Similarly, several interviewees explained that the seeds of a future research project would be found within an existing project. The senior geomorphologist commented:

> The way I do research is I'll have something that I decide I want to look at - whatever it is - and so I will design some sort of field programme - through which I can look at this issue - I can go out and collect data - but in collecting that data I'm always looking around me … and then that's where I get the idea for the next bit of

> research … my next hypothesis has always been developed while I'm testing the current one (Aidan: senior fluvial geomorphologist interviewed February 2015).

The early career geomorphologist explained that new avenues for research often emerged in the final stages of a research project:

> From personal experience I've found that quite often - there have been a couple of things I've done it's been almost the very final stage once everything's been analysed then quite often working with co-authors they ask questions that didn't occur to you so working in that collaborative way can spark new ideas and new ways of thinking about things and whether those are things where you think 'oh that's really interesting - maybe that's something we can look at in the future' or whether it's 'oh yeah actually maybe we need to look at that in a bit more detail because that could change maybe how we're looking at this particular facet of the research because that might change our conclusions so we need to look at that in more detail so those sorts of things have certainly happened (Kumara: early career fluvial geomorphologist interviewed August 2015).

Kumara explained, however, that their own close involvement with a project, and their sense of having laboured with a particular problematic to achieve a written-up analysis, could preclude their own ability to identify potentially fresh avenues of research; new eyes would be needed:

> That's probably one of the really tricky things in research is doing that personally because you - you know you kind of initially conceptualise the landscape and then you do research and maybe see how that conceptualisation changes and that's kind of the foundation for whatever research you're publishing then having kind of written that all up it can then prove quite difficult I think to then personally kind of take another step back and look at it … because you're like ok well I've written it up and now I need to send it out and get it reviewed and that's all done with now so that's kind of a point when it's good to have other eyes look at it and sort of say actually now it's all written up I can see that there's this angle maybe we haven't looked at in as much detail as we could do or how about we kind of flip this bit on its head and see how that all works so it's good to have that erm to work with other people to have that kind of – that different view point because as an individual researcher I think it's challenging to take that step back once you think it's all done (Kumara, early career fluvial geomorphologist interviewed August 2015).

The spaces and timeframes of a given research project needed to be open to the sparking of future research projects. However, although there was room for new research avenues to emerge they needed to be retained in a latent form until a time when they could be developed. Kumara commented that:

> I kind of guess that it's always a matter of the amount of time you've got to do something because quite often I mean in terms of my PhD it was more of a kind of case of having to go well I've got more than enough now and there are other things I'd like to do so I need to stop doing things and finish and then I can maybe put these things on a backburner and develop them a bit later on so I suppose I've done - there's a - I developed an idea on a kind of theory … right towards the end of my PhD and I realised I wouldn't have time to really do anything about this but I

> had this kind of germ of an idea and then after I started my postdoc I just did a weekend of the fieldwork to collect some data to test it and I've got a paper in review about that at the moment (Kumara: early career fluvial geomorphologist interviewed August 2015).

The latent presence of new avenues of research within the confines of a given project indicates a depth to the spaces of research projects neglected in Latour's (1999a) linear account of the transformations that occur between field and text.

SUMMARY: RETHINKING SPACE

This section has examined how the fieldwork is always already shaped, and situated, by a range of dimensions: physical, social, disciplinary, comparative, informational and temporal. This approach to the spaces of fieldwork provides the basis for an empirically strong understanding that emphasises the richly interwoven, and eventful ontology of geomorphological fieldwork.

According to Wylie, ANT employs a 'topological', rather than Euclidean spatial ontology. He argues that:

> The spatial ontology of ANT, in a deliberate differentiation from that proposed within Cartesian and Euclidean geometries, is profoundly *topological*. Within the ANT literature a topological vision of space and spatial relations is persistently invoked, in terms of networks, connections, and configurations of the near and far, such that geography "concerns itself with the topological textures which arise as relations configure spaces and times" (Murdoch 1998, page 359; see also Hetherington, 1997) (Wylie 2006: 521, emphasis original).

However, as seen in chapter two, Latour's analysis retains the vestiges of a Euclidean ontology. This is connected to what Law terms 'old ANT' (2002: 96); an ontology of 'immutable mobiles' whereby objects are immobile in 'network' space, enabling them to remain intact whilst traversing physical space (2002: 96). Law comments that:

> Such is the anatomy of Bruno Latour's notion of 'immutable mobile'. We have learned that *immutability* belongs to network space while the *mobility*, a Euclidean attribute, becomes possible because of network immutability (Law 2002: 96).

This section has taken a closer look at the spaces of fieldwork to argue that they are more complex; it has conceived of fieldwork as located, or 'implace[d]' (Casey 1998: 302), amid a range of dimensions. Such a view concords with Livingstone who, in discussing a 'rudimentary taxonomy' (2003: 21) of the spaces of scientific practice, argues that:

> What all these spaces share … is that – in common with all other places – they are *made*. The become what they are through the activities that 'take place' in them and the human practices that constitute them … space is therefore not dead,

> inert, and fixed; rather it is lively, shifting, fluid. *Space is animated by events*. It is *always* a production. And scientific space is no exception (2003: 85-86).

In addition, the fieldworker is transformed through their encounter with new sites. Again, as Livingstone comments, 'In turn these arenas are active in producing the kinds of subjects humans are in those spaces' (2003: 86).

However, this chapter has challenged the opposition between what might be termed the 'smooth' spaces of the field and 'striated' spaces of text or academy; it has argued that the spaces of fieldwork may be smooth, or striated. In this way, as seen above:

> Smooth space is constantly being translated, transversed into a striated space; striated space is constantly being reversed, returned to a smooth space (Deleuze and Guattari 1987: 474 cited in Casey 1998: 308).

As such 'the field … turns out to be anything but the obvious scientific site it might initially seem to be' (Livingstone 2003: 48). Spaces of fieldwork are 'not points or areas on maps' (Massey 2005: 130); instead they are heterogeneous (human/non-human) events.

CONCLUSION: COMPLICATING REFERENCE

> What we might have called representation is no longer a process of fixing, but an element in a continuous production; a part of it all, and itself constantly becoming. This is a position which rejects a strict separation between world and text and which understands scientific activity as being just that – an activity, a practice, an embedded engagement *in* the world of which it is a part (Massey 2005: 28, emphasis original).

This chapter has sought to provide a fresh exploration of 'implacement' (Casey 1998: 302) in geomorphological fieldwork. Section one found that exploring the field on foot is an important, and for some geomorphologists, crucial aspect of doing geomorphology. However, it challenged the dichotomy, inherent in Latour's *Circulating Reference,* between embodied exploration in the three dimensions of the field site and an analytical gaze over the two dimensions of maps and other data forms. It concluded by employing the Derridean concept of 'supplement' to consider how fieldwork is always already augmented by scientific techniques. The supplement of the scientific (the signifying) represents an addition to the landscape (the signified), but crucially also indicates a lack therein; human engagement with a nonhuman landscape results in the event of fieldwork. Section two examined how the fieldwork is always already shaped, and situated, by a range of dimensions: physical, social, disciplinary, comparative, informational and temporal.

Following this analysis it is necessary to return to Latour's opposition between one image where the scientists move in a 'hesist[ant]' and 'fumbl[ing]' manner through the field and another where they 'gaze' over a set of maps, the latter 'susceptible to the know-how of their age-old disciplines' (Latour 1999a: 29). Latour extends his opposition with a series of diagrams and offers a fresh explanation of the concept of 'reference'. Latour rejects the idea of a 'unique and radical gap that must be reduced through the search for correspondence, for reference, between words and the world' (p. 69). Instead, he argues that a '"disambulatory" conception of reference follows a series of transformations, each of them implying a small gap between "form" and "matter"'. This '"transformation" at each step of the reference … may be pictured as a trade-off between what is gained (amplification) and what is lost (reduction) at each information-producing step' (p.71). These 'transformations' involve a simultaneous reduction and amplification (see fig. 6):

> From forest to expedition report, we have consistently re-represented the forest-savanna transition as if drawing two isosceles triangles covering each other in reverse. Stage by stage, we lost locality, particularity, materiality, multiplicity, and continuity, such that in the end there was scarcely anything left but a few leaves of paper. Let us give the name *reduction* to the first triangle, whose tip is all that finally counts. But at each stage we have not only reduced, we have also gained or regained, since, with the same work of re-representation, we have been able to obtain much greater compatibility, standardization, text, calculation, circulation, and relative universality, such that by the end, inside the field report, we hold not only all of Boa Vista (to which we can return), but also the explanation of its dynamic … Let us call this second triangle, by which the tiny transect of Boa Vista has been endowed with a vast and powerful basis, *amplification* (Latour 1999a: 70-71, emphasis original).

It is surprising that Latour, who insists on collapsing the distinction between nature and culture (Latour 2004: 227) retains a relatively simple, foundationalist, account of reference as he 'follow[s] the many steps between our arrival at the site and the eventual publication' (Latour 1999a: 32). Hinchliffe et al. (2005: 649) argue that Latour's account '…is, to be clear, not about correspondence to something that existed before all this activity, or about gaps between world and word' since 'Both stories would bring us back to those tightly bordered islands called 'human' and 'nature' and lose sight of the promise of nonhumans'. However, in Latour's analysis sign and signified remain *polarised*; Latour states that 'There is, as I have said, a complete rupture at each stage between the "thing" part of each object and its "sign" part' (1999a: 60) since 'at every stage, each element

belongs to matter by its origin and to form by its destination' (p. 56). He sees each step of movement between field and institution as characterised by a gradual loss of 'locality', 'particularity', 'materiality', 'multiplicity' and 'continuity' matched by an increase in 'compatibility', 'standardization', 'text', 'calculation', 'circulation', and 'relative universality' (p. 71). Where therefore is the symmetry between nature and culture? An alternative approach, advocated here, heeds Merriman et al.'s call to 'work against conventional binaries such as stasis–movement, representation–practice (or the non-representational), textual–non-textual, and immaterial–material' (2008: 193).

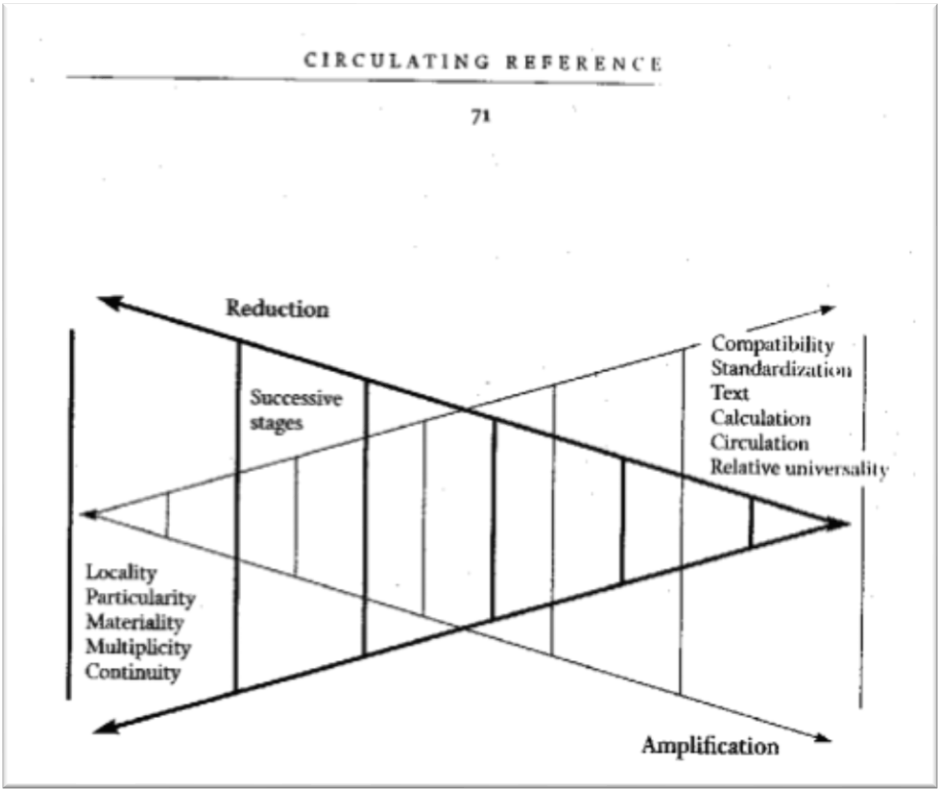

Figure 6: Latour's explanation of amplification and reduction at each step of reference (Latour 1999a: 71)

Again, and conscious of the danger of 'a modish preparedness to employ adventurous theoretical literatures' (Powell 2002: 267), this chapter takes inspiration from Derrida, heeding his call to 'complicate reference' (Kearney 1984: 124). Derrida's project, with regards to western metaphysics, is deeply connected to rethinking the relationship between signifier and signified. However, in an interview with Richard Kearney, he was at pains to clarify his much cited statement that '*il n'y a pas de hors-texte*':

> There have been several misinterpretations of what I and other deconstructionists are trying to do. It is totally false to suggest that deconstruction is a suspension of reference. Deconstruction is always deeply concerned with the 'other' of language. I never cease to be surprised by critics who see my work as a declaration that there is nothing beyond language … it is, in fact, saying the exact opposite. The critique of logocentricism is above all else the search for the 'other' of language … Certainly, deconstruction tries to show that the question of reference is much more complex and problematic than traditional theories supposed. It even asks whether our term 'reference' is entirely adequate for designating the 'other'. The other, which is beyond language and which summons language, is perhaps not a 'referent' in the normal sense which linguists have attached to this term. But to distance oneself thus from the habitual structure of reference, to challenge or complicate our common assumptions about it, does not amount to saying that there is *nothing* beyond language (Derrida and Kearney 1984: 123-124).

As Clark makes clear, 'Derrida is interested in the play of the world, and not just the play of language' (2003: 47). By attending to the interweaving of signifier and signified in the production of spaces of geomorphological fieldwork it is possible to avoid polarising 'compatibility', 'standardization', 'text', 'calculation', 'circulation', and 'relative universality' on the one hand and locality', 'particularity', 'materiality', 'multiplicity' on the other (Latour 1999a: 71).

This chapter has investigated the spaces of fieldwork in geomorphology. In doing so it has developed an account of the spaces of scientific inquiry as characterised by a range of dimensions including the physical, social, disciplinary, comparative, informational and temporal. The next chapter extends these findings to consider field and laboratory experiments undertaken by researchers based at the Rock Breakdown Laboratory, School of Geography and the Environment, University of Oxford. Here the analysis will focus on a well-established question in ANT analyses: how do research objects withstand displacement amid the spaces of scientific inquiry. However, new life is breathed into this old question. The analysis focuses on the disciplinary dimension of scientific spaces and considers the interdisciplinary 'cross-appropriation' (Spinosa et al. 1995: 3), or 'displacement' (Latour 1986), of research techniques into the experimental field and laboratory spaces of rock weathering geomorphology.

CHAPTER 5: CRAFTING INTERDISCIPLINARITY I - TRACING THE CROSS-APPROPRIATION OF TECHNIQUES

INTRODUCTION: CRAFTING INTERDISCIPLINARITY - CROSS-APPROPRIATION

> Pourquoi certains concepts scientifiques connaissent-ils une vie nomade, d'une science à l'autre? Que deviennent-ils lorsqu'ils passent d'une science 'dure' à une science 'molle', ou inversement? Conservent-ils le même sens? Contribuent-ils à unifier le champ des sciences? Ou bien en compliquent-ils plus le relief?[21] (Stengers 1987: 402)

This chapter is anchored in an ethnographic study of research conducted by members of the Rock Breakdown Laboratory at the School of Geography and the Environment, University of Oxford. It examines the interdisciplinary 'cross-appropriation' (Spinosa et al. 1995: 3), or 'displacement' (Latour 1986), of research techniques into the experimental field and laboratory spaces of rock weathering geomorphology. This focus develops a theme present in Latour's analysis (1999a) and prevalent in ANT (Law 2002); however, building on the analysis of the spaces of geomorphological inquiry in chapter four, such spaces are reconceived as shaped by disciplinary differentials among a number of dimensions (others including, as seen in chapter four, physical, social, lateral, informational, and temporal dimensions). In addition, research techniques are understood as a component of, and proxy for, relationally conceived research objects. The principle focus of the chapter is therefore on the displacement of techniques amid disciplines (see Donaldson et al. 2010), as well as across physical space, since, as Daston notes, 'scientific objects flout the boundaries between scientific disciplines' (2000: 12). In order to attend to this displacement the genealogies of techniques are examined; this brings a historical dimension to the analysis and allows attention to 'the past and the present in the present' (Bloch 1977: 278).

This chapter also begins a theme, continued in chapter seven, that considers what is termed 'research entrepreneurship'. The use of this term does not refer to entrepreneurship as a profit-making enterprise; rather it seeks to highlight the constant opening out of research possibilities associated with evolving experimental spaces and practices. With reference to Spinosa et al., three aspects of 'entrepreneurship' (1995: 3) are identified:

[21] Why do certain scientific concepts know a nomadic life, from one science to another? What happens when they move from a 'hard' science to a 'soft' science, or vice versa? Do they preserve the same meaning? Are they contributing to unifying the field of science? Or do they further complicate the relief?

- ‘Cross-appropriation’: where ‘one domain of practices takes over useful practices from another domain’ (1995: 3)
- ‘Reconfiguring’: whereby ‘a marginal practice becomes central’ (1995: 3)
- ‘Articulating’: whereby ‘dispersed or confused practices are brought into clearer focus’ (1995: 3).

Chapters five takes the first aspect as a focus, examining the cross-appropriation, or displacement, of techniques amid the disciplinary, as well as the physical, dimensions of scientific spaces. Chapter seven develops this discussion examining processes of reconfiguration and articulation as the spaces of scientific inquiry are reworked to accommodate interdisciplinary research objects.

Commencing this analysis, this chapter proceeds in three sections that, while taking a departure point in participant observation, emphasise literature- and interview-based research (including material gathered by other interviewers (Grove 2010; Maddrell 2011)). Chapter six returns to an increased emphasis on participant observation, combining the latter with data gathered in the course of interviews and literature study.

Section one situates the chapter in relation to (i) the Science and Technology Studies (STS) literature and (ii) historical texts within the geomorphological literature. Section two takes the mid-twentieth century work of Arthur Strahler as a point of departure, examining Strahler’s career in relation to the war-interrupted work of two of his European contemporaries: Marjorie Sweeting and Richard Grove. It also discusses the role of Strahler’s student, Richard Chorley, who spent time at Columbia, Cambridge and Oxford in this period. Finally, there is review of the development of a laboratory at Oxford, focusing on (i) Marjorie Sweeting, who moved to Oxford in 1951 from Cambridge and (ii) Richard Grove’s doctoral student, Andrew Goudie, who followed in Sweeting’s footsteps in 1970. It should also be noted that Heather Viles, the current leader of the Rock Breakdown Laboratory and a former doctoral student of Marjorie Sweeting, also moved to Oxford in order to take up a Natural Environment Research Council (NERC) studentship in 1981 following undergraduate studies in Cambridge. The interdisciplinary work of Viles, who introduced the term ‘biogeomorphology’ in a 1988 book of the same title (Viles 1988), is examined in chapter seven.

In section two the displacement of ‘concepts’ and ‘information’ (Strahler 1992: 62) are emphasised. However, as Hacking points out, theory is inevitably intertwined with, or ‘true to’ the ‘phenomena produced or even created by apparatus in the laboratory’ (or,

presumably, the field) and 'measured by instruments that we have engineered'. According to this analysis a laboratory science 'matures', developing 'a body of types of theory and types of apparatus and types of analysis that are mutually adjusted to each other' (Hacking 1992: 30). Indeed, as Hacking argues 'what meshes … is … a network of theories, models, approximations, together with understandings of our instruments and apparatus' (Hacking 1992: 30). This 'mesh[ing]' (Hacking 1992: 30) is important and, as section three progresses, the analysis once again becomes more concrete, as it shifts to consider the displacement of techniques, rather than ideas. As seen previously, these techniques are understood as a hybrid-practice produced by human and non-human actors. Section three commences this shift with a focus on work undertaken by Andrew Goudie, Grove's graduate student who, together with Ron Cooke and Ian Evans, pioneered the development of experimental techniques in geomorphological rock weathering studies through the 1970s and 1980s; many of these techniques, and some of the original equipment, are put to work in contemporary experiments. An examination of Sherman's proposal that Goudie might be considered a 'successor' to Strahler and Chorley (1996: 108) is critically considered.

The chapter begins with an ethnographic vignette, returned to again in chapter six, which describes a contemporary field experiment undertaken by a member of the Rock Breakdown Laboratory in Oxford. Here the 'meshwork' (Ingold 2011: 63), or 'assemblage' (Bennett 2009: xvii) characterising the spaces of scientific inquiry benefits from the 'thick description' (Geertz 1973: 3) that participant observation uniquely provides.

SECTION ONE: SITUATING THE ANALYSIS

> Balancing on a ladder a doctoral student carefully gathered a set of small stone samples before climbing down to an improvised workspace on a ledge within an Oxfordshire ruin.
>
> > What I usually do is to take a picture, because when I take the samples down – I can't remember exactly where they were – so what I'm going to do now is to take a picture' (transcribed recorded conversation - December 2012).
>
> Reaching the bottom of the ladder the participant set the blocks carefully on a small mouse-mat pad. Wearing gloves as they worked, and taking care not to knock the samples, they first removed data-loggers – small metal discs, visually similar to large watch batteries - that had been fixed to one of each type of sample with blue tack. Each data-logger had tracked temperature and humidity conditions on the surface of the stone blocks, over the course of recent weeks. Each logger was placed, in turn, into a reader device connected via USB to the laboratory's 'tough-book' - a rugged laptop.

> I came every month since November - I have all the results - my computer is going to crash one day <laughter> (transcribed recorded conversation - December 2012).
>
> This is a really old computer – really really old – but it's really good because it's really robust. (Transcribed recorded conversation - December 2012).

Each transfer of data was a slightly nervous moment for the researcher; occasionally a data logger would fail – surrendering its data before they had chance to capture it. Therefore, there was always relief when the downloaded results had been safely copied and pasted into an excel spread sheet, adding to the ranks of data accumulating month by month.

> It's really stressful - because you know - small mistakes (Transcribed recorded conversation - December 2012).

The data-logger memories were then reset. The rock samples were then weighed, one at a time, using small battery powered scales (resembling kitchen scales) before the awkward process of reattaching the data-loggers began. Finally, the samples were returned to their original places, out of sight, on a platform at the top of the structure.

Figure 7: Small blocks are used for experimental investigation of rock weathering in a heritage conservation project. The data loggers (recording humidity and temperature) are circled (a and b). Once the data is downloaded the samples are weighed (c and d) (Images digitally altered to protect anonymity of participants using Zoner Photostudio 15)

The extract above is taken from field-notes produced during participant observation with members of the Rock Breakdown Laboratory at the School of Geography and the Environment, University of Oxford (see fig. 7). The description recalls elements of Latour's ethnographic study of soil scientists working in the Amazon (1999a): there is a similar focus on the processes of transformation via which scientists gather materials in the field and subject them to a series of refinements, rendering them amenable to

comparison, analysis and mobility across the distance between field, laboratory and ultimately (the production of a) text (see Fig. 8). Latour's methodology has the merit of focusing on the *material* configurations at work; his study is orientated towards scientific practice rather than ideas. Similarly, his attention to the work involved in transforming and safely packaging materials, in order that they may *survive displacement* is important (and retained within the analysis here, though with a reconfigured concept of displacement across space). However, there are a number of deficiencies in Latour's analysis, including an impossibly neat and linear rendering of the relation between field (as origin), laboratory and text (as destination) (Latour 1999a: 56; see Fig. 8 and detailed critique in chapters two and four). Similarly, the hefty logic of Latour's account precludes any opportunity for finely textured descriptive analysis of the contingencies and genealogical details underpinning each step in the transformations he describes.

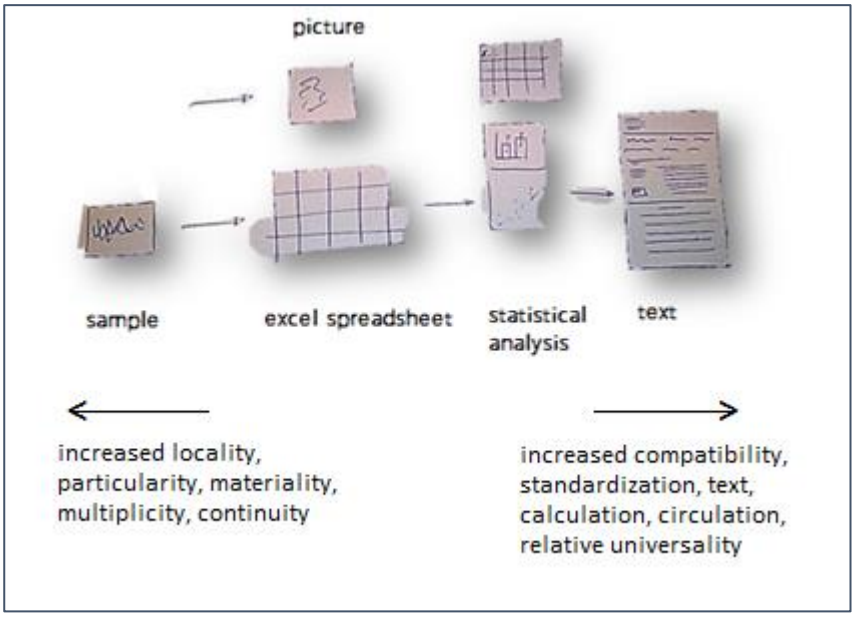

Figure 8: A breakdown of a Latourian 'transformation': moving from sample to number in a spreadsheet, via statistical analysis to peer reviewed text (adapted from Latour 1999a: 71)

This is surprising since, according to Latour's own account, as described by Daston, the 'persistence [of research objects] … depends on the institutionalization of practices and an impressive array of apparatus' and their reality depends on 'the degree of [their] embeddedness in such organized systems of techniques and instruments' (2000: 13). As seen in chapter two, Latour argues:

> We do not have to consider physical entities such as ferments, germs, or eggs sprouting into existence as being radically different from a *context* made up of colleagues, emperors, money, instruments, bodily practices, etc. … [This] has the

> great advantage of requiring us to stabilize neither the list of what makes up nature nor the list of what makes up context (2000: 260, see also 1999a: 164-5).

Such 'context' is only very thinly present in Latour's analysis of soil scientists in the Amazon, which has a tendency to 'funnel[22]' (Star and Griesemer 1989: 390) the focus of his analysis. Early work by Star and Griesemer noted this tendency, and its associated emphasis on the will or aims of the experimenter, rather than the jostling of this with the aims and interests of other actors; they sought to take an 'n-way' approach to analysing the production of scientific research objects, whereby the path of transformation emerges from negotiation between the interests of many actors (1989:389).

The approach taken in this chapter is slightly different; it seeks to emphasise the *genealogies of techniques*, with particular stress on their cross-appropriation or, alternatively phrased, safe displacement amid spaces. According to this approach, techniques, which may be understood as a hybrid-practice produced by human and non-human actors, are a *component of* research objects; they form part of the context (Latour 2000: 260) of which research objects are relationally composed.

SAFE DISPLACEMENT AMID SPACES OF SCIENTIFIC INQUIRY

As seen in chapter two there is a strong emphasis in early ANT studies on the safe transfer (displacement) of research objects across space (Law 2002: 96). This is apparent, in terms of physical distance, in Latour's *Circulating Reference* that sees a distillation or filtering and transformation of field materials as they are displaced across space to a laboratory, where they undergo further experimental practices and are transformed into numbers and words in academic texts (1999a). Elsewhere, Latour is concerned with the transfer of materials through time, for example, in a characteristically counter-intuitive argument, Latour states that Ramses II couldn't possibly have died of tuberculosis since it had yet to be discovered; the specific configurations amid which Koch Bacillus emerged as a scientific research object cannot be overlooked (2000: 247-251, see also Daston 2000: 12). As seen in chapter two, in each case the objects are understood as stable in network space, this enabling their ability to withstand displacement across physical (Euclidean) space or across time (Ek 2012: 45, see also Law and Mol 2001).

[22] Writing ten years before the publication of Latour's Circulating Reference' (1999a), Star and Griesemer argued that 'Our approach differs from the Callon-Latour-Law model of translations and *interressement* in several ways. First their model can be seen as a kind of 'funnelling – reframing or mediating the concerns of several actors into a narrower passage point … The story in this case is *necessarily* told from the point of view of one passage point – usually the manager, entrepreneur, or scientist' (1989: 390).

What then of an ethnographic project dedicated to scientific research objects with a very specific relationship to time: those that form the focus of geomorphology (and the geosciences more widely)? As the study of earth surface phenomena, geomorphology deals with phenomena whose temporal[23] and physical scales dwarf those of a human lifetime, and certainly those of a research project or grant (For a recent discussion of scale within the geomorphological literature see, for example, Inkpen 2011; Phillips 1999; Summerfield 2005b; Viles 2001). Things might seem easier when focusing, as in this case, on a laboratory much of whose bread and butter work concerns the application of geomorphological research techniques to examine the effects of weathering processes in relation to heritage conservation and environmental management; that is to say with relatively extremely young entities such as man-made coastal defences or built heritage. However, hidden in this sentence is the phrase 'weathering processes', indicating a shift of attention, identified in much of the geomorphological literature, as moving from a focus on the 'time-bound' qualitative assessment of specific landforms ('what happened?') to the quantitative assessment of 'timeless' weathering processes ('what happens') (Strahler 1952: 924)[24]; a change explained as springing from 'exasperation with the evident impossibility to reconcile the time scale of observable and mechanically explicable processes with the spatial scale of the classically considered landscape' (Church 1996: 150).

This shift of focus provides an example of the evolution of scientific research objects within geomorphology, moving from Davisian descriptions of landscapes to an emphasis on geomorphological processes. Following Daston and Latour, scientific research objects are understood as being both real and historical (2000: 3); they do exist 'out there' beyond the constructions of scientific minds; however, they also come into being and fade away (Daston 2000). In this way a paradigm change is not just a shift in the ideas through which reality (out there) is interpreted; material and conceptual domains are not so easily

[23] Latour's question concerning the displacement of research objects through time has particular salience to an area of research which has benefitted significantly from advances in dating geomorphological materials formed in the historically distant past (Church 2010: 269)

[24] Church notes the effect of this shift was to 'set space and timescales for the subject; they were the everyday human scales, on the order of metres to kilometres and seconds to years, within which well-defined dynamical processes could be observed and reasonably comprehended. They were quite different than the scales of the preceding period' (2010: 267-8). However, Church also points out the possibility inherent in recent technological developments (including remote sensing, advances in dating techniques and access to personal computing) to 'open a way for a renewed consideration of the history of the landscape' (p. 269). Note too that through the debate within geomorphology concerning this focus on timeless processes vs time bound historical geomorphology. Unwin argues, for example, that an emphasis on 'timeless' process brings with it a focus towards '"equilibrium" morphological systems analysis', proposing instead that 'a mixed stochastic/deterministic framework … [would] allow the time dimension to be reincorporated into physical geomorphology' (1977: 185).

separated - scientific endeavours are therefore understood as being constitutive of scientific research objects (Daston 2000).

SITUATING THE ANALYSIS: HISTORY AND PHILOSOPHY IN THE GEOMORPHOLOGICAL LITERATURE

There is a well-worn track through the histories of geomorphology, one which has been particularly trodden by, among others, Richard Chorley, in a four volume chronicle (Chorley et al. 1964, Chorley et al. 1973, Beckinsale et al. 2003, Burt et al. 2008). As noted above Chorley is one of the key figures in this chapter's analysis. Another key figure, Goudie, described Chorley's work on the history of geomorphology:

> His most durable memorial, and to me his most enjoyable, is a work that reviewers always describe as 'monumental' – the multivolume and yet to be completed *History of the study of landforms* which he has co-authored with his Oxford tutor, Robert Beckinsale. It is the accretion of over three decades of devoted and diverting scholarship (albeit rather more whiggish than some would like) (Goudie 1994: 318).

The perceived 'whiggish' tone of Chorley and his colleagues' rendering of the history of geomorphology is also noted by one of Chorley's former graduate students, Barbara Kennedy (Kennedy 2006: 141).

In the first chapter of the fourth volume of the History of the Study of Landforms (published after Chorley's death and co-edited by Andrew Goudie among others) Cox discusses the relationship between the historical accounts associated with these tomes and Science and Technology Studies (STS). He remarked that:

> Despite working at a time of extraordinary ferment in the history of science – not to mention the philosophy of science studies and other branches or flavours of what are now often called 'science studies' – [Chorley and his co-editors] did not write with much attention to their own methodology, or indeed anyone else's. Whether this was accidental or deliberate it is difficult to say … Chorley, Dunn and Beckinsale chose not to … sprinkle their histories of geomorphology with modish references as indicators of their intellectual chic … They wrote as geomorphologists writing … principally for scientists, scholars and students in or near geomorphology (Cox 2008: 8).

This insouciance had an advantage in Cox's view; his remarks were somewhat scathing about science studies:

> What does seem clear is that had our predecessors chosen some modish framework for their monographs it would have dated their work much more rapidly than their essentially narrative and (to historians' eyes) presentist approach (Cox 2008: 8).

Cox was particularly cautious of 'a multitude of more sociologically orientated scholars and critics, many of whom question essentially all claims and preconceptions about the distinctiveness and merits of science' (Cox 2008: 8). Regardless of this he noted more kindly that 'over the last forty years … these fields in science studies [had become] disciplines or at least approaches in their own right' and that 'to become seriously competent in any of them would have demanded a commitment of time and effort hardly possible for our predecessors or the contributors of the present volume'. While this would not preclude the 'borrowing of ideas or attempts to learn from science studies' it means that 'geomorphologists cannot convincingly pass themselves off as historians or philosophers of science' concluding that 'theirs – ours is a different enterprise' (Cox 2008: 8).

This commentary by Cox provides an interesting insight into the potential scepticism[25] of the geomorphological community when presented with a 'science studies' account[26]. While the move beyond constructivism in sociologies of science may help bridge the philosophical gulf that earlier separated scientists from those that sought to study them, many may still baulk at talk of 'real and constructed' scientific research objects. Chorley himself once wryly remarked that 'when someone mentions theory to a geomorphologist, he[27] instinctively reaches for his soil auger' (Chorley 1978: 1 cited in Rhoads and Thorn 1993: 287, see also Powell 2002: 265); if this is true of geomorphological theory, then it is perhaps even more so in relation to science studies theory, even as science studies has made itself at home within geography (Barnes 2014: 207).

While Chorley and his co-editors' accounts of the history of geomorphology may have, in the view of some, been excessively whiggish (Goudie 1994: 318; Kennedy 2006: 141), this chapter seeks to examine the development of techniques associated with rock weathering geomorphology from an alternative perspective, influenced by *genealogical* approaches. The term genealogy is described by Sherratt as denoting 'the very opposite of tracing a pedigree … with a single, continuous, unbroken line of descent to the unitary source of an item' (Sherratt 2006: 134-135); indeed such attempts are associated with

[25] Though the positive response of geomorphologists to the invitation to participate in this study should be noted.
[26] Barnes recounts the 'disappoint[ment]' of some of his participants 'with [his] history' of the discipline (2014: 221)
[27] It is interesting to note that Chorley instinctively reached for the male pronoun – his former doctoral student Barbara Kennedy, who would no doubt provide much insight into gender in geomorphology, later counselled 'I would say never ever write a book with your PhD supervisor because you don't have the clout to stand up to them' (Maddrell 2011). See Haraway (1989) for discussion of gender in primate studies; although not the principle focus of this research the issue of gender is certainly apposite to any consideration of the development of the discipline of geomorphology.

'legitimis[ing] the king's rule and power' and as being, in the words of Geuss, 'in the interests of a positive valorisation of some item' (Sherratt 2006: 135; note a similar point is made by Taylor (1976) in an early account[28] of the quantitative revolution in geomorphology). As such, genealogies are orientated towards 'multiple' and 'indirect, fragmented, dislocated lines of descent' (Sherratt 2006: 136). As Hill notes, 'there are affinities between ethnography and genealogy' (2009: 310), including the adoption of 'a context-bound critical approach' and an attempt to 'recover excluded subjects and silenced voices'[29] (Tamboukou and Ball 2003: 3-4 cited in Hill 2009:311); more generally it might be noted that both ethnography and genealogy proceed with eyes attuned to nuance and wary of the flicker of tales too neat.

At this stage in the analysis it willingly treads, and rarely strays from, the well-worn histories that describe a mid-century revolution that pulled geomorphologists from their hilltop views across the landscape, tethering them to careful and methodical analysis on a scale more proportionate with their own (Summerfield 2005b, Schumm and Lichty 1965, see also Powell 2007b: 1798, Phillips 1999). It does this, however, with an ethnographer's eyes and feet (inadept and amateurish in their early steps[30]), beginning with that which is at hand, wandering into a thicket, and painstakingly starting to tease out threads here and there (see Jackson 1989; see also chapter three). As Cook et al. comment:

> The ethnographer chooses a base, has certain research questions in mind, finds points in networks of people and places whose comparison promises to shed light on those questions, and then visits some of those points (people, events, and so on) in a sustained and concerted way, in the process, of course, revising the questions with which he or she began. It is only by means of a persistent (if productive) fiction that this activity could be construed as an attempt at a complete description of a bounded site (Cook et al. 2009: 58).

The descriptions of this chapter do not, in any way, provide a comprehensive account of the Rock Breakdown Laboratory (Cook et al. 2009: 58), nor of the highly accomplished careers of the geomorphologists concerned. Instead, taking its point of departure in the

[28] In the context of Cox's discussion of the relationship between science studies and geomorphology it is interesting to note Taylor's comment that his work, while using 'the concepts and ideas from the sociology of science', was 'written for consumption by geographers not sociologists'; citing Andreski he underlines the 'intellectual duty to know ourselves and how we operate' (1976: 141).

[29] This attention to silenced subjects might be extended to an attention to silent actors; that is to say the non-human actors that populate, alongside their human counterparts, the configurations associated with experimental spaces. See Whatmore for a discussion of the role of ethnography in 'redistributing attention from exclusively human actors, what scientists say and do, to the host of non-human devices, codecs, bodies, and instruments that are active parties in 'doing' or practising science' (2003: 93).

[30] Reich described participant observation as 'a time honoured tradition for making a fool of oneself for a point' (Kutsche 1998: 5 cited in Fedorak 2012: 5)

ethnographic vignette with which the chapter commenced, it seeks to attend to, and follow, the trace of the 'past … in the present' (Bloch 1977: 278).

SECTION TWO: A MID-CENTURY REVOLUTION – DISPLACEMENTS OF PEOPLE AND IDEAS

POST-WAR GEOMORPHOLOGISTS: COLUMBIA, CAMBRIDGE AND OXFORD

This section sets out, in broad brush strokes, brief biographical accounts of key researchers based at, or moving between, three research centres: Columbia, Cambridge, and Oxford, in the 1940s-1970s. Table seven shows a series of relationships between these sites and individual scholars during the second part of the twentieth century.

STRAHLER

In the course of the early 1940s, while Cambridge graduate student Marjorie Sweeting (later the supervisor of Heather Viles in Oxford) passed the war in Denbigh, North Wales (Grove 2010: 144, Viles 1996: 429), and her contemporary Richard Grove (later Andrew Goudie's supervisor at Cambridge) was flying aircraft for the RAF (Grove 2010: 37-43), Arthur Strahler remained relatively insulated from the ravages of war[31]. As he analysed the results of his graduate and early post-doctoral work, he realised that his results did not make sense within the dominant, Davisian paradigm (Sack 2008). None the less, constrained by the 'aegis of a dominant figure' (Strahler 1992: 67), and reconciled to his perception, noted above, that 'few doctoral dissertations do more than extend the supervising professor's work' (Strahler 1992: 66-67), it wasn't until 1944 that Strahler began to feel ready to spread his wings; his doctoral supervisor, Douglas Johnson died in February of that year and overcoming criticism ('my failure to discover new and critical geological evidence was highly frustrating to me and fully evident to the defence committee' (Strahler 1992: 67)), his thesis was approved and published in January 1945.

[31] Strahler states that in the third year of his doctoral research he held the University Fellowship before being appointed as 'Lecturer in Geomorphology' in 1941. Fieldwork was reduced due to fuel rationing and student numbers were radically reduced, following the Japanese raid on Pearl Harbour in 1941. Strahler's lectureship continued though shifted towards military application – something that continued in the post war years when his research was funded significantly by the Geography Branch of the Office for Naval Research (1992: 67 and 71).

Table 7: Lineages – research centres and individual scholars. Blue arrows indicate supervisor relationships while red arrows indicate movements between departments

	COLUMBIA UNIVERSITY (GEOLOGY)	UNIVERSITY OF CAMBRIDGE (GEOGRAPHY)	UNIVERSITY OF OXFORD (GEOGRAPHY)
	1939 Strahler completes Masters	1938 Marjorie Sweeting enters Cambridge (undergraduate)	
1940s		1941 Richard Grove enters Cambridge (undergraduate)	
			1948 Chorley begins as undergraduate at Oxford.
1950s	1951-1957 Chorley conducts research under supervision of Strahler		
	1952 Strahler publishes 'Dynamic Basis of Geomorphology'		1951 Marjorie Sweeting moves to Oxford
			1957 Chorley returns to Oxford
		1958 Chorley leaves Oxford and takes up post at Cambridge	
1960s		1964 Andrew Goudie enters Cambridge	
		1967-8 Andrew Goudie conducts research in the Kalahari under supervision of Richard Grove	
		1969 Barbara Kennedy gains her PhD under the supervision of Richard Chorley	
1970s			1970 Andrew Goudie moves to Oxford
		1978 Heather Viles MA – Cambridge	
			1981 Heather Viles begins doctorate at Oxford supervised by Marjorie Sweeting
1980s			Heather Viles undertakes postdoctoral work examining the contribution of acid rain to deterioration of English Cathedrals
			1988 Heather Viles publishes 'Biogeomorphology'

In March of that year he was inspired by an engineer's contribution to geomorphology: Horton's quantitative analysis of drainage systems (Horton 1945 cited in Sack 2008). Strahler remarked on a displacement occurring across a disciplinary differential:

> On first reading, my reaction was one of great excitement. Here, I realised, was a gold mine of fluvial concepts based on a lifetime of field studies by a hydraulic engineer. *This was a remarkable interdisciplinary transfer* of information from hydrology, a geophysical area of knowledge, to a geomorphology largely rooted in geological concepts (1992: 62, emphasis mine).

On receiving a reprint of Horton's paper from the author, shortly after its publication and not long before the death of the hydrologist, Strahler found a typed note pasted to the table of contents reading:

> It is said that "He who runs may read". Some persons are allergic to mathematics and so may run without reading this paper. It is hoped that you will read at least the synopsis and that you will find it sufficiently interesting to tempt you to read further (Horton cited in Strahler 1992: 69).

Strahler was indeed inspired to read further, and 'in reaction to Horton's paper … began a programme of self-improvement in a number of engineering subjects, including hydrology, soil mechanics and fluid dynamics' (1992: 69). He later added to this, 'auditing courses in elements of mathematical statistics given by Frederick E. Croxton for students of business and economics and after that a course on biometrics offered by Howard Levene for students of the Zoology Department' (1992: 70).

Strahler's sense of excitement at the promise he identified in Horton's paper is clear in his seminal 1952 paper: 'Dynamic Basis of Geomorphology':

> If geomorphology is to achieve full stature as a branch of geology operating upon the frontier of research into fundamental principles and laws, it must turn to the physical and engineering sciences and mathematics for the vitality which it now lacks (1952: 924).

However, if Strahler was aware of an opportunity for the interdisciplinary transfer of techniques, he also identified a threat:

> Although the study of fundamental principles of geomorphology by engineers is to be welcomed and encouraged, there is a real danger that the engineer will find it necessary to take over an increasingly greater proportion of geomorphic research and thereby cut it off from its most logical parent, the field of theoretical geology (1952: 924).

Strahler's move was at once interdisciplinary (see Barnes 2008a: 14) and protective of disciplinary specialism.

A EUROPEAN MESSENGER

The publication of Strahler's 1952 paper coincided to within a year with the 1951 arrival of Chorley at Brown University (Haggett and Stoddart 2009: 66) following his undergraduate training in Oxford. Originally intending to study under Armin Lobeck, Chorley discovered on arrival in New York that Lobeck had been taken ill, therefore it was by chance that he became part of a 'firecracker group of graduate students - including Stanley Schumm, Mark Melton and Marie Morisawa ... all working under Strahler' (Haggett 2002: 523, see also Haggett and Stoddart 2009: 67). Following posts teaching geology at Columbia (1952) then Brown University (1954), personal circumstances forced Chorley's return to the UK (Haggett 2002: 523). He registered for a DPhil back in Oxford under Marjorie Sweeting, however, according to Haggett and Stoddart, her absence overseas meant he instead worked primarily with his former undergraduate tutor Beckinsale. While at Oxford he undertook ‘extensive fieldwork’ in the UK, ‘augering samples from the soil’ and bringing these back for analysis at ‘the laboratory run by P.H.T. Beckett in the University School of Agriculture’ (Haggett and Stoddart 2009: 67). However, his time back at Oxford was short; in 1958 he sent a speculative application for work to the universities of Sheffield and Cambridge. Cambridge were quick to reply: ‘the following day he was telephoned by Professor Steers and called for interview… then 31 [he was] to stay in Cambridge for the rest of his life' (Haggett and Stoddart 2009: 67).

Richard Grove, a senior colleague of Chorley's at Cambridge, described his younger colleague's time at Oxford:

> When he returned to this country I think he was – at first returned to Oxford and I think Marjorie Sweeting was his supervisor at first and he was supposed to work on erosion surfaces down in Cornwall or Devon or something like that. And I think he – he revolted and decided this was not a – you know, was not on, didn’t fit with the ideas that he’d absorbed in the States. And then he arrived in Cambridge in – when, in 1957, something like that and was appointed to a demonstratorship (Grove 2010: 264-265).

In this way, according to Haggett, 'it was at Cambridge … that the revolutionary fuse lit by Strahler was to explode in the sixties' (2002: 523).

AN UNLIKELY DISPLACEMENT – THE DEVELOPMENT OF “SCIENTIFIC” TECHNIQUES AT OXFORD’S SCHOOL OF GEOGRAPHY AND THE ENVIRONMENT

While Cambridge and Columbia appeared to offer a ‘benevolent environment’ (Haggett 2002: 523) for the development of fresh ideas, the Oxford environment against which Chorley revolted was, in this period, ‘not exactly world renowned for its scientific ethos’

(Kennedy 1995), despite Marjorie Sweeting's best efforts to cultivate a more scientific approach. Andrew Goudie, a leading geomorphologist closely associated with the development of the rock weathering laboratory since the 1970s, commented on the Oxford department into which Marjorie Sweeting arrived from Cambridge in the 1950s (and into which Richard Chorley returned following his time working with Strahler in the US), noting that:

> The Oxford School of Geography in the 1950s and 1960s was regarded by many outsiders as inward-looking and conservative, and for the most part (though with notable exceptions) the research ethos was relatively weak. Obtaining a doctorate or writing papers in academic journals was seen by some Oxford dons at the time as being something that was best left to Americans and those at lesser civic universities in the UK (Goudie 2011).

As noted above, Chorley, returning from the U.S., stayed in this environment for barely a year (1957-58)[32]. However, Andrew Goudie, a former graduate student of Richard Grove at Cambridge in the 1960s, followed in the footsteps of Marjorie Sweeting when in 1970 he moved from Cambridge to Oxford, taking up a post as Departmental Demonstrator in Statistics and Oceanography. Here he developed work he had begun in Cambridge on experimental techniques for examining salt weathering processes in desert environments. At a symposium convened in honour of his retirement, Goudie recollected a conversation with a Jesuit priest in Oxford, who had argued that publishing was 'not done in Oxford', that it was 'very red brick' (Goudie 2012). Similarly, he recollected that at the time of his arrival there had been just one calculator with a square root key (Goudie 2012).

SUMMARY

This section has set out initial biographical accounts of scholars working across three key sites (Colombia, U.S.A., Cambridge, U.K. and Oxford U.K.) in the second half of the twentieth century. It is clear in this analysis that both physical distance and disciplinary differentials are important dimensions of the spaces amid which 'concepts' and 'information' (Strahler 1992: 62) withstand displacement. Focusing on the displacement amid disciplinary differentials provides an alternative perspective on a traditional ANT theme: the displacement of research objects across space. The spaces of scientific inquiry are therefore reconceived, as seen in chapter four, as defined along multiple

[32] Though further research would be valuable in investigating whether Beckett's laboratory in the University's School for Agriculture, where Chorley analysed his augered soil samples, provided an antecedent for the development of the contemporary rock weathering laboratory, either in terms of experimental techniques or equipment (see Haggett and Stoddart 2009: 67).

dimensions. However, the next section works to deepen and complicate the analysis, tracing the genealogies not only of 'concepts' and 'information' (Strahler 1992: 62) but also of the techniques encountered in the ethnographic vignette with which the chapter opens.

SECTION THREE: ANDREW GOUDIE - A SUCCESSOR OF STRAHLER?

> Just as we might see Chorley as descended from Strahler, we might ask if the succession now continues through a subsequent anointing of Goudie (Sherman 1996: 108).

Sherman's 1996 paper on 'fashion in geomorphology' identifies Andrew Goudie as a 'successor' to a line emanating from Strahler in Columbia, and taking root in UK Geomorphology via Strahler's graduate student Richard Chorley. This section examines Sherman's statement critically, and, in the spirit of a study working under the banner of 'tracing genealogies' argues that the picture is more complicated. Evidence of Strahler's influence on Goudie is considered through an examination of three episodes in Goudie's career. Firstly, it considers the atmosphere among Cambridge geomorphologists in the 1960s, while Goudie was a student, noting the emergence of a 'generation gap' (Taylor 1976: 138) between older and younger members of the teaching and research staff. Reservations towards the newer approaches to geomorphology on the part of Goudie's supervisor are considered. Secondly, it is noted that Goudie's first publication, although based on fieldwork undertaken alongside Richard Grove, was clearly more indebted to techniques borrowed from Strahler than to Goudie's supervisor. Finally, there is a consideration of work undertaken by Goudie in adopting and developing experimental techniques for the analysis of rock weathering.

CAMBRIDGE IN THE 1960S

Goudie was a student at Cambridge in the 1960s, following Chorley's return from Columbia; at this time Chorley was known for his 'lively lecturing style' and 'new ideas from the States' (Grove 2010: 265). Reflecting on this period, in a remark that echoed Secord's discussion of the role of Cambridge tea rooms as spaces of scientific conversation (Secord 2007), Goudie commented that:

> … well as I say there was Lewis there before [Chorley] and I mean Chorley replaced Lewis – and Lewis was a seriously good quantitative geomorphologist – so Cambridge had already moved in that direction but it was this group of them coming together - Chorley, Stoddart and Haggett – and I mean they always used to go to tea in the morning together, you know down at the Guild coffee house, and we joined in sometimes. And it was a very very exciting era – you know you were waiting for the next book – models or whatever to come out – and you know I

> bought them because it was all new and exciting – and you know it really did transform the place in many ways – very rapidly – very rapidly indeed (Interview with Andrew Goudie, October 2014)

However, Goudie's supervisor, Richard Grove, was less than enthusiastic about 'the more speculative type of geography that was introduced by Chorley and ... taken up by Peter Haggett and his contemporaries' (Grove 2010: 145). Haggett notes that it was 'Vaughan Williams and David Stoddart … who particularly encouraged Chorley's ideas, while Alfred Steers as an olympian head of department provided the benign environment in which his experiments could take place' (2002: 523). However, reflecting on Chorley's arrival in a recent interview, Grove commented that: 'I must admit [Bruce] Sparks and I were not very sympathetic to [Chorley's adoption of new ideas following his studies at Columbia] … I think probably we were both slightly irritated' (Grove 2010: 265). Grove indicated that, were it not for the untimely death of Vaughan Lewis, 'I think it's questionable whether Chorley would have been re-appointed to a post in Cambridge in … 1962' (Grove 2010: 145). Asked in what way he had been unsympathetic to Chorley's ideas he further commented:

> I think we felt it was – was possibly too theoretical for us and very much concerned with statistical approaches … mathematical approaches anyway. And I think we'd both been brought up with the importance of – of field observation and measurement rather than – rather than theory (Grove, 2010: 265).

Grove also indicated that he felt Alfred Steers, who Haggett saw as providing a 'benevolent environment' (2002: 523) for the development of Chorley's ideas, would in fact have been wary of Chorley's approach:

> Steers who I – I can remember sort of as an undergraduate introducing sort of theoretical and modelling sorts of ideas into essay answers and he wouldn't have anything of it … [He would say] 'Oh yes, well, this is all very well but,' you know, you've got – you've got to go out and see what – you've got to go out and measure what's – what's going on' (Grove 2010: 265).

Echoing Grove's recollection of Steers, Barnes notes that Steers reacted with scorn to a paper, featuring a 'multiple regression equation' (Barnes 2004: 579), presented by Peter Haggett at a Royal Geographical Society (RGS) conference; Steers reportedly commented that 'This kind of thing has got to stop' (Thrift 1995: 381-82 cited in Barnes 2004: 579).

Grove recalled particular concerns regarding the influence of Strahler's ideas on the post-graduate community:

> I think we were distrustful at first, particularly as to whether it was appropriate in - at the post-graduate stage in - whether - whether it was going to produce acceptable research work. And I think there was a certain tension in the geography department (Grove: 2010: 145).

Although Church describes the production of a 'substantial consensus' in relation to Strahler's new approach, reports from the time indicate what an early sociological study of geography in this period described in terms of an opening 'generation gap':

> By the end of the 1960s presidential addresses normally included a section on the speaker's attitude to the new trends which by this time were self-evident to the whole community. These attitudes seem to relate to the final two of Hoyt's suggested responses - either standing firm or seeking compromise. However, all responses have one common characteristic-the perception of a generation gap which the traditionalist's clearly saw opening up below them … In particular, the emphasis on initial difficulty derived through quantification seems to operate to produce two sets of geographers - the older geographers with little quantitative skills and the young geographers with some knowledge of statistical techniques (Taylor 1976: 138).

Some, such as Steel and Watson (below), were relatively reserved in their criticism; however, others complained of 'the arrogance and pretentiousness of some, and only some, of the "New Geographers"' (Farmer 1973: 8 cited in Taylor 1976: 138). An example of an emerging 'generation gap' (1976: 138) can be seen in a report on the status of British Geography made to the 1972 International Geographical Congress by Steel and Watson (1972)[33]:

> A report of this nature is not the place to discuss the virtues of the so-called 'old' geography or the challenges of the 'new' or to assess the significance of the 'revolutions' in geography, whether quantitative, theoretical or otherwise … In retrospect and in perspective what has been described as the 'quantitative revolution' may represent nothing more than the proper adoption by geographers of new techniques that are commonly used by other natural and social scientists. This has greatly affected the literature and the research of geographers just as it has influenced the teaching in many British departments of geography … The Madingley lectures for 1963, Frontiers in geographical teaching (ed. Chorley and Haggett, 1965), represent the first major impact of the application of quantitative methods to the understanding of geographical problems though a standard geographical text on statistical methods (Gregory, 1963) had already been published. Other works by both these writers, with or without other collaborators,

[33] Such changes were not limited to geomorphology but were occurring across geography. The situation of geomorphology as a sub-discipline of both geology (particularly in North America and geography (Bauer 1996: 381) may have allowed geomorphology to function as one bridge for a more scientific approach; however, in a subject as diverse as geography, there were many other bridges including, for example, economic geography (Barnes 2001b: 560). The role of geomorphology as such a bridge is, as seen above, epitomised in the work of Chorley; a key advocate of the quantitative revolution, Chorley was primarily a geomorphologist (Haggett and Stoddart 2009: 67), however, he did not confine himself to physical geography in (Haggett 2002: 523; Haggett and Stoddart 2009: 74).

> have followed in a steady stream (e.g. Haggett, 1965; Chorley and Haggett, 1967; Chorley and Kennedy, 1971) … while a new publication Progress in Geography provides reviews of current research in selected fields (Board et al, 1970). There is a tendency to stress the contrast between the approaches and methods of the second half of the sixties and all that went before it: but such a view could be short-sighted and ignore the importance of the sound foundations and developing traditions of a subject that has a long and honoured history (Steel and Watson 1972: 144, see also Steel and Watson 1972 cited in Taylor 1976: 139).

As Taylor remarks ‘we can only imagine how different the review would have been had, say, Chorley and Haggett been invited to produce it’ (1976: 139). He also elegantly identifies the paradox involved in this reference to history, his analysis describing at length the work of the quantifiers in rewriting the history of the discipline (1976: 136); he continues: ‘both groups see history as important, they just mean a different history’ (1976: 139).

EXPERIMENTING WITH STATISTICAL TECHNIQUES

Goudie's first published paper, investigating sand dunes, was based on research undertaken when, as a graduate student, he accompanied his supervisor Richard Grove to the Kalahari. As seen above, Richard Grove was not an ardent admirer of Chorley and the ideas he had become a proponent of following his time studying under Strahler in the states. Reflecting on the fieldtrip with Goudie he remarked:

> I think he [Andrew Goudie] was clear by the time I was arranging to go to Botswana that Andrew was about to graduate and that he was intending to do research. And I'd had the air photographs print laydowns of - of the Kalahari region for some years ... I'd made a map of the Kalahari from the air photographs with interesting landforms and so I thought - I thought there would be an opportunity for Andrew to select a research topic there with me and - which is what he did eventually. I - the paper I published on that, I did myself, and he published separately on something to do with pans or dunes but, yes, it was - it was quite a - quite a successful expedition (Grove 2010: 221).

Goudie cites his supervisor's simultaneous publication - noting that 'the Kalahari dunes have been mapped and described in general by Grove (1969)'; equally his use of air photographs 'made available by the South African trigonometrical survey in Pretoria' no doubt derives significantly from methodologies developed by Grove and others. However, Goudie’s experience appears to have contrasted with that of Strahler, who considered his own doctoral work to have been 'largely a waste of time' (1992: 67), and who commented 'few doctoral dissertations do more than extend the supervising professor's work' (1992:

66-67)[34]. Grove appears to have allowed Goudie space to experiment with the new ideas entering geomorphology. Goudie commented that:

> I mean at Cambridge we were inundated with statistics … it was all quite exciting you know I mean there was Haggett and Chorley writing all this very new stuff on networks and so on - and so I got exposed to stats quite early on and I was hopeless at them but I'd seen these sand dunes with these amazing patterns and we'd all done work on the morphometry of rivers and I thought to myself aw - here we have dunes which have the same morphometric characteristics as rivers and that's kind of interesting - and I'd learnt about point pattern analysis - and so I used that – and applied it to dunes – but it was just a little thing on the side really (Interview with Andrew Goudie, October 2014).

As such, Goudie's first paper, entitled 'Statistical Laws and Dune Ridges in Southern Africa' (1969), explored 'the formal analogy between [Kalahari] dune systems and river systems'. Ordering the dune segments 'after the method of Strahler (1952)' it observed that 'these dunes ... obey Horton's law of Stream Numbers' (1969: 404). Additional evidence of Goudie's admiration for Strahler's adoption of techniques from hydrology and his concurrent emphasis on process can be seen in the closing paragraph of Goudie's paper:

> Just as morphological techniques have proved to be of great value in hydrology and fluvial geomorphology, it might prove useful to undertake further studies of the morphometry of dune fields to enable better comparisons and classifications of dunes to be made, and also perhaps to assist in the understanding of the processes whereby sand is transported to form dune chains and networks (1969: 406).

Such an approach would appear to belong to precisely the kind of 'statistical [or] … mathematical' work of which Grove had been wary (Grove, 2010: 265), however, Goudie recollected the sense of freedom he experienced as a research student:

> … both he [Richard Grove] and Chorley - they'd always be affable - 'everything all right?' - they didn't want to micromanage you - they didn't necessarily want to see you - - but they were always there if you wanted to see them. And Dick [Grove] always said you know you get on and do what you want to do - and so I chose Calcrete as it happened … so I got on and did it - and he was quite happy with that - and he gave me lots of help and had lots of contacts for me - - but erm we were never micromanaged (Interview with Goudie, October 2014).

Likewise Grove, understatedly, commented that the research trip to the Kalahari with Goudie had been 'quite a successful expedition'; he explained:

[34] Barbara Kennedy was similarly rueful regarding her experience of writing with her doctoral supervisor, Richard Chorley[34] (Maddrell 2011: 308)

> I'd made a map of the Kalahari from the air photographs with interesting landforms ... I - the paper I published on that, I did myself, and [Goudie] published separately on something to do with pans or dunes (Grove 2010: 221).

Given Grove's concerns regarding the appropriateness of the proponents of a new approach in the production of 'acceptable research work' at a 'post-graduate stage', it seems possible he may have been aware of an 'generation gap… opening' (Taylor 1976: 138) between himself and his student. However, further investigation reveals an interesting possibility for concepts, again understood as components of research objects, to withstand displacement temporally as well as spatially; that is to overcome a generation gap. Goudie comments regarding Chorley:

> I mean Chorley was brought up on the cycle of erosion in Oxford - then of course he went over to the States and came under the spell of Strahler and Schumm and people like that - erm I mean he was particularly close to Stanley Schuum you know they were great buddies and worked together and erm decided that erm what he'd done at Oxford was you know - old fashioned and wasn't going anywhere - and so he got into all this systems stuff - and you asked you know about this scepticism in Cambridge about Chorley's work - well - Sparks was basically a geologist by training and he'd done his original work on erosion surfaces in the south downs and on dry valley systems and how they'd evolved through time - - and then he became an expert nonmulusca (check) for reconstructing past climate - and Dick Grove likewise was interested in past climates - he and his wife - and Chorley certainly in the early days wrote off history as being important in geomorphology - it was really the operation of systems and the development of equilibrium forms - and I think Chorley and Grove were both sceptical about that as indeed did become Barbara Kennedy - I mean it's very interesting if you read Barbara Kennedy's stuff that she started off doing all this quantitative work and wrote the book with Chorley on systems and physical geography and she disowned it effectively later in life - yep and recognised of course just how important the history of landforms were and also the history of the subject you know you can't just write off what's been done before - you know - former people were longer relevant to our ideas today - er so that was interesting.

This comment is interesting when read alongside the memories of both Goudie and his mentor Dick Grove of discussions they shared during fieldwork. Interviewed by Paul Merchant, Grove commented:

> Grove: Yes, that was – no, Andrew was very observant and sensible. We had – we had some entertaining times in Botswana and I remember we – we used to have long talks there in the evening over camp fires about what could be done in physical geography and what books had to be written and so forth. I think it was – it was probably quite productive for both of us.
>
> Merchant: Do you remember what you said about what should be done within geography at that time?
>
> Grove: Oh, well, I – we would talk about things like that … one of the neglected topics was – was dust and its source and implications, and at that time there was only one book I think that we knew about on dust called – by a man called Blacktin [S. Cyril Blacktin1934 Dust Chapman and Hall] but these days quite a lot more interest has been found in the subject. So I don't know whether it – whether those conversations influenced both of us later on.
>
> Merchant: Do you remember anything else that you said about the nature of geography at the time and where –?
>
> Grove: I think we were both mostly interested in – in physical geography and in – and in the climatic history. And I think that's what both of us have been mainly concerned with since, though Andrew's been much more productive than I've been.
>
> (Grove 2010: 228-229)

When asked about these episodes during an interview conducted for this research, Goudie commented:

> Yes I mean we had I mean I did a trip with him around the Kalahari and we also went to Ethiopia so I mean we spent a lot of time - I mean some months together in the field - and it gets dark at 6 o'clock and it also gets cold you know and you sit around a fire and you have a glass or two of beer or wine or whatever you have - beer probably and not much of that - and erm - and we did talk about things - *like you know isn't history important* - and those sorts of things - so yes that's true that's true that's true - but I mean you talk things through in the field all the time - I mean that's how it works - not necessarily around a camp fire (Interview with Goudie, October 2014, emphasis mine).

These comments make more sense when read in the light of a reflection by Michael Church:

> A perversity of geomorphology in the late twentieth century was that, *at the very time when the global synthesis of plate tectonics was emerging, the discipline firmly set its course away from the large-scale problems of landscape history with which it had formerly been largely preoccupied. Reawakening to these 'global problems' began in the 1980s*, initially driven by geophysicists coming to recognize that erosional histories of the terrestrial landscape, and consequent isostatic adjustments, had to be incorporated into their tectonic mechanics. *An early review of the consequent renewal of interest in landscape evolution at the large scale was given by Thomas and Summerfield* (1987). (Church 2010: 274-275, emphasis mine).

It is important to note that Thomas and Summerfield, who produced the 'early review' were both graduate students of Andrew Goudie at Oxford. Summerfield himself echoes Church's comments when discussing the title of the British Society for Geomorphology's journal, *Earth Surface Processes and Landforms.* He comments: 'The naming of this journal squarely reflected [the mid-century] realignment' of geomorphology to 'the study of surface processes at small spatial and temporal scales'; he notes that 'the later addition of 'landforms' to the title [was] somewhat cosmetic'. For a time, the study of the history of landscapes was marginalised by the emphasis on process geomorphology; however, it did not disappear altogether. In Church's analysis, it appears that the subject of large scale landform history was dormant for thirty years: a true generation gap. It is only by attending to hidden histories of geomorphology, surfaced in qualitative interviews, that it is possible to see that concepts can survive a generation gap, re-emerging in future spaces of inquiry. Indeed, it is interesting to think about the role played by fieldwork, in providing a space where *conversations* could occur that might shape the spaces of future field and laboratory research. Latour argues that:

> I was struck, in a study of a biology laboratory, by the way many aspects of laboratory practice could be ordered by looking not at the scientists' brains (I was forbidden access!), at the cognitive structures (nothing special), nor at the paradigms (the same for thirty years), but at the transformation of rats and chemicals into paper (Latour 1986: 3).

Latour's analysis is valuable, however, access to 'scientist's brains' can be gained through interviews; the discursive remains a valuable aspect of both science and science studies. Further, 'paradigms' may turn out to be less homogeneous than they appear.

DEVELOPING EXPERIMENTAL TECHNIQUES IN THE INVESTIGATION OF ROCK WEATHERING

As discussed above, Goudie's early analysis of sand dunes was undertaken with explicit reference to Strahler. It might be expected that a similar emphasis would be found in Goudie's early work on rock weathering experiments; as seen above, in his first publication Goudie appeared keen to adopt techniques, until then primarily employed by fluvial geomorphologists, into other areas of geomorphological inquiry. However, the genealogy of rock weathering experiments is more varied and diffuse, with recognisable antecedents of contemporary experiments stretching back as far as the 1880s (Thury 1828: 180-192 cited in Evans 1969: 164).

Strahler defined 'molecular stress phenomena, more conventionally termed the "weathering processes" as being 'principally stress[es] set up by changes of temperature or physical-chemical changes' (Strahler 1952: 932). He detailed the types of shear stresses including those associated with (i) direct thermal stresses set up by heating and cooling of rocks, (ii) the development of ice crystals, (iii) the growth of salt crystals, (iv) absorption of water by colloids in the rock and soil, (v) contraction of capillary water films between the grains of a soil or granular rock, (vi) growth of plant rootlets in soil or rock, (vii) expansion of rock upon release of confining pressure, as a result of pre-existing elastic strain within the rock (Strahler 1952: 932-933). Early work by Goudie, Cooke and Evans (1970) seems in broad agreement with Strahler's viewpoint when they remark that:

> Disintegration of rocks by salts … might be accomplished by three principle processes: growth of crystals from solution, expansion on hydration, and thermal expansion (Cooke and Smalley, 1968). Crystals have been observed to grow against pressures up to 47 bars, and to hydrate against 63 bars, while much higher pressures are theoretically obtainable by crystal grown or hydration; rock tensile strengths are commonly between 20 and 200 bars. Despite a long history of research (Evans, in press), the experimental evidence is inadequate in respect of the relative importance of these processes, and of its relation to the range of conditions encountered in natural environments. We have initiated a series of experiments to explore certain aspects of these problems (Goudie et al. 1970: 42).

Goudie et al.'s emphasis on the small scale[35] causes of weathering processes seems to adhere to the principles of the 'new geography' emerging following (i) the work of Strahler in the 1950s and (ii) the evangelical work of Chorley in the UK. However, Goudie et al.'s early weathering simulation experiments at the Physiographic Laboratory (under a 'NERC research studentship' (Goudie, Cooke and Evans 1970: 48)) made no explicit reference to Strahler[36]; their methods employed resources with earlier roots.

In a review of work undertaken on 'Salt Crystallization and Rock Weathering' (1969) Goudie's collaborator, Ian Evans, showed that Strahler was not alone in adopting methods from the engineering sciences:

> Early experiments on the disintegration of rocks by salt crystallisation were made by engineers who found Na_2SO_4 to be a convenient and quick-acting proxy for ice. A method proposed by Brard was summarised by Thury (1828, pp. 189-192). 5cm cubes of rock are boiled for 30 minutes in water which has been saturated with

[35] Though note that Goudie is acknowledged as preferring what Summerfield termed 'macro-waffle' over 'micro-piffle' (Summerfield 2012).

[36] Likewise, an earlier 1968 paper published by one of Goudie's co-authors, Cooke, did not cite Strahler, though it is interesting to note that the latter is written in collaboration with an engineer (Cooke and Smalley 1968), given Strahler's early cross-appropriation of ideas from engineering (1952, see also 1992: 62).

> Na_2SO_4 when cold. They are then suspended on wires above dishes of this solution. Whenever saline efflorescence form, the cubes are immersed in the solution, and then suspended once more. Five days after the first efflorescence, the fragments detached from each cube are weighed, to give a measure of the susceptibility of the rock to the crystallisation of salts, and hence (hypothetically) of ice (Evans 1969: 164).

Several aspects of this experiment are recognisable: the cubes of rock, which as seen in chapter five, have become a hallmark of weathering experiments in the Rock Breakdown Laboratory at Oxford; the process of weighing rocks following a weathering experiment is also applied to this day. However, the transfer of such techniques to geomorphology took a considerable time; Evans remarked: 'unfortunately, many decades had to lapse before geographers or geologists became interested in such experiments … the first geographical experiments were described by Birot (1954)' (Evans 1969: 165-166).

Such a delay in the adoption of such techniques within geomorphology might be accounted for in Church's wry statement that 'in geomorphology the nineteenth century ended in about 1950' (1996: 150), a quotation approvingly cited by Chorley in his discussion of 'The Mid Century Revolution in Fluvial Geomorphology' (2008: 948). However, in rock weathering geomorphology, the change was mediated not by Strahler, but by a scholar somewhat dismissively described by Chorley as 'an eminent, *if distinctly traditional*, continental geomorphologist' (Chorley 1969: 613, emphasis mine). Chorley was reviewing the 1968 translation into English of Birot's *Le cycle d'erosion sous les différents climats* (1960) which in turn drew on a series of lectures Birot gave to the 1956 International Geographical Congress in Rio de Janeiro. Birot's reference to the cycles of erosion, like Strahler's doctoral work in the 1940s, remained couched in the terminology of Davisian cycles; Chorley argued that 'it is a pity that Professor Birot's work has taken so long to reach a wider English-speaking audience. A great deal has happened of relevance to climatic geomorphology since the year in which Schumm's work on the Perth Amboy Badlands appeared'[37].

CONCLUSION

This chapter began with an excerpt from field notes describing an experiment underway. It noted the similarity between stages of an experiment and Latourian descriptions: the process of using experimental techniques and equipment to distil and transform experimental materials, converting them into numbers, texts or images before eventually

[37] Note that Schumm was one of the 'firecracker group of graduate students' (Haggett 2002: 523) studying with Chorley under Strahler in Columbia's Geology department.

disseminating them through the production of scientific texts. Taking this as a point of departure, the chapter considered the displacement of research objects amid spaces of scientific inquiry; this was achieved through attention to the genealogies of specific techniques in rock weathering geomorphology.

It was noted that, within Daston (2000) and Latour's (2000) accounts of scientific objects as real and constructed, there is a symmetry between research objects (e.g. geomorphological weathering processes) and the wider configurations of techniques, equipment and people that produce them. Choosing therefore to focus on research techniques, understood as (i) a component of research objects and (ii) a hybrid of human and non-human agency, the analysis sought to (a) trace the fragmented genealogies of particular techniques as they have been cross-appropriated into geomorphology before proceeding in chapter six to an examination of the 'safe displacement' of these techniques into contemporary experiments.

In this way the analysis retained a traditional concern of ANT studies with the survival of scientific research objects as they are displaced across space, but chose to focus on different components of both the objects, and the spaces they were moving through: instead of focusing on research materials as a component of research objects, the focus was on research techniques; similarly, instead of focusing primarily on physical distance as a component of space it also focused on displacement amid disciplinary differentials.

The experimental techniques adopted by Goudie and his colleagues Evans and Cooke, had antecedents that sidestepped the 'revolution' for which, according to Schumm (2004: 671), Strahler was solely responsible. Instead it was Birot, a French geomorphologist described by Chorley as 'eminent if distinctly traditional' (Chorley 1969: 613), who opened the way for the cross-appropriation of these techniques into the study of salt weathering processes. The discussion showed, in relation to the development of experimental techniques for the study of rock weathering, that Goudie cannot, in any simple way, be considered as a successor to Strahler. Indeed, it is hoped that the study has avoided 'the dangers', identified by Goudie himself, 'of over simplifying the story of the evolution of geomorphology and of placing individual geomorphologists into convenient camps' (2011: 31).

CHAPTER 6: CRAFTING INTERDISCIPLINARITY II – TECHNIQUES IN THE CONTEMPORARY ROCK WEATHERING LABORATORY

INTRODUCTION: CONTEMPORARY INTERDISCIPLINARY RESEARCH AT THE ROCK BREAKDOWN LABORATORY

Chapter five examined how a consideration of the genealogies of specific techniques enables a fresh understanding of the way research objects withstand displacement amid the spaces of scientific inquiry. In particular, it critically considered Sherman's contention that Goudie could be understood as a successor to Strahler and Chorley (1996: 108), through a consideration of the historical 'cross-appropriation' (Spinosa et al. 1995: 3), or displacement, of research techniques into rock weathering geomorphology.

This chapter seeks to move forward another generation to examine how techniques cross-appropriated into geomorphology in the second half of the twentieth century continue to be reappropriated; it looks at the factors underpinning their 'safe displacement' into contemporary experiments in biogeomorphology and conservation heritage undertaken in the Rock Breakdown Laboratory at Oxford (the two specialisms of Heather Viles, who leads the laboratory). These displacements are examined with attention to the role of diverse actors including supervisors, colleagues and laboratory staff, equipment, texts, and software. In doing so, the chapter examines the role that a range of actors play in enabling the safe displacement of research objects amid the spaces of scientific inquiry. Once again this is a theme with ANT heritage, however, it is examined with respect to differing conceptions of both research objects and the spaces they traverse.

As seen in chapter two, Law explains that:

> Vessels are spatially or topologically multiple, inhabiting both Euclidean and network spaces. They are also homeomorphic within each of the forms of space, holding together physically in the one, and functionally or syntactically in the other. However, they move only within Euclidean space, remaining immobile within network space. (If there is rupture in the relations between the components in network space then they are no longer a network object.). At the same time it is this immobility within network space which affords their displacement within Euclidean space, that allows them to sail successfully from Calicut to Lisbon (2002: 95-96).

This thesis seeks to challenge this ANT account of space; chapter four saw the (field) spaces of scientific inquiry as potentially shaped by a dimension of physical distance, but equally as shaped by social, disciplinary, lateral, informational and temporal dimensions.

The chapter does not pretend to examine a case study which in some way represents a larger whole. As Cook et al. argue:

> … from the outset … we have doubted that it would make any sense to regard our proposed field locations – or indeed any set of locations we might propose – as parts which we could aim to piece together in order to arrive at an adequate description of some greater whole, the understanding of which would in turn contextualize and explain the several instances we observe (2009: 50).

The term 'case studies' could be substituted for ethnographic 'field locations' in the above quote, without any loss of meaning. Case studies do not form *representative* parts of a greater whole; it would be false to suggest that the materials presented here typify geomorphology more generally. Instead, it is the particularity of the case described that provides the impetus, and validity, of the study.

Returning to the vignette with which chapter five opened, it is possible to see a heritage conservation doctoral student, based at the Rock Breakdown Laboratory, visiting a field site following an interval of several days. As discussed in chapter five, the technique employed by the student had been developed, almost two hundred years earlier, by engineers (Thury 1828 cited in Evans 1969):

> Balancing on a ladder a doctoral student carefully gathered small rock samples, and made their way down to a small platform where they had set out a set of electronic scales, a rugged-looking, aging laptop and a small 'mouse-mat'-like pad … Reaching the bottom of the ladder the participant set the blocks on the small mouse mat pad. Wearing gloves as they worked, and taking care not to knock the samples, they first removed data-loggers – small metal rounds similar to large watch batteries. These had been fixed to one of each type of sample with Blu-Tack – which in damp conditions had become difficult to manipulate …. Each data-logger was placed into a 'reader' connected via USB to the laboratory's 'tough-book' – a rugged laptop that has been carted around the world by a globe-trotting group of researchers - designed for conditions far more extreme than this peaceful Oxfordshire heritage site, nestled in a small makeshift town, close to the regularly sounding bell of a nearby church. Once the temperature and humidity data from the loggers had been transfer to the laptop the rock samples were weighed, one at a time, using small battery powered scales (resembling kitchen scales). Finally, the awkward process of reattaching the data-loggers began, using Blu-Tack, before the samples were returned to their original places, out of sight, on a platform at the top of the structure (vignette reconstructed from fieldnotes).

An awareness of the historical genealogies of particular techniques adds depth to what appears, in the course of participant observation, as a simple transformation. The accretion of the activities of previous researchers, and more particularly, the development of a laboratory as a repository of research equipment and techniques, significantly

constitutes contemporary practice and associated scientific research objects[38]. This past activity is something that Latour briefly acknowledges, but does not investigate further in his study of Amazon scientists. He comments:

> Remove both maps, confuse cartographic conventions, erase the tens of thousands of hours invested in Radambrasil's atlas, interfere with the radar of planes, and our four scientists would be lost in the landscape and obliged once more to begin all the work of exploration, reference marking, triangulation, and squaring perfomed by their hundreds of predecessors. Yes, scientists master the world, but only if the world comes to them in the form of two-dimensional, superposable, combinable inscriptions (Latour 1999a: 29).

Building on this analysis, this section draws on participant observation and interviews with contemporary members of the Rock Breakdown Laboratory at Oxford. It examines the appropriation of specific techniques within contemporary experiments in three sections, focusing on the role of (i) people and equipment (ii) literature and (iii) software respectively.

DEVELOPING TECHNIQUES I: FIELD EXPERIMENTS IN ROCK WEATHERING - THE ROLE OF PEOPLE AND EQUIPMENT IN SAFEGUARDING THE DISPLACEMENT OF TECHNIQUES

The vignette above described research undertaken by a researcher whose previous training had been in restoration and art history, followed by graduate work in conservation. In the course of a repeat interview the participant commented:

> Participant: My background is totally different. I'm a conservator but I'm getting access to science. I couldn't study that if not in England – because I would have my doors closed for that. So yeah I learned … perhaps here [the laboratory leader] values your knowledge as a conservator – but in other countries … they thought that science is the base – so you have to develop your thesis based on that. So this is why, you know, I cannot be a scientist. So I'm learning what I need to get results. But I can't consider myself a scientist at all.
>
> Ethnographer: But would you consider your work to be scientific?
>
> Participant: Yes – I consider my job to be conservation science – could be. So it's like my work is based on science with an application to conservation, but I am not a scientist.
>
> (Interview July 2013)

[38] However, as the discussion above illustrates, the term genealogy incorporates a cautionary note: attention must be paid to the fragmented nature of lineages (Sherratt 2006) in order to avoid replacing one neat and linear account, of a translation from field through laboratory to text, with another equally neat account, of the displacement of techniques through history.

While observing this researcher at work in the field, there was an opportunity to explore how an almost two-century old methodology had made a safe passage into the hands of this researcher. Asked about the design of the research project, the research student commented:

> When I designed it, mostly it was [my supervisor]. Yep it was [my supervisor]. Well - [my supervisor] has been working in this type of research for twenty years at least - and has a really particular way to work. And we all follow this way. For example I was in a conference and someone asked me what I was doing. And I said I have all these <inaudible> stones. They asked 'ok who is your supervisor?'. And I said [the name of my supervisor]. They were: 'I knew it'. <Laughter>
>
> (Transcribed recorded conversation - December 2012)

In an interdisciplinary setting an individual researcher may not initially possess in-depth training in particular methods; however, the 'safe displacement' of techniques into the fabric of a particular research project remains essential if the research is to stand up to scrutiny. In these circumstances the network of actors sustaining the safe transfer of techniques may be dispersed within a wider network, incorporating colleagues and, in this case, the doctoral supervisor of the researcher.

Alongside the role of the supervisor in supporting the adoption of particular research techniques the researcher emphasised the role of colleagues and laboratory technicians, also based at the Rock Breakdown Laboratory:

> Participant: I was quite worried the first time I came here and I told [my supervisor] 'look I have no idea about science' - no idea no I mean I studied science at university for my degree but how to produce –
>
> Ethnographer: experimental?
>
> Participant: Yeah exactly - and she gave me some books - kind of really basic books like lab techniques - how to measure in a balance … this kind of things - and it helped me a lot to see people doing that in the lab - also [the laboratory technician] is always there to help and she is used to having a lot of students with different backgrounds - so in the beginning you feel really strange saying sorry I don't know how to do that but in the end you see that a lot of people ask help so in the end you say ok it's fine - you can teach me to do that and she's quite helpful - she's quite willing to help you and it's good. The thing is I can't say I'm a scientist because I don't know - I don't know all the scientific methodology - but I learn the ones that I need for -
>
> Ethnographer: for this particular work -
>
> Participant: Yeah - so if I need three or four techniques it's the ones that I learn
>
> (Interview July 2013)

As well as other human beings working in the laboratory, the research student identified the role of equipment as a determining factor in the development of a research methodology:

> And also because the equipment that we have in the lab is the same - all of our studies have the same kind of - all of us look at colour - because it's the equipment that we have in the lab.
>
> (Transcribed recorded conversation - December 2012)

Asked again about the role of equipment in research design the researcher commented:

> Participant: The thing is erm - basically what I do is adapt the equipment we have in the lab and also the experiences of people that study in the lab - to my research question - - so in my research question it's like I want to know - I don't know the changing colour it's like - ok what equipment is available in the lab - so I use that equipment. So - erm - I don't know if I answered your question?
>
> Ethnographer: Yes - you do. Did the availability of equipment ever change your research questions or open up a different research question?
>
> Participant: I think about the research question knowing about what equipment was in the lab - so it's like which equipment do we have in the lab - we can measure colour, weight, … I can use these things to - as a proxy for my research question - yes then I will use it - the ones that I can't use it I don't use it - but I first - I look at the equipment that we have - the purpose of the equipment - what it's useful for - then I thought that if it could be useful for also my research - so it was that way around.
>
> (Transcribed interview – July 2013)

The role of equipment in research has been emphasised elsewhere in the science studies literature as forming part of the configuration that constitutes scientific research objects (Daston 2000, see also Hacking 1992: 30). However, if techniques are understood as a hybrid of human skill and material equipment, the participant's comments, above, underline the role equipment plays in sustaining the displacement of research techniques.

A process of cross-appropriating equipment is actively pursued by members of the laboratory at all levels; as new equipment is added, new research objects form and experimental spaces open out. The laboratory functions as a repository for this equipment and the associated techniques, as well as providing an academic home for the scholars. Again the relationship between field and laboratory is mediated via these pieces of equipment, with an on-going 'n-way' (Star and Griesemer 1989: 389) traffic moving back and forth between the spaces of field and laboratory. In this way the spaces of laboratory and field are folded into one another and are mutually sustaining, with field often functioning as an extension of the laboratory (via the displacement of equipment

and techniques) and laboratory often functioning as an extension of field (as, for example, field data, or weathered materials are introduced into laboratory experiments and simulations).

The process of incorporating new techniques, or applying existing techniques to new experimental configurations, is challenging. For example, one participant explained ongoing work in the laboratory testing equipment to ensure that results can be trusted. Discussing the size of the blocks of stone used within the experiments the research student commented:

> I select them – it's kind of a strange size … I selected because one of the equipment that I needed … that needed three times longer than wider … so I have … so they needed to be three times if I wanted to use that equipment – because I was talking with different people in the department and they were telling me that it was quite reliable – the equipment - so they advised me to use that one (transcribed interview – July 2013).

While the participant had made a choice of size based on the requirements of particular equipment, in the course of their research they discovered that this could lead to unreliable results:

> But during this year people in the lab have been … have done some tests – and it is not published yet - but she has told us that probably it's too small for the measurements to be reliable for this – so probably I couldn't use the results for that – but this was something I didn't know before doing it – so when I selected the size of my samples - it was regarding equipment … requirements and also the practical point of view … it has to be not so heavy and easy to hide (transcribed interview – July 2013).

This demonstrates the role of colleagues in the laboratory in ensuring techniques are applied 'safely', not only in terms of generating reliable results, but also in maintaining a negotiated cooperation of human and non-human agents, satisfying the expectations of a community of scholars and according with the researcher's own judgement concerning the reliability of their practices.

The role of people and equipment in underpinning the transformations associated with experimental practice also has, layered into it, the role of texts. As seen in relation to the spaces of fieldwork, in chapter four, texts are not understood simply as the 'destination' (Latour 1999a: 56) for the series of transformations involved in experimental practice. Instead, texts are understood as constitutive of the configurations underpinning the cross-appropriation, or safe displacement, of techniques. For example, on two separate occasions the research student linked their work to a study undertaken by the laboratory leader, as seen in the following dialogues:

(i) Ethnographer: Did [your pilot studies] change anything in your research design?

Participant: Er no - it was pretty – I - because many people have to use this kind of equipment in different environments - so yeah - if Heather [Viles] for example used a lot of pieces of equipment to see how stone weathers in the Namibian desert so it's like - I mean in my case my DPhil is also a weathering stone study but under different environmental conditions - but it's really really similar to other projects.

(Transcribed interview – July 2013)

(ii) Participant: For example, Heather has a really famous article - she put all these small stones in the Namibia desert - to see kind of temperature change - [laughter, with affection] - so it's kind of these small little samples placed around.

(Transcribed recorded conversation - December 2012)

Finally, the research student situated their work in an overlap between geomorphology and conservation, attributing ownership of the techniques to geomorphology, and ownership of the application to cultural heritage:

Participant: When I was doing my lit review I looked at some scientific papers but mainly I was looking at conservation papers … I was inspired by the problems of site managers - I didn't look at it through the scientific point of view.

Ethnographer: So in a way your research questions were formed in relation to a literature - in an interdisciplinary kind of -kind of literature - a literature coming from outside geomorphology - or outside of heritage conservation. Is it outside heritage conservation as well?

Participant: I'm using geomorphological techniques to study a cultural heritage question - they don't overlap always - in this case yes because in this case I'm studying environmental conditions.

(Transcribed interview – July 2013)

The role of supervisors, colleagues and equipment in sustaining the displacement of techniques has been examined here, together with an initial look at the role of literature in sustaining such displacements. This analysis has incorporated some provisional comments on the ramifications of these factors for the relationship between field, laboratory and text. Some observations were also made on the relation between physical models and numerical models. The next section analyses the work of another member of

the laboratory; this picks up the discussion of the role of literature, tracing debates in the literature that accompanied the historical cross-appropriation of techniques employed in contemporary experiments.

DEVELOPING TECHNIQUES II: LABORATORY EXPERIMENTS IN ROCK WEATHERING – THE ROLE OF LITERATURE IN SAFEGUARDING THE DISPLACEMENT OF TECHNIQUES

Building on the adoption of Brard's techniques for the experimental investigation of salt weathering, Goudie, together with Cooke and Evans, was involved in early experiments employing new pieces of equipment: environmental cabinets programmed to produce variations in temperature and humidity. First used by Kwaad (1970) in Holland, Cooke published a paper on the first application in the UK at Bedford College (now Royal Holloway), London in 1979. Cooke's analysis built on simpler laboratory investigations of weathering carried out at the beginning of the 1970s with Andrew Goudie and Ian Evans (Goudie, Cooke and Evans 1970) and a further paper by Andrew Goudie in 1974. Cooke describes the ability of the cabinet to replicate changing temperatures and humidity, though notes the cabinet's limitations in terms of the type of heat and resulting uniformity of temperature[39] (1979). Andrew Goudie's help during the course of the experimental work and in the production of the text is acknowledged by Cooke (1979: 358) and indeed Goudie is the lead author of a response to a critique of the methods developed by Cooke and himself (Goudie and Cooke 1983 in response to Smith and McGreevy 1982). A similar environmental cabinet was purchased by the laboratory at Oxford, in the early 1980s; Andrew Goudie commented:

> …when i was made professor of geography here in 1984 - I had as it were a bride price and I was told that up to a certain limit I could have something to help my research which is when we got the environmental cabinet and Ron Cooke got one at about the same time in London and he employed one of my graduate students - a doctoral student who'd finished his doctorate … and they did work of a similar type as well - and now of course it's a hugely used piece of equipment (Interview with Andrew Goudie, October 2014).

Indeed, this equipment, and associated techniques, is still in use today; for example, the following vignette (drawn from fieldnotes produced during participant observation at the

[39] An issue which has been more recently tackled through a change from 'oven' heating to 'lamp' heating, following a critical paper by Smith et al. (2005). One participant commenting: 'there's also one [paper] about the way that blocks are heated - - I might have mentioned that before - - so you can heat them in an oven - so that heats by convection - it's an indirect heating - whereas there's a paper by Warke and Smith - where they said ok most people are using this oven system - -and does that replicate when it's heated by the sun - and you get very different responses - - so everyone now would use lamp heating - because it was accepted that the method that things were being done in that way were not particularly relevant to the real world - - so everyone uses lamps' (Interview with participant – July 2013).

Rock Breakdown Laboratory, Oxford) describes their adaption into a contemporary biogeomorphological experiment examining bioprotection and the weathering of coastal defences:

> The geomorphologist handled the rugged blocks delicately - touching them with the tips of a balanced hand, his manner somewhat resembling that of an archaeologist examining a valuable artefact, or a socialite cradling their teacup in an upmarket bistro. And yet he would subject them to a gruelling routine – moving them at six hour intervals between a tidal simulator (constructed from two large polythene crates, a pump and salt water) and the temperature and humidity controlled climate of an environmental cabinet primed with cycles of data from a meteorological station. As they were removed from the tidal simulator temperature probes were inserted into small holes drilled from beneath; this allowed the geomorphologist to examine the effect of marine organisms, which had colonized some of the samples, on the temperature differential across a small depth from the surface of the rock – a key predictor of weathering. The samples were then set out on trays, surrounded by a layer of small polystyrene balls (similar to those found in a child's beanbag) – this is an attempt to insulate all surfaces except the top surface (as would be the case if the sample was part of a much larger structure such as a sea defence or harbour wall) before being placed on a shelf in the environmental cabinet. The latter, resembling a large glass-fronted oven, was an aging component in the laboratory, and one with solid vintage – Heather Viles had used it in the course of her own PhD work in the early 1980s and Andrew Goudie had also made use of the equipment in his work on salt weathering. During the period of the experiment, the age of the equipment was showing and it was in need of repair; this lead to some frustrating delays for the researcher (participant observation 2012).

This section focuses on the role of academic literature throughout the development and contemporary application of such techniques. It begins in 1982 with a consideration of a somewhat antagonistic exchange[40] in the pages of Geografiska Annaler, starting with Goudie and Cooke's co-authored response to a critical paper by Bernard Smith and his colleagues, which itself was written in response to Cooke's publication of their developing experimental methodology:

> The paper by McGreevy and Smith (1982) on the design of simulation experiments to assess the role of salt weathering in hot deserts contains a series of criticisms of previous work. Some of these criticisms are valid (though not necessarily surmountable) and recognised by those who did the work, while others seem to be themselves in error or to be misleading … Plainly earlier experimental work can be greatly improved, and it is important that we consider with care the various criteria that we employ in experimental simulations of laboratory processes. However it is also important to read earlier literature with equal care

[40] It is interesting to note an equally barbed exchange a decade earlier, when Goudie's own tail was in danger of being salted by an earlier generation of scholars. Writing, albeit alongside Professor Edmund Gilbert, from Hertford College, he crossed swords with Chorley's former undergraduate tutor, Robert Beckinsale (1908-1998 – lectured at Oxford from 1944 to 1975) (Beckinsale et al. 1972).

> lest old snails place salt on the tails of their younger colleagues. (Goudie and Cooke 1983: 296 and 298).

This rebuke was countered by McGreevy and Smith:

> That Goudie and Cooke should comment on our paper has come as no surprise. Disappointingly, however, their discussion appears to constitute an emotive rather than an objective response … Towards the end of their comment Goudie and Cooke accuse us of failing to do our homework by omitting to 'read earlier literature with equal care'. As we stated earlier, we were aware at the time of writing of much of the literature they cite, but did not consider it of direct relevance to studies of *rock* weathering in *hot* deserts – perhaps they might be advised to take equal care in reading the titles of papers! … More seriously we do object to being criticised for omitting literature which was not available at the time of submission … and in one instance … had not been published! A salty retort to older colleagues' final sentence is scarcely necessary. We conclude by emphasising that laboratory simulations ought to be conducted with an awareness of possible exaggerative effects, in terms of rock weathering behaviour, which can be introduced into an investigation by use of unnatural weathering conditions (McGreevy and Smith 1983: 298 and 301, emphasis original).

This debate continued, though perhaps in more constructive terms; reflecting on those early exchanges, Smith and McGreevy, together with co-authors Warke and Kane commented in 2005 that:

> Some 20 years ago, two very junior researchers had the temerity to suggest that results from simulation experiments of salt weathering in hot deserts could owe more to experimental design than conditions experienced by rock outcrops (McGreevy and Smith 1982). Senior colleagues quickly took them to task (Goudie and Cook 1983), especially over the relevance of salt and temperature regimes that had been employed in tests. Undeniably, however, since the early 1980s, numerous refinements and, hopefully, advances have been made in the use of simulation experiments to understand salt-weathering mechanisms and the factors that control them (Smith et al. 2005).

These seemingly antagonistic debates between colleagues provide a robust opportunity to test and safeguard the displacement of techniques into a new disciplinary space. However, such barbed exchanges between 'old snails' and their 'younger colleagues' (Goudie et al. 1983) appeared long forgotten when an early career researcher, and member of the rock weathering laboratory, commented (prior to news of Smith's death in 2012) that:

> If I was to write a paper about …[environmental] cabinet work … and if I was to send that for review and Bernie Smith might very well be one of the people I suggest to review it - if he was to come back and blow all my methods out – and say don't do it this way you should have done it this way - erm that could be quite - you know not very nice. But the way I know Bernie works he wouldn't do that … so it depends who. So it would certainly be constructive criticism. … I would hope that any reviewer questioning methods would do it in a constructive way – erm –

> but I think – it's very important - it's all part of the review process – to question methods (transcribed interview with research participant).

These examples show the role of literature, throughout the last thirty years, in attempting to ensure a 'safe' development and ongoing reinvention of techniques. It is interesting to note both the antipathy underlying the early communications and their relatively direct tone – with protagonists addressing one another in an elaborate conversation played out in the particular space of an academic journal.

The role of literature in rendering techniques mobile among disciplines, not least in the case of Strahler's (geomorphological) appropriation of Horton's approach (engineering / hydrology), merits further investigation. In the course of the study the ethnographer has, for example, seen a volcanologist pull from their bag a clutch of archaeology papers as they explained their struggle to appropriate three dimensional imaging techniques (see Fig. 9); similarly Viles, discussing Goudie's work on the Schmitt Hammer, commented that:

> Heather Viles: Just to give you a more tangible example just to go back to the Schmitt Hammer … so as you know that came from concrete testing and so in the early days when Andrew Goudie was using it in I guess the 1970s - there was no comparable data for rocks so all you could you first of all was say oh well that's interesting - limestone is this value - 40 whereas concrete was actually only 35 or whatever - and then you'd go to granite and think good lord granite is now you know hugely much higher than the values that come from other things and so I think sometimes we do we take the numbers from equipment in other fields - we also take the analysis techniques from other fields - so when we were searching for how to quantify roughness - surface roughness - we would look at erm not necessarily the data collected by other scanners but how they'd analysed that in other fields … and I can't give you any example now of where that might have come from … but it might have been something like metals testing or something where it's important to measure the roughness - so - so there is a transference not only of data but analysis techniques of that data.
>
> Ethnographer: Does that happen primarily through texts or through conversations?
>
> Heather Viles: Er I would say largely through textual things - through looking at papers. I think that's the most important thing so you know you would often find … him reading something about concrete and you'd think why are you reading that - and he was reading it just to get the method.
>
> (Transcribed interview 12 December 2012)

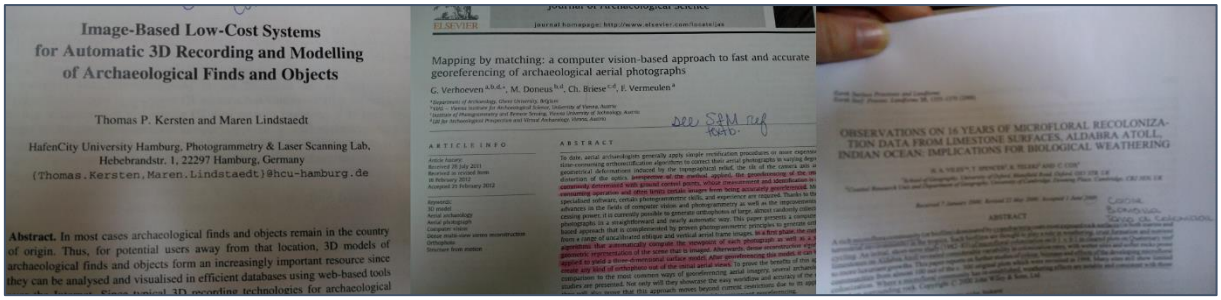

Figure 9: Cross-appropriation of techniques - A volcanologist pulled a selection of archaeology articles from their bag as they explained their efforts to adopt 3-D imaging techniques, already explored in the archaeology literature, for a study of environmental hazards

SUMMARY

The importance of publishing has increased dramatically since the 1950s (Cooke 2012; see also Steel and Watson 1972: 145; Cooke and Robson 1976: 94; Munton and Goudie 1984: 30; Heffernan et al. 2013: 271), as seen in the discussion above concerning the reticence towards publishing still present among some at Oxford at the time of Goudie's arrival in 1970.

DEVELOPING TECHNIQUES III: QUANTIATIVE DATA AND STATISTICS

> Statistical analysis is not an end in itself; it is a versatile and powerful tool for use in an intermediate stage of certain quantitative investigations … Statistical methods may be put in the same category as the Brunton Compass, the petrographic microscope, the spectroscope, or the X-ray camera (Strahler 1954: 1).
>
> Statistics is one of the bases by which we now understand the world; it is our reality. If so, then it becomes necessary to illustrate the processes by which the construction of that reality occurs. It is here that a social and cultural perspective on quantitative methods is useful. It can be the crowbar for prising off the lid of the black box of statistics and taking a peek inside (Barnes 1998: 221).

Statistics initially found its way into British geomorphology via Strahler's student Richard Chorley[41] and Chorley's student Peter Haggett – the latter remembered by Richard Grove as producing tripos papers that were 'more or less publishable as they stood' (Grove 2010: 141). Strahler 'advocated the development of mathematical and statistical models in which geomorphic processes and landforms are treated as manifestations of molecular and gravitational shear stresses acting on earth materials' (Rhoads 1992: 433). This led to a 'veritable explosion of empirical studies' occurring in geomorphology in the 1950s (Rhoads 1992: 433).

[41] Though Steel and Watson point out that at the time of the Madingley lectures of 1963 had already been pre-empted by Gregory's text on statistical methods in geography (1972: 144).

According to Nigel Thrift, it was Chorley's student Haggett, whose 'influence in spreading the word about numbers was possibly greater than anyone in the discipline [of geography]' (1995: 381-382 cited in Barnes 1998: 217). The application of statistical techniques in geomorphology was becoming 'black-boxed' by the mid-sixties (Barnes 1998: 217) with a network of human actors, encompassing 'a multiple geographical elite' (Thrift 1995: 380 cited in Barnes 1998: 217) entwined with 'non-human inputs such as computers and desk calculators' (Barnes 1998: 217). It was in this context that Andrew Goudie moved from Cambridge to Oxford, to take up a post as 'departmental demonstrator in statistics and oceanography' (Goudie 2012). Here the 'hum of calculating machines' (McCarty 1979: 123 cited in Barnes 1998: 217) was conspicuously absent; as seen above in 1970 the department possessed just one calculator[42] with a square root key (Goudie 2012). Goudie, together with Marjorie Sweeting and others, was to spearhead a move towards greater emphasis on quantitative methods[43] (Goudie 2012).

THE CONTEMPORARY LABORATORY

Barnes argues that 'by the mid-1960s … the sides of the black box that was to be quantitative geography were beginning to be nailed shut. The network was becoming stable' (1998: 217). Part of the process by which this occurred in Oxford is described, briefly, above; however, for at least some members of the interdisciplinary team of

[42] See Unwin 1974 for an analysis of quantitative techniques using 'possession of computing hardware and the ability to use it' as a proxy for 'general levels of numeracy) (p. 200). Drawing on data gathered in 1972 and 1973 Unwin notes that 'the average department has about 20 calculators of all types, about half of which are likely to be old fashioned mechanical and electromechanical devices and the remainder recently-purchased electronic ones' (p. 200). However, this number is of limited value since (i) the cost of calculators reduced rapidly in the course of the 1970s which may have accounted for the 'recently-purchased electronic' models - therefore the average data for geography departments UK-wide in is not immediately comparable with the equipment available when Goudie arrived in 1970; (ii) it is not clear what kind of calculator (mechanical, electromechanical, electronic) would have a square root key and it is therefore difficult to compare Oxford's School of Geography to other departments based on this data. Goudie's comment does highlight the importance of calculators as a technology increasingly available through the course of the 1970s; this finds resonance in a quote from *Laboratory Equipment Digest* in March 1973: 'No one should doubt that the electronic calculator is now as familiar and vital a piece of laboratory equipment as the Bunsen burner and the test tube' (cited in Deeson 1974: 419).

[43] Further research is required to examine the extent to which statistical analysis was employed in the interpretation of data generated in this shift to quantitative investigation of process, as well as to understand how such statistical analysis is similar to, or differs from, the Strahler-influenced approach to statistical analysis employed by Goudie in his first paper on sand dunes (1969). Similarly, additional research is required to understand what kind of training Goudie himself had in statistics, the influence of his Cambridge education on this, whether he was recruited from Cambridge specifically to bring this statistical emphasis across to Oxford (as his initial job title appears to have indicated) and whether the role of statistics in his interpretation of data produced through physical laboratory models of weathering processes has changed over the course of the last thirty to forty years, as he worked across a broad range of research interests.

geomorphologists and conservation heritage researchers based at the Rock Breakdown Laboratory in Oxford, statistics remains something of a foreign language:

> It's like oh my god - I'm so scared of the point when I have to do statistics - and analyse with all of these numbers - for that I'm kind of scared.
>
> (Interview December 2012)

> So I mean I've always shied away from stats and maths and all that sort of thing – and I think – I don't want to say all – but probably a majority of geographers would probably tend to stray away from maths and statistics.
>
> (Interview July 2013)

The first of these comments was made by a laboratory member whose limited familiarity with statistical techniques owed to an interdisciplinary background – with undergraduate training outside geography or the geosciences. In the second case, although trained in geography and the natural sciences in another U.K. university, relatively limited undergraduate training had been undertaken; indeed, as a doctoral student (also at another U.K. university), and like Strahler, the person concerned had audited courses designed for other scientific disciplines in order to glean the relevant techniques. This provides evidence of 'multiple … indirect, fragmented … lines of descent' (Sherratt 2006: 136) associated with the genealogies of techniques.

Echoing early misgivings of Haggett, who commented in 1965 that 'numbers were accumulating almost too fast' (1965: 277 cited in Barnes 1998: 219), one member of the laboratory, who had returned to academic work following professional experience in heritage conservation, commented:

> People are very focused towards data – whereas I'm focused towards producing solutions
> But there's no way around it
> I need to do that in order to be able to use the labs, to conduct research
> There's a tension here…
> …Recently I had to think 'right I have to choose and either be an academic or a practitioner'. At this point I have to be an academic.
>
> (Interview – April 2013)

Similarly a second interviewee described a sense of being overwhelmed by the production of data:

> The data is so difficult – in terms of environmental data – because I have – I have this many spreadsheets – they are unbelievably difficult – thousands and thousands and thousands of numbers. So I told [my supervisor] … I don't know what to do with this data – so [my supervisor] told me that [they] are going to help me with that.

(Interview July 2013)

However ‘black-boxed’ statistics had become, it had not become second nature to all those required to employ it. The need to use statistics was acknowledged, both in the production of evidence supporting strategies for environmental management, and, crucially, in order to be published:

> Probably in the vast majority of geomorphological papers that are published – so in Geomorphology journal or in Earth Surface Processes and Landforms – that would be the two main ones – most of them will have some statistics in there … So to get a good paper you need to have some strong statistics to back up what you’re trying to say … Also in terms of applying some of the work that geographers do - so more and more - as I think we've said before - there's more and more push for people, researchers, to be doing applied, useful science - making decisions about how to manage the environment - and as soon as you start having to make decisions about actually how you're managing things then need to have strong evidence that you can you know back up - and statistics is the way to do that I suppose.
>
> (Interview July 2013)

It is interesting to note how the need for statistical techniques to be employed in the production of texts provides a driver for the incorporation of statistical techniques at an experimental level. It should also be noted that there are likely to be intradisciplinary differentials[44] surrounding the degree of incorporation of statistical techniques, with different sub-disciplines of geomorphology demonstrating varying degrees of proficiency in their application.

[44] Such an explanation for an emphasis on reductive quantitative techniques within geomorphology was provided by one interviewee; they conjectured that the dominance of such techniques may be rooted in the peculiar requirements of fluvial geomorphology and the influence of the later within geomorphology.

HOW MUCH TO KNOW?

Here a key question, and recurring theme, emerges concerning the employment of statistical, and other interdisciplinary techniques: how much do you need to know?

> Ethnographer: And is it the same with a statistical test or do you – is it something that you can lift the lid on and kind of see the mechanism at work?
>
> Geomorphologist: Erm – that's a good question and – I think you can approach statistics in both ways. I think there are people who need to understand exactly what the test is doing – and what all the numbers in the equation mean – and why they're where they are – and how they relate to each other – so they really need to understand the kind of ins and outs of the test – erm – and I think ideally that's the way that statistics needs to be taught - and kind of looking through the lectures that the undergraduates here get I think that's sort of the way that they try to get people to understand - because if you understand why you're dividing such and such by such - it becomes much more intuitive - and you're much more inclined to be using things correctly. Erm, however, I am sure there are lots of people – and I'm guilty of this sometimes – is that sometimes you just want an answer – and I think it's very easy to kind of - not worry about what the test is doing – you just kind of pick the test – make sure that the parameters are met – at least – that certainly has to be done – erm and then you just kind of plug all the numbers in and press go, and get your number out. And it's significant or not. And it can be as simple as that – erm – and so you might argue well do you need to know the ins and outs of the equations – what all the equations mean – well maybe not - particularly with all the computer programmes.
>
> (Transcribed interview – July 2013)

The interviewee is somewhat equivocal about the need for researchers to deepen their knowledge of statistics. This touches on an important aspect of interdisciplinary research: knowledge may, instead of being concentrated in the minds and hands of individual researchers, or 'experts', be more diffusely available within a network. A researcher may be more reliant on their supervisor for guidance in statistical techniques, as in the case of the research student (above) who described how they would need their supervisor's help in beginning to analyse what they were experiencing as an overwhelming accruing dataset. Or, as mentioned towards the end of the quote above, computer software may incorporate knowledge. In another section of the same interview, the potential for technological support in generating statistical analyses was discussed:

> Ethnographer: Do you use sort of – Excel – or do you use any other…?
>
> Geomorphologist: Erm, I use Excel for sort of the basic stuff and there is a kind of extension package in Excel that you have to download … there's a little button you

click on – the statistics add on or something - and it just lists all the different tests – and you just basically pick them –

Ethnographer: Ok

Geomorphologist: The danger with Excel is that there's very little explanation of what the tests are, of what the assumptions are, and so you can easily do - an ANOVA on data that doesn't meet the requirements – because Excel doesn't tell you that it's wrong – it still lets you do it and it will give you a result –

Ethnographer: Yes

Geomorphologist: So it – Excel does require a bit of sort of caution – I think. Erm – there's another programme that we have on the department network called Sigma Plot. This was new to me – but it's actually very very good – because it gives you much more – it gives you much more explanation – so I think there's even – I think there's – so there's this thing called 'the advisor'

Ethnographer: Ah ok

Geomorphologist: Erm – you know so it actually tries – it makes you think about what data – so what do you - what do you want to do – do you just want to describe it? If you're comparing two groups - or if you want to measure the strength of association – so for me to pick that it would sort of suggest that - - so it makes you think what categories you've got – and all this kind of stuff – so you can click through it – and then it'll give you a suggestion – about what you need to do … and then it can kind of guide you through it. So this programme's quite good in that it can sort of hold your hand while you go through it

Ethnographer: Yes –

Geomorphologist: Erm – and the same with the outputs of the test – so as well as helping you pick which test you need to do – another challenge that some people have is that you do all these – what you might see as complicated maths – and then you get your number at the end and it's p equals whatever it is – and you think 'what on earth does that mean' – erm – but this spells it out – and so Excel just gives you that number – it doesn't tell you anything else – whereas this will say erm you know your p value is such and such - using this test this means that category A is significantly higher than category B – so it really puts it into kind of plain English - Erm – so I think there are certainly technological programme advances that are helping that perhaps haven't done statistics – to really make sure that they're doing

Ethnographer: To go through it kind of safely in a sense?

Geomorphologist: Yeah – Definitely – and that can only be a good thing really.

(Transcript of Interview – July 2013)

As well as the use of computer equipment in statistical analysis, the geomorphologist highlighted, once again, the role of talking to colleagues, particularly when undertaking interdisciplinary work:

> Geomorphologist: But I would always advise – as I say I'm kind of helping a couple of undergrads with their dissertations and I've said to them I'm very happy to advise on statistics and try and help you design your experiments to do the sorts of tests that I know how to do – but I've always said you know I'm not a statistician and so I would always advise going to a specialist – particularly those students who are doing sort of ecology stuff – there's nothing more valuable than sitting down with an experimental ecologist – and talking through your experimental design with them - with kind of stats in mind – because they really know. That's quite…
>
> Ethnographer: From quite an early stage – in a sense?
>
> Geomorphologist: Yes – I would certainly recommend people doing that - because you know once you can really hone down – as I say – what your question is – and how you want to test that question – how you want to test your hypothesis statistically – then the kind of design of the experiment almost falls into place – but you must get that bit right.
>
> (Transcript of Interview – July 2013)

The application of statistical techniques has taken hold in geomorphology over a period of more than half a century. However, this research suggests that the knowledge of how to use and apply statistical techniques may be 'held' by a network of actors in a diversity of ways: an individual may not have an intuitive grasp on the application of a particular technique, grounded in thorough training during or before undergraduate work, even if such training may be considered ideal. The application of statistical techniques may also be incorporated into the network in the form of computer software, and in the form of a network of scholars whose specialities can be drawn on, guiding the development of a particular research project.

Statistics, as seen above, represents a technique that has been incorporated into geomorphological investigation over a long period of time. However, such cross-appropriations are not without hurdles. For example, in 1954 Strahler was extremely optimistic in his view of the potential application of statistical techniques to geomorphological research, commenting that:

> The geomorphologist who seeks to go beyond mere verbal descriptions of scenery to a rigorous quantitative approach will invariably be taking limited samples from vastly larger populations. Statistical methods offer means of developing unbiased, objective sampling procedures and of extracting the maximum possible information from the numerical data thus recorded. Testing procedures guard against the use of unreliable data as the basis of drawing unwarranted

> conclusions. On the other hand, the statistical tests may give increased confidence in stating conclusions where significant differences and trends can be demonstrated. As quantitative analysis opens up vast new areas of study of land forms and the processes involved in their development, the methods of statistics will become an indispensable part of geomorphic research (Strahler 1954: 24).

Reflecting on the use of statistics forty years later, however, he was more circumspect[45], remarking:

> A new spectre now looms over the horizon of science in what is now identified as The New Philosophy. Science philosophers - some of them, at least - have turned from telling scientists how they ought to carry out their business to observing how they actually do it. In the language of philosophy, the schools of logical positivism and logical empiricism (purveyors of a normative philosophy) have given way to a naturalistic philosophy. (See Harold I. Brown, 1977.) This change of view places much greater emphasis on a decision-making process based on an individual scientist's experience. Call it 'subjectivity', if you will. The use of this prior knowledge in making judgements is represented mathematically by the Bayesian view of probability, expressed in Bayes's Theorem. It dates back to 1763! The theorem allows us to determine a conditional probability, meaning 'probability conditioned by prior knowledge'. Unfortunately, it is extremely difficult to obtain the numbers needed to insert into the theorem. Perhaps with a note of sadness, we must demote statistical analysis to a realistically lower level. To obtain further insight into this upheaval, I recommend a paper by mathematicians James O. Berger and Donald Berry (1988) (see also Howson and Urbach, 1989). The realization of practicing an illusion is disturbing, but science can continue to generate reasonably reliable information through the practice of pooling of judgements of several or many experienced scientists and the counterbalancing effects of peer antagonism and open debate. Perhaps we can reassure ourselves that there is 'safety in numbers' – not in the significance numbers of our statistical tests, but in numbers of experienced scientists focused on the same problem (Strahler 1992: 74).

Strahler's comments are interesting in their reassignment of emphasis away from the techniques which, several decades earlier, he had seen as having an equal place in experimental work as the use of 'the Brunton Compass, the petrographic microscope, the spectroscope, or the X-ray camera' (Strahler 1954: 1). However, his comparison of statistical and other experimental techniques is appropriate; just as one of the laboratory members learned, in the course of their field experiments, that the size of sample they had selected would not allow a particular piece of equipment to produce accurate results, Strahler discovered new information that caused him to consider 'demoting' the use of statistics (1992: 74).

[45] See also Ziliak and McCloskey for a wider critique of what they describe as the 'cult of statistical significance' across a wide range of disciplinary fields (2008).

The fragile process of ensuring the safe cross–appropriation of techniques could equally be seen during participant observation as a laboratory manager demonstrated a particular technique to a new member of the laboratory:

> In many cases the equipment, often designed for a very different application and adopted by the laboratory members, produces results that are only known to be valid if a strict experimental protocol is followed: constraints are often associated with the comparability of results across samples. In some cases samples are prepared to allow this comparability: they are cut from the same area of a quarry, prepared with the same saw, sanded to a similar degree of roughness, examined by the same person (one participant working in a small team explained how it was important that the same individual undertook all the microscopic work of a particular kind, in order to minimize variations across the results produced).
>
> In seeking uniformity of samples, however, some of the variability relevant to the geomorphological processes could be lost: on one occasion I noted irregular stones that had been gathered from the beach; the participant explained that these belonged to a Masters student who had chosen to use pre-weathered stones in order to increase the realism of his research. Similarly, some surfaces, at particular stages in the experiments, are intrinsically irregular: for example, granite has many different colours – this made it difficult to use colour testing equipment (as a proxy for understanding weathering impacts) effectively; similarly surfaces that had been colonised by microorganisms as part of the experimental design presented irregular surfaces less amenable to hardness testing (using equipment that relied on a smooth surface).
>
> (Based on extracts from fieldnotes)

In relation to each of these cases the scientist, together with a community of scholars, must make a judgement about how reliably she can employ a particular technique to generate data. Each of these judgements plays an important role in sustaining the transformations made throughout experimental practice.

CONCLUSION

Retaining a traditional concern of ANT, this chapter has investigated the survival of scientific research objects as they are displaced across space; however, the focus has been on research techniques (rather than research materials) as a component of research objects, and on displacement across disciplinary differentials (rather than physical distance) as a dimension of space.

The argument sought to situate the agency of individual scientists within wider experimental configurations, considering the role of (i) supervisors and colleagues (ii) equipment (iii) literature and (iv) software, in the safe displacement of research techniques. A ‘residual humanism’ (Thrift 2007: 77) persisted throughout, with (i)

continued emphasis on discursive reflection (interviews) and literature analysis alongside participant observation and (ii) discussion of the role of a scientist's 'judgement', which continually comes into play as they work to map experimental protocols developed for, and displaced from, other disciplinary settings, into the ever opening out spaces of contemporary experimental practice.

However, a question arises concerning the embodied nature of tacit knowledge (see Dilley 1999) within, not only individuals, but also amid (interdisciplinary) configurations of diverse actors. There are hints of attention to such an approach in the knowledge management literature; for example, in a discussion of tacit knowledge sharing Augier et al. comment that: 'contexts are not "just there" as static entities; instead contexts are emergent phenomena'. However, Augier et al. do not embrace the agency of non-humans; they continue:

> The same idea has been put forward by Erickson and Schultz (1997). They describe context as a mutually constituted, constantly shifting, situation definition that *emerges through the interaction of the involved individuals*. "Contexts are not simply given in the physical setting ... nor in combinations of personnel.... Rather, *contexts are constituted by what people [do and where and when they do it].* As McDermott puts it succinctly (1976), `people in interaction become environments for each other... (Erickson and Schultz, 1997, p. 22). And Dilley agrees (1999, p. 19): "Context is both constitutive of social action and itself the outcome of social action, it is both a generative principle and a resulting outcome." (Augier et al., 2001, emphasis mine).

In fact Augier's discussion elides the shift between Erickson and Schultz's understanding of context as 'constituted by what people do' (e.g. human agency) and Dilley's comment that 'context is both constitutive of social action and [its] outcome'; the latter is much closer to Latour's insight that it is not necessary to differentiate between 'physical entitities such as ferments, germs or eggs' and 'a *context* made of colleagues, emperors, money, instrument, bodily skills, and so on' (1999a: 164-5, emphasis original), that is to say between 'nature' and 'culture', or, equally, between social action (human) and its (non-human) context.

There is potential for future research to explore how this insight may extend and contribute to the ethnographic study of tacit, or embodied, knowledge sharing or in Srnicek's terms 'cognitive assemblages' (2013: n.p.). Such research could investigate (i) how 'human' and 'non-human' actors, whose configurations constitute research objects, demonstrate, hold and skilfully enact, embodied, tacit, know-how (ii) the distribution of this know-how across these configurations, and the role of scientific judgement in determining appropriate distribution of such know-how (glimpsed, for example, in the comments of the

geomorphologist (above) who discusses the use of software in applying statistical techniques to the analysis of data) and (iii) the consequences of this distributed / embodied (in human/non-human configurations) know-how for the safe displacement of techniques among disciplines, technologies and communities of practice.

CHAPTER 7: CRAFTING INTERDISCIPLINARITY III - RESEARCH ENTREPRENEURSHIP AND INTERDISCIPLINARY BECOMING

INTRODUCTION

Chapter six examined the role of a range of actors in enabling the safe displacement of techniques, amid disciplinary differentials, into the spaces of scientific inquiry. It concluded with a reflection on the possibility of considering tacit knowledge as embodied not only in the bodies of individual researchers, but also in 'cognitive assemblages' composed of a range of actors.

This chapter builds on the analysis of chapters four to six to consider the second research question associated with this thesis: 'How are the spaces of scientific inquiry reworked to accommodate interdisciplinary research objects?'. It does this through a consideration of biogeomorphology: a sub- or inter- discipline that considers interactions between ecological and geomorphological processes.

The term 'research entrepreneurship', introduced in chapter five, is returned to. Where chapters five and six considered 'cross-appropriation' (Spinosa et al. 1995: 3) as an aspect of research entrepreneurship, this chapter focuses on 'reconfiguration' and 'articulation' (p. 3). It begins, however, with two accounts, separated by one hundred years, each describing what might now be termed 'fluvial biogeomorphology':

> A tree trunk or shrub has become lodged in a riverbed; it attracts sand and forms the beginnings of a sandbank. In the rather shallow riverbed, the increase in friction causes the water to flow a little more slowly. This reduces its competency, causing new sand to be deposited in the same spot. The riverbed becomes even shallower, the friction even greater, the competency reduced yet further and an ever greater amount of sand is deposited in this spot. The sandbank reinforces itself until another occurrence, an external one, induces the cessation of this law of self-growth. The sandbank can, for instance, cause the entire riverbed to become restricted, as with a dam, or the water mass to become so concentrated that it is forced to surge through the narrow gap. Consequently, the sandbank, which had reinforced its upper surface and slowly expanded sideways in the direction of the current, is destroyed (Behrmann 2015: 132 (republished from 1919))

> I went and saw this big river … and it was blatantly obvious -you only had to look at it to see that these trees were actually being dumped on gravel bars - they were growing like mad as a result of that when you got water and sediment coming down they were trapping the sediment - the flow was diverting around them and you were getting what we call pioneer islands which were only there because that tree was there - in other words you were starting to identify a process where all right the flow of water and sediment was still very important - but without the vegetation you wouldn't get that feature - so the vegetation is actually interacting with flows of water and sediment rather than just happening to grow where the

> water and sediment allowed it - so it gives this sort of interaction idea that came through at that time (Interview transcription 12/02/2014).

Two descriptions of landscape change, though separated by nearly one hundred years, and by genre[46], bear striking resemblance to one another; each describes the role played by vegetation in landscape evolution. The first is an excerpt from German geomorphologist Walter Behrmann's 1919 publication *The Process of Self-Reinforcement* (recently republished alongside Tim Ingold's *After Behrmann: Three Short Tales of Self-Reinforcement)*, while the second is an extract from a 2015 interview with a fluvial biogeomorphologist.

Such accounts are, however, uncommon in the intervening period. Indeed the interviewee cited in the second account experiences the 'interaction idea', relating geomorphological and ecological processes, as new and time specific; they commented: 'it all started to change I would argue around about the late 90s'. As seen in previous chapters, the latter part of the intervening period had been associated with process geomorphology. The same participant commented:

> If you go back to the beginning of the sort of present process based era of fluvial geomorphology – so you're going back to people like Leopold, Wolman and Miller – so that's [nineteen] sixty four I think – so you're going back to the early sixties – and then Schumm came in – that – although all of those people acknowledged that vegetation played some sort of role in rivers it was always very much the perspective that the plants will be where the river allows them to be if you like - so it's basically flows of water and flows of sediment that were key and then you can add on other factors afterwards - but it's that flow regime and the sediment that's being eroded and transported and deposited and what size that sediment is - so have you got a river that's predominantly a bedload river - so it's got quite a coarse material and stuff moves along the bed and is dumped here and produces a <inaudible> there and produces forms or is it a fine sediment river in which case it moves in suspension and then you get a big impact on how the flood plain develops because this stuff spreads everywhere - so - that - those ideas were predominant and if you look at - if you like the pictures that people at that time produced of rivers - or even the photographs they took - yes they've got islands in their river with vegetation on them - there's vegetation on the flood plain but the assumption is those plants are there simply because erm you know you haven't got big shear stresses from the flow and then therefore the plants are able to colonise all these relatively quiescent areas … that was the view … (Interview transcription 12/02/2014).

The interviewee also identified land management practices as having mitigated against attention to the role of ecological processes in fluvial geomorphology:

[46] One bearing the hallmarks of formal academic writing and the other the colloquial language of an interview

> The fact of the matter is that there weren't many rivers in England or even most of mainland Europe - at that time - where people were leaving the wood in - because it - you know the problem with Europe is that we've been here - people have been here for so long in relatively high density - and also - very erm keen on you know managing their environment to their own benefit - that for centuries it's been making rivers drain water off the land - you know that was the aim - so you don't want wood in it - because clearly that's causing - so you take it out - and therefore - you know one couldn't observe these features (Interview transcription 12/02/2014)

Disciplinary and land management factors had, for an extended period during the twentieth century, mitigated against consideration of interactions between ecological and geomorphological processes. In the intervening period, the discipline of geomorphology had undergone a quantitative revolution; it had moved its focus of analysis away from Behrmann's evocative description to embrace an emphasis on quantifying physical processes.

This chapter focuses on the period from the 1980s to the present day. The first part draws on Spinosa et al.'s categories of reconfiguration and articulation as components of 'world disclosing' entrepreneurship (1995: 3) to employ the concept of 'research entrepreneurship' introduced in chapter five. It examines how geomorphologists and ecologists have worked to extend and open out discursive dimensions of the spaces that accommodate new or altered research objects. The second part sees a shift of emphasis to consider how geomorphologists, and the discipline of geomorphology, participate in a world disclosing movement of becoming, or eventfulness animated by contingency.

PART ONE: RESEARCH ENTREPRENEURSHIP: RECONFIGURATION AND ARTICULATION IN GEOMORPHOLOGY AND ECOLOGY

RESEARCH ENTREPRENEURSHIP: RECONFIGURATION AND ARTICULATION

This section draws significantly on Spinosa et al.'s analysis of entrepreneurship (1995, 1999), emphasising the ability of entrepreneurs to 'disclose new worlds'. Perhaps unsurprisingly, Spinosa et al. see the creation of an enterprise as essential to entrepreneurship (1995: 6)[47]; however, this assumption is not adopted within this chapter.

[47] Spinosa et al. comment: 'The innovation of a new understanding of work, like many other innovations, does not, however, make an entrepreneur. It makes only an inventor. In the case of our composite entrepreneur it resulted in an academic degree and presumably could have led to an academic career. But instead our entrepreneur formed an enterprise for *marketing* his new conception' (1995: 6).

In common with the use of the term ‘entrepreneur’ at work in Hughes’s discussion of ‘the ecology of institutions’ (1970: 62 cited in Star and Griesemer 1989: 389), this study adopts the term entrepreneur while leaving behind the commercial ‘enterprise’ stressed by Spinosa et al. This difference is emphasised in the prefix ‘research-’ given to the term entrepreneur.

In an analysis put to work within Star and Griesemer’s early ‘Science Technology and Society’ (STS) discussion of a natural history museum (1989: 389), Hughes comments that:

> Someone inside the institution acts as an entrepreneur … one of the things the enterprising element must do is to choose within possible limits the environment to which the institution must react, that is, in many cases, the sources of its funds, the sources of its clientele (whether they be clients who will buy shoes, education or medicine), and the sources of its personnel of various grades and kinds (Hughes 1970: 62 cited in Star and Griesemer 1989: 389).

Although the research entrepreneur forgoes a commercial enterprise, the term retains characteristics of entrepreneurship that relate to the term’s quotidian usage; in this way it works to highlight key aspects of the approach to research fostered by Viles and members of the laboratory. For example, the Rock Breakdown Laboratory faces restricted equipment budgets; if necessity is the mother of invention, the need to find ways to continue scientific work leads to an entrepreneurial approach to developing the laboratory. In the course of ethnographic fieldwork the following examples presented:

> A professor based in the laboratory described how an advertisement in an inflight magazine for a unmanned aerial vehicle (UAV or ‘drone’), aimed at amateur enthusiasts, sparked interest: it presented the possibility of accessing a technique for producing images of a field site at a much reduced cost compared to similar technology available for professional use.
>
> Similarly a trip to the local Do-It-Yourself (DIY) store was cited as an opportunity to identify new techniques, for example, in the course of participant observation a dust bag attached to a drill, intended to allow DIY enthusiasts to undertake their home improvement efforts without creating a mess, had been adopted to collect the dust from drilled rock samples for measurement and analysis.
>
> (based on extracts from field notes)

The laboratory has also worked with equipment suppliers, testing equipment designed for other sectors (e.g. construction or medicine) to assess whether such equipment can be adopted for geomorphological and related applications. This has led, in one instance, to a research studentship funding such work. Beyond developing the laboratory itself, an entrepreneurial eye for funding opportunities has become increasingly requisite – the

applicability of research to contemporary challenges was seen by at least one interviewee as closely tied to future job prospects; a factor which links towards mode-2 discussions (Gibbons et al. 1994).

This somewhat quirky and imaginative approach to conserving precious equipment budgets, undertaking as much science as possible at minimum expense, underlies the 'research entrepreneur' approach; without the presence of this characteristic in the work of the geomorphologists at the Rock Breakdown Laboratory it would be difficult to attribute such a term.

However, it is the 'world disclosing' aspect of Spinosa et al.'s discussion of entrepreneurship that is of key importance to the analysis set out in this chapter. The remainder of this section is structured according to the first two of the three characteristics of world disclosing entrepreneurship identified by Spinosa et al.: 'reconfiguration' (where 'marginal practice' becomes 'central') and 'articulation' ('whereby dispersed or confused practices are brought into clearer focus') (1995: 3)[48].

RECONFIGURATION: WHERE MARGINAL PRACTICES BECOME CENTRAL

1988 saw the publication of Heather Viles's edited collection *Biogeomorphology*; a book that introduced the term biogeomorphology and gathered relevant research from the margins of the discipline. Although Viles (1988: 5) commented in the introduction that the term was 'not intended to be a long-lived term describing a separate field of research, but rather used … simply to focus attention', she noted during an interview that this apparent tentativeness belied a greater, albeit quietly held, sense of ambition:

> You tend to think right well I'm not going to make a lot of this, I'm going to bring it together and see what happens. And I think some people might come from a more heroic school of, you know, 'I really want to set this up and therefore I'm going to'. So I think hidden personalities come out in this (Interview transcript October 2014).

As Thornes remarks, early work in biogeomorphology was written 'by geomorphologists for geomorphologists' (1990: 3), while, as seen below, later biogeomorphology functions sometimes as a sub-discipline and sometimes as an inter-discipline[49]. What then, did Viles and her counterparts wish to draw attention to? How were early researchers in biogeomorphology seeking to reconfigure their parent disciplines?

[48] The third, cross-appropriation, is discussed in chapters five and six in an examination of interdisciplinary genealogies of techniques employed in rock weathering geomorphology.
[49] Note Thornes's remark that 'the next stage of development [of research in the field of vegetation and erosion] will rely heavily on cooperation with ecologists if meaningful progress is to be achieved' (1990: 3). Donaldson et al.'s 'among disciplines' is salient here (2010: 1534)

Viles's critique of mainstream geomorphology is evident as she writes: 'several recent geomorphology texts give the impression that landform development occurs within a largely abiotic environment', with the focus on the interaction between organisms and rocks limited in 'most textbooks' to a 'picture of plant roots' (1988: 8). Two years later, Thornes noted that while among geomorphologists there might be an 'implicit acceptance of the importance' of geo-/bio- interaction, as a profession they remained 'reluctant to enter this arena in a serious fashion' (1990: xiii). A decade later, in a paper Viles co-authored with a former doctoral student, it was noted that while 'Day and Goudie comment that a rock surface must be thoroughly prepared with a carborundum to remove dust and dirt prior to measurement' such preparation would 'remove any microtopographical variation or microbial organism that may play a protective role [in mitigating rock erosion]' (Naylor et al. 2002: 10). In a more recent interview Viles explained that geomorphology had tended to be 'brown not green' (Interview May 2012). Similarly, Butler et al. comment that: 'the interaction of ecological actions and processes with the geomorphological world … went largely unappreciated in the geomorphological literature for the majority of the 20th century' (2012: 157, see also Thornes 1990: xiii) . Finally, Angela Gurnell commented, on receiving the 2012 Linton Award, that in the late 1990s she had 'literally been treated as a mad woman' over her focus on the role of plants in fluvial geomorphology (field notes 26/6/2012).

In order to enable the accommodation of interdisciplinary research objects, biogeomorphologists worked to reconfigure the discursive dimensions of the spaces of geomorphological inquiry. To do this they:

(i) Claimed their authority through reference to the antecedent discipline(s)

> For example, Viles notes that: 'the histories of biogeography, ecology and geomorphology … illustrate some common concerns and shared methodologies, which have developed out of the early unity of natural history' (1988: 5) and Butler et al. comment that: 'the significance of the geomorphic impact of animals was recognized early by key figures in the history of natural sciences and the discipline of geomorphology, most notably with research projects by Charles Darwin (1881, earthworms) and William Morris Davis (1928, coral reefs)' (2012: 157).

(ii) Worked to 'translate', comparing concepts from geomorphology with those of ecology

For example, Viles points out parallels between ecology and geomorphology, mentioning comparisons between Clements's work in ecology and Davis's in geomorphology; the impact of systems approaches in the fifties and sixties, and 1980s concepts of punctuated equilibria in biology which may be compared with neo-catastrophism in geomorphology (Viles 1988: 5). Similarly, Stallins identifies parallels between reductionist/process based studies in geomorphology and models of plant succession in ecology (2006: 209).

(iii) Undertook similar 'translations' comparing the concepts of biogeomorphology and related inter- / sub- disciplines with concepts un-available to their predecessors in each of the antecedent disciplines

For example, in a paper entitled 'Geomorphology and Ecology – Unifying Themes for Complex Systems in Biogeomorphology', Anthony Stallins discusses non-linearities in biogeomorphological interactions, working to translate between geomorphology and ecology, as well as between recent scientific paradigms (non-linearity) and their predecessors (2006).

Drawing on a proposed concept of 'research entrepreneurship', this section has developed an analysis of how biogeomorphology relates to the first of Spinosa et al.'s attributes of entrepreneurship – reconfiguration – denoting efforts to bring marginal practices to the centre. Similarly, it has seen how Viles and others situate their work – at times agonistically or antagonistically (Barry and Born 2013) – identifying shortcomings in antecedent disciplines. It has noted, however, that as biogeomorphologists have sought to craft a new field of research, they have worked this into the existing disciplinary fabric, drawing authority from precedents in antecedent formations as well as translating concepts into the terms of both antecedent and cognate disciplines (e.g. geomorphology and ecology).

The next section turns to consider the second attribute of entrepreneurship identified by Spinosa et al. (1995): articulation. It continues, but expands on, the discussion of biogeomorphology, explaining a proliferation of related, overlapping, terms and areas of research that have burgeoned in recent decades.

ARTICULATION – BRINGING DISPERSED OR CONFUSED PRACTICES INTO CLEARER FOCUS

> Literature reviews are *the* quintessential site of identity work, where the novice researcher enters what we call occupied territory – with all the imminent danger and quiet dread that this metaphor implies – including possible ambushes, barbed wire fences, and unknown academics who patrol the boundaries of already occupied territories. … The novice researcher is not only an alien in foreign fields, but is unaware of the rules of engagement, and the histories of debates, feuds, alliances and accommodations that precede her entry to the field. This is not work for the faint/feint-hearted! (Kamler and Thomson 2006: 29).

When Heather Viles conducted a literature review of research relevant to her NERC-funded doctoral investigation into the effect of lower plants and algae on limestone, she realised there was a lot of research scattered about in different places, lacking a single person to bring these together (interview notes 2/5/2012). Her work on this resulted firstly in the production of a 1984 paper 'Biokarst: Review and Prospect' followed, as we have seen, by the 1988 publication of the edited collection *Biogeomorphology*. This work can be understood as an example of what Spinosa et al. call 'articulation', defined as bringing 'dispersed or confused practices … into clearer focus' (1995: 1).

The role played by this process of articulation can be seen in the comments of a fluvial geomorphologist who participated in this ethnography:

> I use the word biogeomorphology because it would be nice to have a kind of bio-fluvial geomorphology or something like that I don't know … and I use biogeomorphology because it's relatively short and it's got in there two elements that I'm interested in - but you're probably aware there's loads of jargon out there - so there's ecohydology, hydro-ecology, what the difference between those two I don't understand or know - then you get … hydro - no - yes – hydro-morphology - which is a very weird word - to me - completely meaningless because water doesn't have a shape does it - so - but they all mean - <pause> I think they mean - <laughs> hydrology and fluvial geomorphology - that's what they mean … and then there's biogeomorphology - and there are all these words that people are happily thinking up … if I had to describe to somebody quickly what I did - I'd probably say I'm a biogeomorphologist - my work on rivers - because I'm not like, for example, Heather Viles - I mean she'd say she's a biogeomorphologist but what she does and what I do are completely different - erm so I suppose … biogeomorphology probably is now an accepted sub-discipline of geomorphology but it's broader – a lot broader than what I do - it's everything where organisms - and so you … could be working on beavers on rivers - that would be biogeomorphology wouldn't it - but it's animals - you could be … working on erm weathering - biological materials - or organisms - you could be - you could be looking at permafrost - couldn't you - and whether the vegetation is preventing an active layer developing or not - that would still be biogeomorphology wouldn't it - so I guess biogeomorphology probably has got street cred - and is accepted now - you need people to write books on it - so again Heather edited a book didn't she … back in I can't remember - I think it might have been '89 - quite a long time ago anyway - which was called *Biogeomorphology* - now if you produce a book - and

you put a name on it - and it has reasonable sales - then the word will be taken up (Interview transcription 12/02/2014).

While this account indicates the role played by articulation it also indicates a diversity of terminology amongst which 'biogeomorphology' jostles. Indeed, considered alone, an account focused on the work of Viles risks to portray an overly neat and linear genealogy of biogeomorphology as a sub-field within geomorphology. As Barry and Born argue, 'any analysis of the inventiveness of interdisciplinarity must attend to the path dependence of specific interdisciplines, their genealogies and multiplicity' (2013: 42). This section seeks to complicate the account of of biogeomorphology provided so far, examining a range of efforts at 'articulation', with attention to the ways different, related, terms jostle and overlap, emphasising the multiplicity of the field's development. It first considers the way in which, even as Viles sought to articulate previously dispersed practices, 'bring[ing] together' 'scattered' research under the term biogeomorphology, a parallel process of diversification and fragmentation was underway, as a number of nascent nomenclatures emerged. For example, while biogeomorphology had grown out of the discipline of geomorphology, the term, 'ecosystem engineering' was proposed by, among others, the ecologist Clive Jones (Jones et al. 1994). A second focus for this section is an examination of renewed efforts at articulation; that is attempts to integrate biogeomorphology and ecosystem engineering within the production of academic texts.

In the period following[50] the 1988 publication of Viles's book, *Biogeomorphology,* a number of meetings and publications with a similar focus emerged:

- As seen above, alongside Viles's book, 1988 also saw a British Geomorphological Research Group (BGRG)[51] meeting dedicated to the consideration of the 'importance of vegetation by geomorphologists' (Thornes 1990: xiii). The proceedings of this conference were published in 1990 in an edition edited by Thornes (1990)
- In 1991 there was a Zoological Society of London symposium on animals and their burrows (Butler 2012:1)

[50] Viles's first publication in the yet to be named field of biogeomorphology came in 1984 (Biokarst: review and prospect). At the same time, early interest in fluvial eco-geo interactions was occurring in the UK and USA (see Harman et al. 1984). In so far as the researcher has been able to ascertain, these concurrent efforts were independent and although intersecting (for example, in the 1988 publication of Biogeomorphology, which contained a chapter on fluvial biogeomorphology) they are not directly linked even if they might be understood as part of a movement.
[51] The British Geomorphological Research Group – founded in the early sixties – in part to 'carry out field tests on various mapping techniques' (Goudie, Price 1980: 241) has now been incorporated into the British Society for Geomorphology (BSG).

- 1995 saw the Binghampton Symposium on Biogeomorphology, Terrestrial and Freshwater Systems (Butler 2012: 1, Wheaton et al. 2011: 265)
- More recently, 2008 saw the Binghamton Symposium on Zoogeomorphology and Ecosystem Engineering (Butler 2012: 2), the Meeting of Young Researchers in Earth Sciences (MYRES III) – with the theme 'Dynamic Interactions of Life and its Landscape' and the British Hydrological Society (BHS) meeting on Eco hydraulics at Scales Relevant to Organisms (Wheaton et al. 2011: 266)

As indicated in these recent events, a jostling of new terms attempting to delineate a fresh area, or related areas, of study, can be identified. This is paralleled in texts written under the flags of:

- Ecogeomorphology (Wheaton et al. 2011)
- Zoogeomorphology (Butler 1995, Butler 2012)
- Ecosystem engineering (Jones et al. 1994, Jones et al. 1997).

Other compound terms present in the literature include: biogeography, geoecology (Naylor et al. 2002), geobiology (Naylor 2005), biogeochemistry, geomicrobiology (Viles 1988: 5) and the activities of the geosphere – biosphere international programme are also mentioned (Phillips 2003: 20).

Figure 10 shows two alternative mappings of this emerging field of research, drawn from these literature sources. This diversification shows that the genealogy of the field(s) investigating interactions between biotic and abiotic processes (referred to hereafter as 'eco-geo' (Wheaton et al. 2011: 265)) cannot be understood as linear; even as efforts to gather dispersed literatures were underway (articulation), processes of fragmentation occurred, with multiple nomenclatures developing within a short space of time. As the diagrams below show, however, such fragmentation is mediated by emergent connections.

(i)

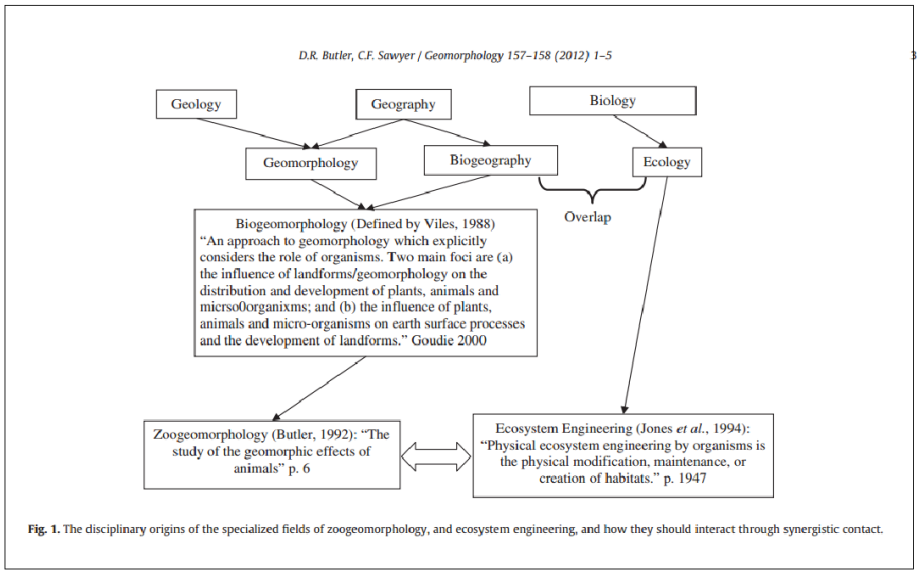

Fig. 1. The disciplinary origins of the specialized fields of zoogeomorphology, and ecosystem engineering, and how they should interact through synergistic contact.

(ii)

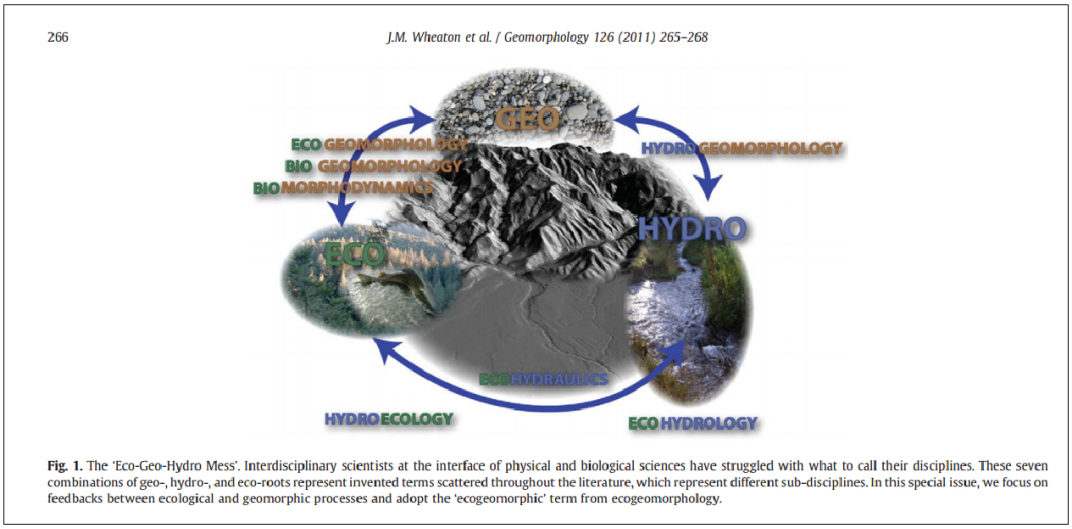

Fig. 1. The 'Eco-Geo-Hydro Mess'. Interdisciplinary scientists at the interface of physical and biological sciences have struggled with what to call their disciplines. These seven combinations of geo-, hydro-, and eco-roots represent invented terms scattered throughout the literature, which represent different sub-disciplines. In this special issue, we focus on feedbacks between ecological and geomorphic processes and adopt the 'ecogeomorphic' term from ecogeomorphology.

Figure 10: Diagrams of an emergent interdisciplinary field of research

From (i) Butler et al.'s introduction to a special edition of the journal Geomorphology entitled 'Zoogeomorphology and Ecosystem Engineering' - presenting fifteen invited papers from the 42nd Annual Binghamton Geomorphological Symposium convened in Alabama, USA in October 2011 (2012: 2) and (ii) Wheaton et al.'s preface to a special edition of the same journal entitled 'Multiscalar feedbacks in Ecogeomorphology' (2011: 266)

The proliferation of terminology and associated range of research interests indicate the validity of Barry and Born's imperative: 'Each interdisciplinary field must be analysed as precisely in formation and 'in play' – as a multiplicity' (2013: 13). The temporal movement

animating such configurations is examined in the next section. First, however, there is one additional way that the discursive dimension of the spaces accommodating scientific research objects are expanded; not only does the proliferation of terminology merit attention, but also the looseness and inconsistency of meanings associated with such terminology.

An early career geomorphologist, interviewed in the course of this research project, explained that different terms provide an indication of differing, albeit related, research interests:

> … going back to … before when we were talking about the kind of ecology and geomorphology kind of crossovers - erm - there's no real accepted term for that and yeah you can tell a lot about someone's sort of theoretical outlook as well as their - erm - possibly their disciplinary background from what they call it - so in terms of some people call it ecohydrology - some call it biogeomorphology - some ecosystem engineers - there's a few others that that slip my mind at the moment but some things like ecohydrology, ecomorphology would be kind of I would characterise as being much more from an ecology background and looking at the influence of ecology on things like geomorphology and hydrology - erm biogeomorphology I would characterise as being more focused on - erm - the sort of smaller kind of organisms so things like biofilms, algae, possibly macroinvertebrates erm but and then ecosystem engineers is kind of quite an old concept which kind of almost - erm - gives agency to - erm - organisms which are modifying the habitats … so you can tell a lot about someone's - with certain examples - about someone's training or their background - erm their kind of theories that they are thinking about (Interview transcription 10/08/2015).

Similarly, in the preface to a 2011 special issue of the journal *Geomorphology*, Wheaton et al. reflect on the diverse terminology geomorphologists may encounter:

> Ecogeomorphology and biogeomorphology are generally considered synonyms (Hupp et al., 1995a), but they do sprout from slightly different research interests. Both are part of a larger family of terms that researchers have used somewhat inconsistently to describe research at the interface between some combination of hydrology, geomorphology and/or ecology/biology (Wheaton et al. 2011: 265).

However, and in line with the 'inconsistent use of terms' acknowledged by Wheaton et al., agreement over the import of specific terms is not necessarily shared; while the early career geomorphologist cited above saw the term ecosystem engineering as 'quite an old concept', another early career researcher interviewed for the study saw ecosystem engineering differently:

> One early career research participant described how they defended the inclusion of a chapter focused on ecosystem engineering in a biogeomorphological doctoral thesis - submitted in a UK geography department with links to Oxford's Rock Breakdown Laboratory – despite some questioning from one of their supervisors.

> Describing the incredible popularity of the concept they saw it as incorporating 'similar ideas' as biogeomorphology, but as having gained greater attention. Seeing these ideas as crucial to biogeomorphology in the next decade they described having to persuade one of their doctoral supervisors of the merit of engaging with this terminology and research conducted by its proponents. The researcher felt that it was important to engage with the fresh impetus, and shift of emphasis, which they saw as being offered by this new terminology. However, their supervisor had questioned the value of the terminology, suggesting that it offered nothing new to the discipline of ecology which, in their view, had always incorporated a focus on the relationship between organisms and their environment (based on fieldnotes, summer 2012).

Another perspective on the term 'ecosystem engineering' is provided by a critical blog post by ecologist Jeremy Fox. Fox acknowledges that the article introducing the term ecosystem engineering (Jones et al. 1994) is the most highly cited article ever published by the journal *Oikos*, with over 1200 citations. However, while noting the term's 'prominence' as an 'ecological idea', he is critical, decrying a perceived 'over-generality' in the term, a charge he equates with (i) vaguely defined terms, (ii) loose analogies, (iii) "lack[ing]… a general mathematical version", and (iv) "reliance on purely statistical tools" of comparison (Fox 2011: n.p.). Fox acknowledges the term's stronghold in the ecological literature, with reference to reviews by Wright and Jones (2006), and he notes that these are ideas that have 'spurred huge amounts of useful work' which he says 'will retain much value even if the original ideas … ultimately prove to have been over-expansive'. Indeed, he observes that it is perhaps the very vagueness he perceives in the concept of ecosystem engineering, which provides its capacity to inspire and influence, remarking: 'I'm sure these ideas would've been much less inspirational and influential if they had initially been more narrowly and precisely tailored' (Fox 2011: n.p.).

Fox's observation concerning the connection between the vagueness, with which a concept is articulated, and its potential for wide application, is partially elucidated by the notion of 'boundary object'. This term, developed by Star and Griesemer in a study of a natural history museum in the early twentieth century[52], describes an object that is 'both adaptable to different viewpoints and points of view and robust enough to maintain identity across them' (1989: 387) with their boundary nature [being] reflected by the fact that they are simultaneously concrete and abstract, specific and general, conventional and customized (p. 408). However, two questions emerge: firstly, is the singular entity inherent in the concept of a boundary object too restrictive (Donaldson et al. 2010) to characterise adequately the proliferation of terminology and even greater range of

[52] Coincidentally, Star and Griesemer developed the term in a study of a Berkeley natural history museum during a period which saw ecology 'distinguishing itself from its basis in natural history and adopting new methods' (Star and Griesemer 1989: 394)

research? Secondly, what type of object do Star and Griesemer refer to and how does this relate to the usage employed in this thesis?

Donaldson et al. argue that the term 'boundary object' implies an essentially epistemological approach: a singular object exists about which differing perspectives emerge (2010: 1532). Following Mol (2002), however, they propose an ontological approach:

> The ontological standpoint suggests that when the situation to be addressed by interdisciplinary research is controversial, that may be because the different sides of the controversy are presenting not differing perspectives, but different, partially connected objects. The essence of a multiple object is that its identity is not robust enough to be maintained uncontroversially across domains of practice without additional strategies of support (Mol, 2002). Multiplicitous objects make poor boundary objects as they require some form of coordination to hold together (Donaldson et al. 2010: 1532).

This approach, positing 'different, partially connected objects' (p. 1532) more easily describes the diverse configurations of research associated with the term 'ecosystem engineering': this is not a single field about which different perspectives exist but, in Barry and Born's terms 'a field of differences, a multiplicity' (2013: 5).

A second difficulty with the term 'boundary object' concerns the meaning of 'object'. According to Star and Griesemer, 'in natural history work … these objects [include] specimens, field notes, museums and maps of particular territories' (1989: 408); however, this thesis would refer to such items as artefacts. The term 'object' is instead understood as a focus for research – that is to say a 'scientific research object' as discussed by Daston (2000) and others. Such objects may be less concrete: for example, in the case of geomorphology the objects in question are often geomorphological processes, while ecosystem engineering is similarly 'process-focused' (Wright et al. 2006: 205), paying attention to the interaction of biotic and abiotic processes.

However, Fox's discussion does not directly concern processes, but ecosystem engineering as a terminology, and associated area of inquiry; how then does this relate to a research object? Star and Griesemer provide a partial answer in their linking of representations and objects; they comment that 'in natural history work boundary objects are produced when sponsors, theorists and amateurs collaborate to produce representations of nature' (1989: 408). They continue:

> When participants in … intersecting worlds create representations together, their different commitments and perceptions are resolved into representations - in the sense that a fuzzy image is resolved by a microscope. This resolution does not

> mean consensus. Rather, representations, or inscriptions, contain at every stage the traces of multiple viewpoints, translations and incomplete battles (1989: 413)

However, the term 'representation' again implies a stark division between the objects of scientific research, and their representation in the discourses of scientists. This division breaks down in the hands of Daston and Latour, who understand scientific research objects as being both 'real *and* historical' (Daston 2000: 3, emphasis original); as such scientific endeavours are understood as being constitutive of scientific research objects (Daston 2000). Indeed, according to Latour's account, as described by Daston, the 'persistence [of research objects]… depends on the institutionalization of practices and an impressive array of apparatus' and their reality depends on 'the degree of [their] embeddedness in such organized systems of techniques and instruments' (2000: 13). As seen in previous chapters Latour argues:

> We do not have to consider physical entities such as ferments, germs, or eggs sprouting into existence as being radically different from a context made up of colleagues, emperors, money, instruments, bodily practices, etc. … [this] has the great advantage of requiring us to stabilize neither the list of what makes up nature nor the list of what makes up context (2000: 260).

In this way a terminology, and an associated field of academic inquiry, may, in a similar way to texts, be understood as a discursive dimension of the spaces within which research objects emerge. Further, drawing on Daston and Latour's argument, terminologies are constitutive of the research objects themselves. The range of terminology described above serves to multiply and extend the discursive dimension of the spaces that both accommodate and animate interdisciplinary research objects.

SUMMARY

Part one examined the role played by research entrepreneurship in the development of biogeomorphology and related areas of research. It first considered reconfiguration (bringing marginal practices to the centre) and articulation (bringing dispersed or confused practices into clearer focus). The analysis continued to consider how processes of gathering (articulation) and fragmentation occurred simultaneously. This developed into a discussion of boundary objects and multiple areas of research. Throughout part one there has been an emphasis on the (human) '*effort*' required not only 'to sustain stable networks of relations' (Law and Singleton 2005: 337, emphasis original), but also to 'protend or open up the space of future possibilities' (Barry and Born 2013: 5). However, as Barry and Born make clear, this inventive effort is necessary but not sufficient in the production of eventful becoming (2013: 6). As such, 'invention should not be understood

as a moment in time, but as a process' (Barry and Born 2013: 6); as such, it occurs not only in space but also time. The next section builds on the analysis so far to explore how interdisciplinary configurations emerge through a hybrid (human and non-human) process of becoming.

PART TWO: NEW RESEARCH OBJECTS IN SPACES DEFINED BY AN ONTOLOGY OF BECOMING

This section examines innovative research into the role of ecological factors in UK and European rivers, with key disciplinary developments occurring from the end of the 1980s until the early 2000s. It is explored primarily with reference to an interview with Aidan, a senior geomorphologist encountered in chapter four, whose career stretches from the 1970s until the present day, with a focus on interactions between ecological and geomorphological processes occurring from the late 1980s onwards.

Texts continue to play a key role in defining a discursive dimension of the spaces accommodating new research objects. The senior geomorphologist explained that prior to 2000 they had been unable to publish outside geomorphological journals:

> But I think again the big break through is since 2000 because before then people like me were publishing in geomorphological outlets - so *Earth Surface Processes and Landforms* - *Geomorphology* - Binghampton Symposia - which were driven by geomorphologists - but - and I'd tried publishing in ecological outlets and failed - and then - suddenly after 2000 and whether it's because I started to understand more how ecologists think and therefore how you have to present it - or whether the ecologists were getting more interested in geomorphology - I don't know - but suddenly - I found it possible to publish in ecological outlets - and that was a major breakthrough - because once you start publishing in the ecological journals then the ecologists will look at it - and so it - again - it's the last 15 years really that these - these links … have been made (Transcribed interview with Aidan – February 2015).

This shift, enabling publication in both geomorphological and ecological journals does not, as seen above, indicate that early work produced by biogeomorphologists found ready acceptance in mainstream geomorphology. The senior geomorphologist readily acknowledged the role played by Viles's 1988 publication *Biogeomorphology* in demarcating a new disciplinary space. Similarly, it is important to note that it can also be difficult, in the present day, for researchers working in the area of biogeomorphology to publish in mainstream geomorphological journals. The early career researcher commented that:

> Yeah I - because geomorphology is a kind of strange one - I … definitely self-identify as a geomorphologist but geomorphology's quite a - I've realised the more I've kind of gone on with research that geomorphology's quite a sort of nebulous

> term and it's - erm - what it kind of touches on will - well what people think it touches on will differ depending who you talk to so although I consider myself to be a geomorphologist one might look at the research projects I've worked on and say well that's not really geomorphology, that's more hydrology or more kind of ecohydrology or - there's no real geomorphology - in there you know you're not kind of sketching landforms or describing them and that's what geomorphology is and in terms of things like submitting papers to geomorphology journals you know I have had things sort of sent back saying there's not enough geomorphology in it - kind of taking maybe a slightly narrower view of what geomorphology is - - so yeah it's something that it's not - it's not just a kind of identity thing it does actually then feed into research as well as kind of people identifying what they think geomorphology is or what journals define geomorphology as and sort sifting articles on that basis and so it seems a very I think because it's quite a nebulous concept the definitions shift as well not just between people but my perception is the shift (inaud.) time as well (Transcribed interview with Kumara – August 2015).

This account of the amorphousness of geomorphology as a discipline, as well as of interdisciplinary research areas, is important; as Barry and Born argue: 'it would be a mistake to contrast the homogeneity and closure of disciplines with the heterogeneity and openness of interdisciplinarity' (2013: 7). However, at this stage it is necessary to notice that throughout these accounts there is evidence of the effort involved in building and maintaining the discursive dimensions of interdisciplinary spaces. The senior geomorphologist's account moved between descriptions exhibiting a sense of pushing against the status quo and, conversely, a sense of the 'sheer chance' animating and producing momentum within a burgeoning research avenue. This was described with reference to the motif of a wave:

> Well when - when we started just working on [a particular biogeomorphological process] - in the Faircrest - I think people thought we were extremely odd … (laughter) you see the thing I've often said to people is to be successful - really successful in life - and get loads of NERC grants and so on - you obviously need to be innovative - but you want to be on the leading edge of the wave - you don't want to be ahead of it - and unfortunately in the Faircrest we were ahead of it - by about well at least ten years I would say. So nobody else in the UK was looking at that sort of stuff - and it was almost dismissed as being trivial - I mean we published papers because in the US people were starting to work on this - so I think the first paper I've got with Sean- and [he] was the one who was promoting the [work on this ecological process] I hasten to add - I think was 1984 - and that was the same date that there was a sort of monograph produced in the US - in the northwest US - on [the same biogeomorphological process] - I mean that was based on lots of people working on [the same ecological process] - but that monograph … in 1984 was the real beginning of [such] research being treated seriously in the US - being a big thing - and our first paper was in '84 - so there had been nothing going on in Europe at that time. Because you know lots of scientists will - you know see what somebody else is doing and think well that's interesting … and then they join in and they develop their own area in it - but that's only when the first scientist is on the … leading edge of the wave <laughs> and as

> I say we were out here <gesture to indicate a position ahead of the wave> somewhere (Transcribed interview with Aidan – February 2015)

Later in the interview the same geomorphologist returned to the motif of the wave to describe present-day research:

> So I thought well what we really need is we need some sort of conceptual framework to think about these … rivers - because we haven't got very many - erm - in terms of what their geomorphology might look like - what their vegetation might look like and how those two might interact to give you your habitat mosaic and your processes which influence the fish and the organisms that can move around - and so I developed a very simple conceptual model - and I thought it was so simple - it just sort of described the way I was thinking at the time - and actually you know colleagues on that project have really taken to this - ecologists, hydrologists, and so on - and this very very simple framework erm and I - I think you know - we're at this wonderful stage in fluvial biogeomorphology at the moment - where there's lots and lots of people working on it but it's only been going since 2000 seriously - and so you can still come along and actually throw quite simple ideas in - and people will run with them because it's - the timing's right - and I am now actually on the crest of the wave rather than ahead of it (Transcribed interview with Aidan – February 2015).

As Barry and Born make clear 'in order for inventiveness not only to open up possibilities but to bring about an event, it is necessary for it to be recognised and taken up by others' (2013: 6). Such eventfulness is predicated on the timeliness of inventiveness; that is on its occurring in a space, human and non-human, that is both receptive towards, and animating of, a movement or becoming. The next sections explore the factors beyond texts that produce a wave-like disciplinary becoming that resonates with the geomorphologist's descriptions.

BECOMING

A river had been chosen by chance in an area that, unusually for the UK, had not been subject to typical land and river management practices. As discussed in chapter four, the field site was chosen for reasons independent of the biogeomorphological research focus that developed:

> It was sheer luck that when I was appointed to Edgewick I needed to set up a catchment so I could take students out and say look this is a flow gauging station, this is a rain gauge and so on – and then it was sheer luck that I chose the Faircrest to do it in … (Transcribed interview with Aidan – February 2015).

Having set up a field station at this site, the geomorphologist, who at the time was in the process of moving from another related discipline into fluvial geomorphology, had started to pay attention to the relationship between vegetation and ground water; therefore, they

were already producing work involving attention to ecological processes. They commented:

> …and then Sean came along - a couple of years later - and we went out and [he] said 'oh look at all [this]' - which was in the main channel it wasn't in my little catchment - and [he] said 'oh - - I think we should - we should look at that' (Transcribed interview with Aidan – February 2015).

Through this work a space emerged within which it was possible to continue investigation of ecological processes. Several years later the geomorphologist began working on another UK field site and, simultaneously, a site in continental Europe. At the second UK field site the geomorphologist worked alongside an ecologist and an engineer who had been experimenting with alternative river management approaches. The geomorphologist described how the objects of research included ecological as well as geomorphological processes:

> And then there was thoughts about - yeah and of course as wood accumulates or trees spout - then they trap seeds which are moving down through the river because if you're depositing sediment you're depositing seeds - so I started thinking about all these processes which are actually ecological processes - but I mean I'm not an ecologist - and I started doing a lot of work on seed banks and you know how they vary through time and where seeds end up and - erm - how native species can sort of actually survive in rivers and can outcompete these invasive species - if you leave the river alone - -so all those ideas came along and I think I just got known for it (Transcribed interview with Aidan – February 2015).

Through the 1990s a study group developed around the European field site. The geomorphologist described the connections among scholars precipitating this:

> It's just sheer chance - you have to be with the right people so - - on the Ananes - well there were three key ecologists - there was Lana Page … there was Shelly Barnett who was [their] postdoc - and then there was another ecologist called Kyle Mathis who I happened to have known for years - we were appointed together at Edgewick in the early 70s - but [they] happened - huh - to have left Edgewick and got a post in Wita in close proximity to Lana Page - you know so - you've got these three ecologists - all of whom - I think - at that time were quite unusual for being able to think big as well as small … so it was having those three people together who were ecologists who were interested in the bigger scale - along with myself and somebody called Felix Reynolds who you may have come across - so we were two geomorphologists - who just happen to be interested again in quite large scale fluvial geomorphology - and that little group together - we were able to go into the field and actually talk in a way where we could communicate with one another … so for that work it was just sheer chance - that we got together…
>
> … you know you can look back at it and think oh yes you know there was this sort of I don't know global trend or European trend - or whatever but actually ultimately it comes down to individual people happening to get together at the right time - and being able to communicate with one another and er - and everything else goes from that (Transcribed interview with Aidan – February 2015).

This invocation of 'luck' or 'chance' is striking given the role played by deterministic rules in geomorphology[53]; indeed, it is no less arresting for the fact that the interviewee is commenting on the role played by chance in the social circumstances of research rather than the evolution of the landscape. Refusing to differentiate between the physical becoming of a landscape and the social becoming of a discipline is intuitively easiest when the two are tightly bound in applied geomorphology, with the performative impacts of geomorphological analysis shaping the landscape through environmental management. However, even where geomorphology is not applied, landscape and landscape science are subject to events which intersect and intertwine in hybrid becomings, not subject to differentiation between the social and the physical. Geomorphologists attend with great care to the processes of landscape change; it would be strange if these very processes did not work their way into the stuff of the discipline itself with the latter a formation produced through dialogues of both human agency and the non-human agencies of the landscape.

The research objects associated with the interdisciplinary field of biogeomorphology are not only composed of the *geo* (earth) and the *bio* (life) (Whatmore 2013: 161); they are equally composed of the social. In undertaking biogeomorphological research, human actors *participate in* the geo/bio/social becomings of the world; in this way these scientists 'disclos[e] new worlds' (Spinosa et al. 1995: 3).

CONCLUSION: SPACE AND BIOGEOMORPHOLOGICAL RESEARCH OBJECTS

This chapter addressed the second research question: 'how are the spaces of scientific inquiry reworked to accommodate interdisciplinary research objects?'. The focus for the chapter was on 'biogeomorphology', an interdisciplinary field of research that considers interactions between ecological and geomorphological processes. The chapter began with two accounts describing instances of what might now be termed 'fluvial biogeomorphology'. The descriptions, though separated by one hundred years, shared a striking resemblance; however, it was noted that in the intervening period such accounts were rare. This cause of this period of inattention to interacting bio/geo processes was attributed, by a participant, to the domination of process geomorphology in the second half of the twentieth century.

[53] Though see Phillips 2007 for a discussion of landforms and landscapes as 'circumstantial, contingent results of deterministic laws operating in a specific environmental context, such that multiple outcomes are possible' (2007: 159).

In order to investigate how the spaces of scientific inquiry were reworked, since the 1980s, to accommodate the interdisciplinary research objects of biogeomorphology, part one of the analysis began by considering the 'reconfiguring' and 'articulating' work carried out by geomorphologists such as Heather Viles. 'Reconfiguration' and 'articulation' were understood as connected to the concept of 'research entrepreneurship' introduced in chapter five. The first term, 'reconfiguration' investigated work undertaken to bring 'marginal practice[s]' to the centre (Spinosa et al. 1995: 3), while the second examined work whereby 'dispersed or confused practices are brought into clearer focus' (Spinosa et al. 1995: 3). It was noted however, that processes of articulation were followed by fragmentation and diversification, with a proliferation of terminology, and associated fields of research, considering 'bio'/'geo' interactions. This fragmentation was in turn followed by attempts at rearticulation.

The contested nature of the various terms, used to denote 'eco'/'geo' interactions, was discussed through a consideration of the term 'ecosystem engineers'. It was noted that one ecologist described work undertaken under the banner of this term as being marked by 'vaguely defined terms', 'loose analogies' and a perceived 'over-generality' (Fox 2011: n.p.). This prompted the question of whether such terminology might be considered through Star and Griesemer's term 'boundary object'; this denotes an entity that is '… adaptable to different viewpoints … and robust enough to maintain an identity across them' (1989: 387). However, it was noted, following Donaldson et al., that the term 'boundary object' implies an essentially 'epistemological' approach (2010: 1532) whereby a single object is subject to multiple points of view; instead, following Mol (2002), Donaldson argued for an ontological approach positing 'different, partially connected objects' (Donaldson et al. 2010: 1532). The relationship between the terminology describing a field of research and the 'real and historical' (Daston 2000: 3) research objects that emerge from discursively-shaped spaces of research, was then examined.

The second part of the chapter drew on an interview with a senior biogeomorphologist. It began, as part one concluded, with attention to the discursive; it noted a shift in publishing regimes enabling the accommodation of interdisciplinary research objects. It continued with a consideration of a disciplinary movement described by the interviewee in terms of a 'wave'; the participant commented on their experience of being ahead of the wave in the early stages bioegeomorphological research, then on the crest of the wave at the present time. The analysis continued by returning to materials briefly examined in chapter four; it noted how chance circumstances, occurring on social as well as physical dimensions, shape the spaces of scientific inquiry. In this way symmetrical social and natural

processes are interwoven in the production of both the spaces, and research objects, of scientific inquiry. Not only the ‘geo’ and the ‘bio’ is in play in biogeomorphological research, but also the social. In this way scientists participate in the ‘disclosing [of] new worlds.

CONCLUSION

This thesis has examined two research questions:

(i) ‘How do research objects withstand displacement amid the spaces of scientific inquiry?’

(ii) ‘How are the spaces of scientific inquiry reworked to accommodate interdisciplinary research objects?’.

To address these questions, the study employed an ethnographic methodology to investigate two case studies: rock weathering and fluvial geomorphology. These case studies were linked by, and the second case study emerged from, additional exploratory research focusing, in particular, on the British Society for Geomorphology (BSG). Another linkage between the case studies was formed by the examination, in chapter seven, of research undertaken in the field of biogeomorphology; attending to interactions between ecological and geomorphological processes, biogeomorphological research is conducted in both rock weathering and fluvial geomorphology.

The research has several empirical and theoretical strengths. Empirically, it provides a valuable study of contemporary geosciences - an area where the Science and Technology Studies (STS) literature is sparse. By focusing on contemporary and recent (late twentieth and early twenty-first century) geomorphology, an interdisciplinary field of research found in both geography and geology departments, it offers an important contribution to the histories of geography literature and the literature on interdisciplinarity. Further, and relatedly, through its attention, in chapter seven, to the interdisciplinary field of biogeomorphology, the study provides a bridge between existing literatures on the biosciences and nascent explorations of the geosciences. Theoretically, the research offers an incisive critique of Actor-Network Theory (ANT) analyses. It challenges ANT conceptions of space, offering an alternative account of space that overcomes insidious dualisms that polarise sign and signified. In addition, it develops a historically sensitive approach, attending to genealogies of techniques and biographies of individuals; an approach that has been marginalised in ANT analyses.

Chapter one situated the study in relation to the eighteenth and nineteenth century roots of geomorphology. It considered the opposition between indoor (laboratory and museum) and outdoor (field) science. It was noted that this opposition has been replicated and extended in the ANT literature. A series of contrasting categories were identified: field versus academy, indoor versus outdoor, place versus placeless, particular versus

universal and material versus text. It was noted that underlying these oppositions there remain the vestiges of a Euclidean account of space. It was argued that an alternative conception of space, developed in this thesis, would enable a better understanding of the rich configurations that characterise spaces of scientific inquiry.

The introduction continued with a consideration of the research objects of geomorphology. It was argued that despite the promising theoretical accounts of Daston (2000) and Latour (2000), Latour displayed a lack of curiosity regarding the histories of specific research objects. Drawing on Latour, however, it was argued that research techniques may be understood as a component of, and proxy for, research objects that are otherwise difficult to gain a handle on. This provided a rationale for examining the genealogies of specific research techniques. In this way, it was argued, the thesis addresses both conceptual and empirical deficits in ANT accounts.

The introduction returned to the nineteenth century to consider how the research of figures such as Cuvier and Darwin transcended emergent disciplinary boundaries. The complex roots of geomorphology were discussed and the continued interdisciplinary nature of geomorphological inquiry was highlighted.

Chapter two was orientated around the three themes identified in the introduction: space, research objects and interdisciplinarity. It provided a review of the relevant literatures associated with each theme and situated the thesis in relation to existing theory and research. It concluded with a discussion of the limited existing STS research on the geosciences; four fleeting mentions of geomorphology in the literature were examined.

Chapter three set out the methodological approach adopted in this thesis. It explained the rationale for selecting ethnography as the core method employed. It continued to set out a number of influences on the research design, a discussion of multi-sited ethnography and the selection of two case studies supported by additional research. The ethnographic methods of participant observation, interviews, and analysis of texts were discussed. This was followed by a discussion of research ethics and the positionality of the ethnographer.

In order to begin examining the first research question, chapter four provided a conceptual apparatus for thinking about spaces of scientific inquiry. Focusing on spaces of fieldwork in geomorphology, the chapter began by challenging an opposition between a detached, analytical examination of maps (and by extension, models, datasets etc.), and an embodied, exploration of a field site; this distinction was made in Latour's account of fieldwork (1999a) and was echoed elsewhere in the literature (Paterson 2007). In

contrast with this opposition, it was noted that geomorphologists do not explore the field empty handed; one participant described using a metre rule, the latter functioning like a third leg or walking stick when working in rivers. In addition, aerial photographs were listed as important field equipment. Another participant described their experience of going into the field alongside modellers, while another described undertaking exploratory work, carried out using remotely sensed datasets and a Geographical Information System (GIS) package, in preparation for fieldwork. Derrida's term 'supplement' (2001: 366) was employed to consider how maps, models, aerial photographs, and remotely sensed datasets (signifiers) function as 'supplements' to the landscape (signified). The term 'supplement' has a dual meaning: the 'supplementary' signifiers may appear auxiliary to the signified but they also indicate a lack within what might otherwise be considered complete in and of itself: the landscape. This interaction of signifier and signified was seen as having a symmetrical ontology, producing augmented, informed (Barry 2005), spaces of fieldwork.

The second part of chapter four continued the consideration of the spaces of fieldwork. It examined the way that fieldwork is situated by a range of dimensions not limited to physical distance or proximity. The social and disciplinary proximity or distance associated with the spaces of fieldwork were also considered. Similarly, spaces of fieldwork were understood as situated by the presence and absence of other field sites and the informational dimension of texts and remotely sensed data. Finally, the spaces of fieldwork were understood as shaped by the timeframes of a particular research project and the nesting of one project in relation to others.

The chapter concluded with a critical reflection on the surprisingly linear Latourian account of reference in *Circulating Reference* (1999a). It argued that, through attention to the interweaving of signifier and signified, in the production of the spaces of fieldwork, it is possible to move beyond Latour's opposition of 'compatibility', 'standardization', 'text', 'calculation', 'circulation', and 'relative universality' on the one hand and 'locality', 'particularity', 'materiality', 'multiplicity' on the other (1999a: 71).

However, for the purposes of this study, one of the most important outcomes of chapter four was the argument that spaces of fieldwork are situated by a disciplinary dimension, and a range of other dimensions, as well as by physical distance and proximity. This finding provided a basis for investigating the first research question in chapters five and six. With the spaces of scientific inquiry reconceptualised as being shaped by disciplinary

dimensions, the notion of displacement changed to consider interdisciplinary displacements or, in Spinosa et al.'s terms (1995: 3) 'cross-appropriation[s]'.

The question of how research objects withstand displacement amid scientific spaces was seen to take forward classic ANT themes. As seen above, spaces were conceptualised as shaped by a disciplinary dimension. Further, rather than focusing on the displacement of research materials, there was a focus on the displacement of research *techniques*, understood as a component of, and proxy for, research objects. The focus was therefore on the displacement of techniques amid disciplinary dimensions of the spaces of scientific inquiry.

To investigate this, chapters five and six focused on research conducted by members of the Rock Breakdown Laboratory in the School of Geography and the Environment, University of Oxford. In order to understand the displacement of techniques, amid disciplinary dimensions, these chapters were both anchored in relation to an ethnographic vignette; this described a member of the Rock Breakdown Laboratory carrying out experimental work at a local field site. Some parallels with Latour's description of a series of transformations of research materials (1999a: 73) were noted; however, Latour's account was criticised as (i) putting forward an impossibly neat and linear account of the relation between field and text and (ii) lacking a finely textured account of the genealogies of the techniques encountered. In contrast to Latour's account, chapter five sought to trace the genealogies of techniques observed during participant observation; this provided a means to consider the displacement of techniques amid disciplinary differentials.

Before proceeding to consider the genealogies of techniques there was a brief consideration of the nature of geomorphological research objects and a discussion of the geomorphological literature on the history of geomorphology. Following this, section two of chapter five examined the movement of people and ideas between Columbia, USA and the University of Cambridge and then Oxford, UK. It complemented and expanded upon Barnes's (2008a: 14) brief discussion of Arthur Strahler's contribution to the quantitative revolution in geography. Following a story that is well-rehearsed in the geomorphological literature (e.g. Schumm 2004), it followed Strahler's 'cross-appropriation' (Spinosa et al. 1995: 3) of 'concepts' and 'information' (Strahler 1992: 62) from Horton's (1945) quantitative analysis of drainage systems. It continued to follow the story of Richard Chorley's work under Strahler in the USA and his subsequent return to the UK where, following a brief period at Oxford, he settled at Cambridge in the department led by Alfred

Steers. It was in this way that, according to Haggett, 'it was at Cambridge … that the revolutionary fuse lit by Strahler was to explode in the sixties' (2002: 523).

Until this point in the analysis the genealogies traced were focused on ‘concepts’, ‘ideas’, and ‘people’ rather than techniques. In addition, the story was a neat one, lacking the attention to ‘multiple … indirect, fragmented … lines of descent’ (Sherratt 2006: 136) associated with a Foucaultian approach to tracing genealogies. This began to change, however, towards the end of part two of chapter five and as the analysis progressed into part three. There was a successive movement of three geomorphologists, Marjorie Sweeting, Andrew Goudie, and Heather Viles, from Cambridge to Oxford; this coincided with a gradual change at Oxford with the development of a laboratory, a shift towards quantitative methods and an increased emphasis on publication. Indeed, the analysis considered Sherman’s contention (1996) that Goudie might be considered as a successor to Strahler. This appeared as a simple story of diffusion of ideas associated with the movement of individuals between academic institutions. However, closer attention to Goudie’s early work, and to his rapport with his supervisor, Richard Grove, who was sceptical to Chorley’s new ideas, revealed a different, more nuanced story.

Another aspect of the argument of the thesis, introduced in chapter five, was use of the term ‘research entrepreneurship’ to describe the qualities of the geomorphological and related interdisciplinary research studied. The term, adapted from Spinosa et al.’s discussion of entrepreneurship (1995), was employed to highlight the constant opening out of research possibilities associated with evolving experimental spaces and practices; that is the ‘world-disclosing’ (Spinosa et al. 1995: 3) potential of scientific inquiry. Three aspects of entrepreneurship, identified by Spinosa et al., were discussed: cross-appropriation, reconfiguration and articulation (p. 3). However, this chapter focused on just one of these: cross-appropriation. Understood as a synonym for ‘displacement’, ‘cross-appropriation’ refers to a process whereby ‘one domain of practices takes over useful practices from another domain’ (p. 3). The processes of reconfiguration and articulation were treated in chapter seven.

This analysis has a merit, unusual in science studies, of incorporating historical attention to the genealogies of techniques. In this way the chapter provides a valuable empirical contribution to the histories of geography literature. It is argued that such an approach is essential to understanding how ‘persistence [of research objects] … depends on … the degree of [their] embeddedness in such organized systems of techniques and instruments’ (Daston 2000: 13). This provides an historically inflected answer to the

question of how techniques, understood as components of research objects, withstand displacement amid the spaces of scientific inquiry. Despite the apparent diffusion of ideas and movement of people, this displacement is not considered as being primarily relative to physical space. Instead it is the displacement of techniques amid *disciplinary* differentials that formed the key focus for discussion.

Chapter five explored the genealogies of techniques discussed in an ethnographic vignette about field experiments undertaken by a member of the Rock Breakdown Laboratory at the University of Oxford. This analysis provides a way to think about the displacement of techniques, understood as a components of research objects, amid disciplinary differentials of the spaces of scientific inquiry. Chapter six returned to the same vignette to consider the same question from a different point of view. It examined the role of a range of actors in enabling the safe displacement of techniques amid the spaces of scientific practice. The chapter concluded with a reflection the potential for tacit knowledge to be held not only in human bodies but in what Srnicek (2013: n.p.) termed 'cognitive assemblages'.

Chapter seven built on the analysis in chapters five and six to address the second research question: 'How are the spaces of scientific inquiry reworked to accommodate interdisciplinary research objects?'. This question was addressed through a consideration of the interdisciplinary field of biogeomorphology. The chapter began with two accounts by fluvial geomorphologists, separated by a century, describing the role played by vegetation in rivers. While these accounts bore a striking resemblance to one another it was noted that during the intervening period such accounts were rare. A participant explained that, in their view, this was due to the domination of process geomorphology since the mid-twentieth century. The remainder of the chapter considered the period, since the 1980s, whereby such accounts once again became possible within and beyond geomorphology.

Part one of the chapter seven returned to the term 'research entrepreneurship' introduced in chapter five. Where chapters five and six considered 'cross-appropriation' as an aspect of the 'world-disclosing' quality of entrepreneurship, chapter seven focused on 'reconfigur[ation]', 'which makes a marginal practice central', and 'articulati[on]', 'whereby dispersed or confused practices are brought into clearer focus' (Spinosa et al. 1995: 3). Work by Heather Viles, reconfiguring aspects of geomorphology by bringing marginal practices to the centre, was investigated. This was followed by a consideration of 'articulati[on]', whereby dispersed or confused practices are brought into clearer focus';

attention was paid to the way spaces are discursively reworked to accommodate interdisciplinary research objects. However, it was noted that in the period following the introduction of the term 'biogeomorphology', in a book of the same name (Viles 1988), a proliferation of terminology emerged including 'ecogeomorphology' (Wheaton et al. 2011), 'zoogeomorphology' (Butler 1995, Butler 2012) and 'ecosystem engineering' (Jones et al. 1994, Jones et al. 1997). In this way processes of 'articulati[on]', whereby dispersed or confused practices are brought into clearer focus' (Spinosa et al. 1995: 3), were followed by fragmentation, diversification and then further attempts at re-articulation.

In considering the proliferation of terms associated with nascent fields of research into interactions between ecological and geomorphological processes, it was noted that the meaning of terms, and their relationship with emergent fields of research, was not uncontested. Taking the example of 'ecosystem engineering', differences of opinion concerning the value of the term were explored. One ecologist described the term as being marked by 'vaguely defined terms', 'loose analogies' and a perceived 'over-generality' (Fox 2011: n.p.).

This prompted a consideration of whether a given term describing a field of interdisciplinary research might be considered using Star and Griesemer's term 'boundary object'; an object that is '… adaptable to different viewpoints … and robust enough to maintain an identity across them' (1989: 387). However, the analysis heeded Donaldson et al.'s comment that the term 'boundary object' implies an essentially epistemological approach (2010: 1532). It therefore considered their proposal, following Mol (2002), of an ontological approach positing 'different, partially connected objects' (Donaldson et al. 2010: 1532). The relationship between the terminology describing a domain of research and the 'real and historical' (Daston 2000: 3) research objects emerging from those configurations was then probed. It was argued that the terminology and associated fields of research shape the spaces within which research objects emerge, extending and multiplying a discursive dimension of these becomings. Further, it was argued that in this way such spatial dimensions become constitutive of the research objects that emerge from them; to paraphrase Latour (2000: 260) we do not have to consider research objects '… as being radically different from a context made up of colleagues, emperors, money, instruments, bodily practices…' and, presumably, texts.

Part two of chapter seven focused primarily on an interview with a senior geomorphologist who investigates biogeomorphology in fluvial systems. It began where part one ended, with reference to the discursive; a shift in publication regimes enabling the publication of

biogeomorphological work in ecology journals. The interviewee continued, however, to describe a disciplinary movement, associated with fluvial biogeomorphological research, in terms of a 'wave'. At first they experienced being ahead of the wave – finding that their work was 'dismissed as being trivial' (Interview with Aidan, February 2015). Later in the development of the discipline, however, they described being 'on the crest of the wave rather than ahead of it' (Interview with Aidan, February 2015). The final section of chapter seven returned to consider materials touched on in chapter four; once again, the role of chance circumstances was noted in the social and physical dimensions shaping the spaces of geomorphological inquiry. The symmetrical, and interwoven configurations of social and natural, science and environment were acknowledged. Biogeomorphology was seen not only as connecting the *geo* (earth) and the *bio* (life) (Whatmore 2013: 161); instead there was a sense of the possibility that in biogeomorphological research human researchers participate in the geo/bio/social becomings of the world. Returning to Spinosa's terminology, it was seen that these scientists 'disclos[e] new worlds' not through their lively analysis of an otherwise mute, lifeless matter; instead they participate in the 'spacing' (Ek 2012: 42), or 'splacing' (Doel 1999: 9) of a lively world.

Nigel Clark describes a tension, inherent in contemporary writing on the 'geo', between the recognition of human agency folded into the 'earth processes' and the recognition that '"our" moment of planetary significance is likely to be brief and bounded (2013: 49). The first of these two opposing pulls is found in what Clark terms a 'relational ontology' whereby 'any object of concern is never simply given, but is always constituted out of particular sets of practices, strategies, apparatuses and dispositions' (2013: 48). Similarly, it has found succour in the 'Anthropocene thesis' '…add[ing] weight to the idea that there is no nature worth speaking of which is external to the domain of human activity' (2013: 49). The second argues that 'the major earth shaping forces remain impervious to our touch' (2013: 49). This thesis conforms to Clark's recommendation that he would 'much rather see this tension there – pulsing away productively – than feel it was being sidestepped or evaded'. It begins with a chapter that argues that the spaces of geomorphological fieldwork are always already augmented by the human. It concludes with attention to disciplinary becoming, and the way scientists participate in evolving geo/bio/social configurations. Perhaps such an analysis is not too removed from that of the geomorphologists, Baker and Twidale, who comment:

> We see Geomorphology as an holistic, chronological, integrative field-based science, that is integral to the study of a dynamically vibrant planet. We look forward to the time when geomorphologists … will experience the excitement of

> unraveling the development of landscapes through time, of *conversing with the landscape and responding to its promptings*. We trust that in the not too distant future Geomorphology will again be seen as an integral part of, on the one hand, genetically-oriented holistic studies intent on understanding the present spatial distribution and interrelationships of earthly phenomena, and, on the other, of studies concerned to analyze and explain the evolution of the Earth through time (1991: 95).

This passage is quoted in full to bring out another point. Baker and Twidale's comment concludes with a consideration of the (interdisciplinary) future of geomorphology. In bringing this thesis to a close, and reflecting on opportunities for future research, it is helpful to turn to reflections on the future of 'geomorphology'. First, however, the next section provides some concluding comments on the contributions of this thesis.

CONCLUDING COMMENTS

INTEGRATING A GENEALOGICAL AND BIOGRAPHICAL APPROACH WITH ACTOR NETWORK THEORY (ANT)

As Powell argues, Actor Network Theory (ANT) is 'the most successful posthumanist approach' (2007a: 317). Similarly, Barnes comments that ANT is 'the best-known variant' of science studies and 'certainly the version most commonly found in geography' (Barnes 2014: 207). The dynamism associated with ANT studies, and the value of its posthumanist approach, brings much that was previously missing to science studies and the geography of science. However, in addition to the shortcomings of ANT-accounts of space engaged with in this study, an additional weakness of ANT accounts is tackled: for all the value of attending to non-human and material agency in contemporary scientific configurations, there can be a lack of attention to (i) the genealogies of specific configurations and (ii) specifically human biographies that are woven through them. As seen in the introduction, in his description of scientists at work in the Amazon region, Latour comments:

> Remove both [their] maps, confuse cartographic conventions, erase the tens of thousands of hours invested in Radambrasil's atlas, interfere with the radar of planes, and our four scientists would be lost in the landscape and obliged once more to begin all the work of exploration, reference making, triangulation, and squaring performed by their hundreds of predecessors. Yes, scientists master the world, but only if the world comes to them in the form of two-dimensional, superimposable, combinable inscriptions. It has always been the same story, ever since Thales stood at the foot of the pyramids (Latour 1999a: 29).

Despite this reference, Latour's curiosity is not sufficiently piqued to investigate more fully the genealogies of techniques employed in the field. In this study, however, there is a strong emphasis on this historical dimension. For example, chapters five and six investigate the genealogies of techniques encountered in contemporary experiments undertaken at the University of Oxford's Rock Weathering Laboratory.

As well as a lack of emphasis on the history of techniques in ANT-influenced science studies, there is, concurrently, a decisive shift from the discursive towards the consideration of material configurations. This may be seen in an early comment by Latour:

> I was struck, in a study of a biology laboratory, by the way in which many aspects of laboratory practice could be ordered by looking not at the scientists' brains (I was forbidden access!) … but at the transformation of rats and chemicals into paper (Latour 1986: 3-4).

This emphasis in science studies on practice (what scientists do), rather than discourse (what scientists say) has delivered many valuable insights. However, as chapter four of this study demonstrates, the discursive and the material are woven together in the spaces of scientific inquiry. As such it is as wrongheaded to dismiss scientists, their brains, and their discourses from the correct subject matter of science studies as it is to forgo attention to practice, non-human actors, and the material. If scientists' brains are not directly observable in the context of ethnographic research, it does little justice to the intelligence of scientific colleagues, and their readiness to engage in often highly-reflexive reflection, to assume that what they have to say is of little interest to the study of science. A valuable corrective emphasis, found in the work of Powell (2008a) and Barnes (2001a) draws attention to the importance of biographical research in science studies. This human and historical dimension may be incorporated without contradiction into work that, like this study, engages deeply with the valuable lessons of ANT, without replicating the weaknesses associated with this tradition of inquiry.

DIVERSIFYING CONCEPTUAL RESOURCES

In common with much ANT-inspired work, throughout this thesis theory has played a valuable part in 'thinking through research' (Pryke et al. 2003: 1). Along with ANT, there have been a number of parallels, and resonances with assemblage theory in the conceptualisation, analysis and writing of the research. Assemblage theory, like ANT, is best understood as a set of overlapping ideas rather than a coherent body of theory (see Anderson and McFarlane 2011: 124); however, a number of key aspects of assemblage

theory may be identified. Like ANT, assemblage theory describes socio-material configurations, or assemblages, as being 'composed of heterogeneous elements that may be human and non-human, organic and inorganic, technical and natural' (Anderson and McFarlane 2011: 124, see also Anderson et al. 2012:178 for a discussion of the relationship between assemblage theory and actor-network theory). In addition, as Anderson and McFarlane note, 'the term [assemblage] is often used to emphasise ... multiplicity and indeterminacy...' (2011: 124). This resonates with the emphasis in this thesis, drawing on Barry and Born, on interdisciplinary configurations 'less as a unity and more as a field of differences, a multiplicity' (2013: 5). Again, the language of assemblage theory resonates with the theoretical approach of this thesis in its emphasis on an ontology of fragmentation. As Anderson and McFarlane note, in assemblage theory 'an emphasis is placed on fragility and provisionality; the gaps, fissures, and fractures that accompany processes of gathering and dispersing' (2011: 125). Here there are parallels with the emphasis on tracing geneaologies characterised by ‘multiple’ and ‘indirect, fragmented, dislocated lines of descent’ (Sherratt 2006: 136). Finally, where the penultimate chapter of this thesis draws on a Deleuzian ontology of 'becoming' (De Landa 1998: 29), this unsurprisingly resonates with the emphasis in assemblage theory on 'emergence' (Anderson and McFarlane 2011: 124).

However, despite these lively theoretical convergences, assemblage theory is not turned to as providing an overarching theoretical framework for the study. To do so would contravene Marcus's vital advice to would be ethnographers: 'Ethnographic projects that are heavily motivated by and cast in culture theory terms must be allowed to "breathe", especially in terms of their descriptive accounts of things, before the theory kicks in.' (1998: 18). To give the empirical descriptions of this thesis room to breathe an overarching theoretical framework is eschewed; instead the rich theoretical resources of continental philosophy and recent social theory drawn on in a piecemeal fashion more in tune with the patchwork ontology characterising post-modern and post-structuralist resources. In this way, and without contradiction, the study draws on Ingold (2011) and Wylie (2005) to '[think] through' (Pryke et al. 2003: 2) the haptic, embodied dimensions of fieldwork; Derrida's discussion of 'supplementarity' (2001) to consider how the human and non-human inter-relate to produce always already 'informed' (Barry 2005), multi-dimensional spaces of fieldwork; Deleuze and Guattari’s discussion of the relationship and transformation between ‘smooth’ and ‘stratified’ spaces to consider the opposition espoused by Latour (1999a), and challenged in this study, between an embodied movement through the field and an abstract gaze from above; and, as discussed above, a

Deleuzian ontology of becoming is drawn on to describe the wave-like evolution of geo-bio-social configurations associated with the interdisciplinary field of biogeomorphology. This has the additional advantage of the study serving, not only as a moment in 'dialogues' among human and physical geographers, and as a bridge between STS studies of the 'bio' and nascent STS research into the geosciences, but also as contributing a series of threads to rich efforts to '[think] through research' (Pryke et al. 2003: 2) using, without being beholden to, the resources of recent social theory.

EARTH SCIENCES AND THE ANTHROPOCENE

The balance, between the theoretical and empirical, inherent in Pryke et al.'s subtitle 'thinking through research', is also at the heart of the response this thesis offers to recent human geographical scholarship on the 'geo' and 'anthropocene'. Thinkers such as Yusoff and Clark have devoted considerable energy to distilling valuable insights from the theoretical resources of post-structuralism (see for example Yusoff 2017). However, one deficiency of such work is its failure to balance the theoretical and the empirical. Through a study of geomorphology, this study seeks to begin rectifying that balance.

In addition, it is interesting to note that Yusoff's analysis of the anthropocene, drawing on Deleuze and Guattari's discussion of Doyle's fiction geologist 'Professor Challenger' (Yusoff 2017: 2), understandably emphasises a conflictual relationship between the human subject 'anthropos' and the earth. This account portrays the fact that humanity's ability to pour scorn upon the earth knows few bounds; equally, it describes the ability of the earth to assault viciously the life that clings to it. In contrast, however, in its study of some aspects of geomorphology and biogeomorphology, chapter seven of this study introduces a different register; it describes the geo, the bio, and the social in a more benevolent, indeed 'reciprocal' (Yusoff 2017: 3), relationship. This is not to argue that Yusoff's account is incorrect; in the context of debates on the Anthropocene such a case would be difficult to make. Instead, this thesis supplements Yusoff's account of mutual destruction with a description of another, creative form of eventful relation among humans, non-humans, and the geo.

FUTURE RESEARCH AVENUES:

In order to decipher future avenues for research this conclusion returns to consider the enigmatic word that has guided this research project: 'geomorphology'. This is the term that was used by the majority of the scientists encountered in this study to describe their disciplinary affiliation. It should be noted anecdotally, however, that a few scientists, particularly those undergoing graduate training or at an early stage in their careers,

appeared to distance themselves from this term. While in chapter seven there was an analysis of the diverse terms used in parallel with the term 'biogeomorphology' this study has not undertaken a similar analysis of terminology applied to describe 'geomorphological' areas of inquiry. Chapter one saw some initial discussion of the complex roots of 'geomorphology' in the eighteenth and nineteenth centuries; it was noted that the eighteenth-century term mineralogy had a broader meaning than the contemporary science of the same name and, like the nineteenth century geology that followed it, it would have encompassed what has been termed, throughout this study, 'geomorphology'. The remainder of this conclusion provides a reflection on some snippets of empirical material gathered in the course of the research but not explored further within this study; this material provides possible indications for future research.

While human geographers have awakened, recently, to the notion of humans as 'geomorphic agents' (Yusoff 2013: 781, see also Clark 2013: 49, Dalby 2007: 112 Dalby 2014: 7), some geomorphologists[54] have started to reflect on the longevity of their discipline and the term 'geomorphology' (Woodward 2015, see fig 1). In their 'Introduction to the Discipline of Geomorphology', Gregory and Goudie present a table that charts the development of geomorphology through periods of 'youth' (pre-1900), 'maturity' (1960-2000, 'old age' (2000-) and 'rejuvenation' (2010-) (Gregory and Goudie 2011: 15, see fig. 11). Gregory and Goudie's attention to the lifetime of geomorphology as a discipline was echoed by Jamie Woodward during the 2014 Frost Lecture at the annual meeting of the British Society for Geomorphology (Woodward 2014), and in a later paper based on this lecture (Woodward 2015). Woodward uses Google N-gram (see fig. 12) to plot the usage of terms including 'geomorphology' and 'landforms'; his analysis shows an apparent decline in the use of these terms during recent years.

[54] While others see evidence of a contemporary 'geomorphological turn' arguing that 'earth scientists, and particularly geophysicists, have decided that geomorphology matters' (Summerfield 2005a: 779).

Table 1.6 Discipline growth applied to geomorphology

Stage of growth of discipline (adapted after Jensen and Dahlberg, 1983)	*Application to geomorphology*
Preliminary growth period with small absolute increments of literature and little or no social organization	**Youth:** pre-1900, with origins in geology as well as in geography
	1900–1960: a period when geomorphology grew so that by the 1960s although some believed maturity to have been accomplished, the emphasis upon long-term landscape evolution meant that insufficient attention had been given to processes, to other branches of geomorphology and the relations of geomorphology to other disciplines
A period of exponential growth when the number of publications double at regular intervals and specialist research units are established	**Maturity:** 1960–2000, substantial growth achieved as illustrated by many new journals (Table 1.4) and books reflecting the branches of geomorphology (Table 1.5), with the influence of systems, models, quantitative and statistical methods and remote sensing
A subsequent period when the growth rate begins to decline and although annual increments remain constant, specialization and controversy increase	**Old age:** 2000– , growth rate may have declined but multi-disciplinary research has progressed involving geomorphologists, being exemplified by hybrid branches of the subject
A final period when the rate of growth approaches zero, specialist research units and social organization break down, and the subject reaches maturity	**Rejuvenation:** 2010– , a new phase where the role of geomorphology is redefined, enhanced by new techniques and potentially a more vibrant holistic and resilient discipline

Figure 11: A table from Gregory and Goudie (2011: 15) presenting the development of geomorphology through periods of 'youth', 'maturity', 'old age' and 'rejuvenation'.

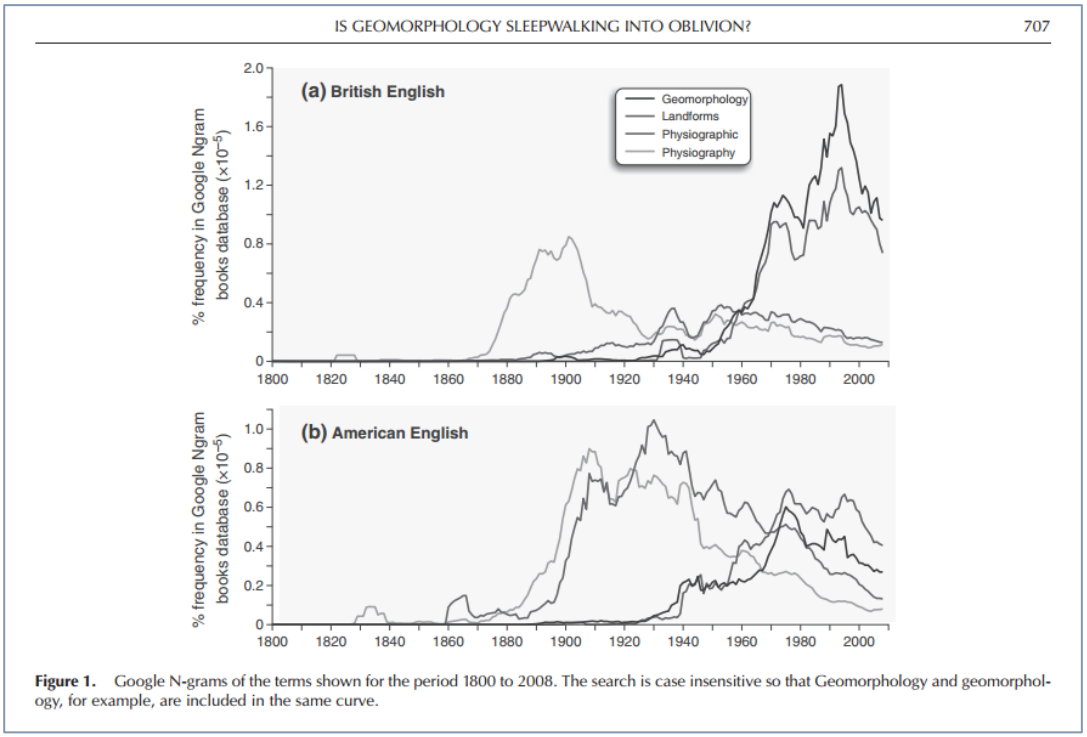

Figure 1. Google N-grams of the terms shown for the period 1800 to 2008. The search is case insensitive so that Geomorphology and geomorphology, for example, are included in the same curve.

Figure 12: Google N-grams of the terms (Woodward 2015)

It is not possible to say how many geomorphologists would concur with Woodward's analysis; perhaps some would instead concur with Gregory and Goudie's argument that

Geomorphology is undergoing a period of ‘rejuvenation’ (2011: 15). Some comments made during the course of interviews, including the interview with Andrew Goudie, point to an interesting moment in the disciplinary evolution of geomorphology. Firstly, an interview with Goudie highlighted a concern regarding the possible decline of the term ‘geomorphology’, although this was tempered by discussion of the possibility that people who are doing ‘geomorphology’ may be carrying out their activities under a different disciplinary name:

> Andrew Goudie: well it's interesting - I was over in Cambridge Saturday before last - erm - fifty years on - when I was an undergraduate - we had a reunion - and we had a talk from the head of department - and he showed us what they do in their courses now - and the word geomorphology did not appear
>
> Ethnographer: ok
>
> Andrew Goudie: so you know they have quaternary and environmental stuff and they have glaciology and they have volcanism - but geomorphology itself does not appear as a specific field of study - for the undergraduate course - which slightly annoyed me
>
> Ethnographer: yeah, yeah
>
> Andrew Goudie: I mean they're obviously doing geomorphology - and they're doing some very good geomorphology - but the number of geomorphologists in some British universities has plummeted … so - I worry in one sense that geomorphology is not flourishing in the UK – erm, but as you say there are a lot of people doing things that are geomorphology but they call themselves something else
>
> Ethnographer: yeah. Does it - does that matter?
>
> Andrew Goudie: probably not - so long as the quality of the work - I mean geomorphology's just a term anyway - I mean it was invented - erm - quite a long time ago but people were doing perfectly good - what we call geomorphology before the term was invented so it's not the end of the world if people erm - and you know the - - European Geophysical Union and all these bodies - they they they're lots of people are doing geomorphology - it's often called earth processes group or something of that sort - they think it sounds sexier probably don't they
>
> Ethnographer: yeah yeah yeah
>
> Andrew Goudie: but they're still doing geomorphology
>
> Ethnographer: yeah
>
> Andrew Goudie: and there's a lot of it there, and there's a lot of it in the AGU [American Geophysical Union], a lot of it in the GSA [Geological Society of America], erm - - the AAG [Association of American Geographers] has a

> geomorphology speciality group - they use the name - - erm - but it's something that's got to be watched I think - got to be watched.
>
> (Interview with Andrew Goudie, October 2014)

Another interviewee, a mid-career researcher, reflected on a decline in membership of the British Society for Geomorphology, attributing this to the migration of scientists into other related disciplinary fields in pursuit of emerging techniques. They commented:

> One thing the BSG [British Society for Geomorphology] has been struggling with has been membership numbers over the last x number of years and I think part of that is down to dating techniques - there was you know a kind of revolution in our ability to date things and I think a lot of people went out there and became - you could call it a kind of terrain chronologists maybe more than they are geomorphologists in a land forming sense and they all joined the QRA [Quaternary Research Association] and drift away from BSG and I think BSG has had a problem in you know maintaining people who saw their primary allegiance to this family of people who got very skilled in undertaking different forms of stable isotope or radio nuclei dating you know it suddenly exploded beyond carbon dating into things which were much shorter resolution - you know higher resolution - shorter half life - data sets are some which have allowed a whole new realm of studies - rather than having something at a hundred year level of accuracy you're now into well years months maybe - and one of the things … I was thinking about … is one of the problems geomorphology has - and it's lost a number of people who might have otherwise thought of themselves as geomorphologists but think of themselves as quaternary scientists because they're not actually - the dating of materials became the end point rather than the means to an end which is to understand the formation of landscape which is kind of true geomorphology - and there was a couple of calls and I can probably put a number - I can't remember who mentioned it but somewhere in my conference notebook … a couple of times in the conference there was - maybe I just heard this because i wanted to - there was a kind of implicit call that dating should come back within geomorphology and now we've got those things back under control a bit now we need to start understanding the landscapes again - using the dating information to understand the landscapes a bit more - maybe it's more my hope but bring those people who use the dating tool as just a tool – I mean I think it's pretty obvious and perhaps self-evident that for a decade when a new technology comes in everyone runs off and plays with it and explores and pushes it and explores every nuance of that particular data source and I think that happened for a while but now maybe there'll be a slow reversion back where people who are the experts for that particular technology are looking for ways to kind of apply that technology to land forming issues rather than just being an expert in that technology (Interview with mid-career geomorphologist, October 2014).

In terms of future research, the important point is not whether ‘geomorphology’ is in decline or in rude health; instead, the key point to distil from such debates concerns what

might be termed the ‘morphology’ of (inter)disciplines. History is an important dimension of such inquiry and through adopting an ethnographic methodology that is attuned to genealogies such subtle disciplinary formations can be deciphered.

In innovation studies, recent research has sought to examine the ‘destabilisation of existing regimes’ as an important ‘flipside’ to ‘energy transitions’ towards a greener power supply (Turnheim and Geels 2012: 35). Turnheim and Geels comment:

> While the transitions literature is dominated by studies of emerging innovations, this paper has demonstrated that the destabilisation of existing industries is an equally important (and interesting) parallel process (2012: 47).

Turnheim and Geels’s attention to industries undergoing perceived decline is instructive and a parallel might be made in science studies. It can be tempting as STS scholars to follow the money and kudos surrounding sciences that are in the ascendency at a particular moment. However, there is much to be learned from attending to their less obvious counterparts.

This study has advocated the investigation of research techniques, the latter understood as a hybrid (human and non-human) proxy for, and component of, ‘real and historical’ (Daston 2000: 3) research objects. Future research could continue to investigate the relationship between geomorphology and related disciplines by focusing on specific techniques and tracing their presence across related disciplinary formations. For example, this could take up the example of dating techniques, suggested in the mid-career researcher’s comments. A future study could be initially focused on mid-career researchers (the generation whose careers have been shaped by the acquisition of new dating techniques) who are members of the BSG. It could be rapidly expanded to engage with members of related learned societies including the Quaternary Research Association, the European Geophysical Union and other relevant associations. An ethnographic methodology could be complemented by literature mapping techniques (Rafols and Meyer 2010) that allow examination of the disciplinary affiliation of articles discussing particular research techniques. Additional mapping techniques may be hinted at in Landström’s (2015) work to map the relationships between a range of actors including non-human agents.

In addition, research could be undertaken to deepen the theoretical understanding of space developed in chapter four; participant observation could be used to further explore the interweaving of signifier and signified in field research. Attention could focus on remote sensing techniques such as airbourne LiDAR and field techniques such as

terrestrial LiDAR. The relationship between numerical modelling and fieldwork could also be examined in more detail.

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APPENDIX: CONSENT FORM AND CONSULTATION DOCUMENTS

Note: where details in the consultation documents below may compromise anonymity of a participant either the participant concerned has given permission for their name to be cited in the research or the details have been removed.

Consent Form

1. I have understood the objectives of this study and have had the opportunity to ask questions.

2. I understand that I can withdraw from the study without consequence at anytime simply by informing the researcher of my decision.

3. I understand that the records of this research will be kept private. Academic work based on this, and other interviews, will not include any information that will make it possible to identify any participants. Research records will be kept in a secure file to which only the researcher will have access.

4. I understand that this research will be written up as a thesis. On successful submission of the thesis, it will be deposited both in print and online in the University archives to facilitate its use in future research. It may also provide a basis for the production of academic articles published online or in peer reviewed journals

5. Please select:
Where elements of my participation in this research are referred to in the thesis or other research output:
□ I wish to be acknowledged by name
or
□ I wish my details to remain anonymous

6. I am aware of who to contact should I have questions following my participation in this study.

7. I understand that this project has been reviewed by and received ethical clearance through the University of Oxford Central University Research Ethics Committee.

I agree to participate in this study.

Name:

Date:

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Field lab model text

Space and Practice in Geomorphology

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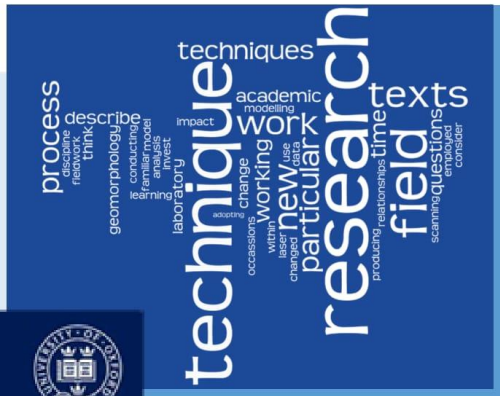

UNIVERSITY OF OXFORD

This research investigates the spaces and practices of field, laboratory, model and text in geomorphology, examining how these form and are formed by the changing foci and evolving techniques of geomorphological research, as well as considering the relationships between these spaces. It also examines the adoption of techniques across disciplinary boundaries into geomorphology and the evolution and modification of geomorphological research objects in work occurring at or across shifting disciplinary boundaries.

Your input to this research would be extremely valuable and I would be grateful for the opportunity to interview you. Interviews are generally informal and may last approximately an hour. The format is intended to be open-ended and will cover a range of themes; the questions on the right are intended just as a starting point for discussion, and focus on *techniques.*

Sample interview questions:

"How has the availability of field techniques for rapidly producing large quantities of data (for example, digital elevation models) impacted on your research?"

For example, has the availability of such techniques:

- altered the scale or focus of your research?
- altered the process of undertaking fieldwork, lab work or analysis?
- altered your ability to apply your research to real world questions?

"What challenges are involved in adopting techniques developed in other disciplinary settings to fluvial research (for example laser scanning)?"

Space and Practice in Geomorphology

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This research investigates the spaces and practices of field, laboratory, model and text in geomorphology, examining how these form and are formed by the changing foci and evolving techniques of geomorphological research, as well as considering the relationships between these spaces. It also examines the adoption of techniques across disciplinary boundaries into geomorphology and the evolution and modification of geomorphological research objects in work occurring at or across shifting disciplinary boundaries.

Your input to this research would be extremely valuable and I would be grateful for the opportunity to interview you. Interviews are generally informal and may last approximately 45 minutes. The format is open-ended and will cover a range of themes - the questions on the right are intended just as a starting point for discussion.

Sample interview questions:

It would be interesting to understand how your experiences have varied during different research projects and/or stages of your career. Therefore these questions could be considered in relation to your doctoral research and/or work undertaken since:

Can you describe an experience of adapting a piece of equipment or a model to put it to work in a new research context? What difficulties were encountered and how were these overcome?

Does fieldwork have a role beyond generating data? For example, do you need to recall or refer to fieldwork when (i) analysing and interpreting of remote sensing data or (ii) using numerical techniques to model ecological and/or geomorphological processes?

Do all members of a research project need to spend time in the field? To what extent is it possible to divide labour between different activities?

How important is consultation with colleagues when collecting and interpreting data, undertaking numerical modelling and/or exploring potential ideas for future research? What role does social media play in enabling dialogue?

When working with a particular field site, dataset or model how focused are you on particular objectives? Is it necessary / possible to step back and look at the bigger picture, or to attend to factors that are not initially part of your study but that may be important? How does this vary through the course of a research project?

Can you talk through or (even better) demonstrate some of the processes involved in (i) working with numerical models and (ii) analysing remote sensing data?

How do experiences, skills, equipment, models and datasets generated in the context of past research projects open up opportunities and ideas for new work?

What changes would you like to see, in contemporary academic geomorphology, in order to enable effective research?

The development of experimental techniques for the study of weathering

- Your collaboration with Ron Cooke and Ian Evans – including (a) work undertaken at the Physiographic laboratory in Cambridge and (b) the acquisition of climatic cabinets at Bedford College London and Oxford

Your recollections of Cambridge in the 1960s and Oxford in the 1970s onwards

- Working with Dick Grove to undertake field studies in the Kalahari
- Memories of Barbara Kennedy as a contemporary at Cambridge and a colleague at Oxford
- Memories of Marjorie Sweeting and the development of a laboratory at Mansfield Road
- Differences between Cambridge and Oxford at the time of your arrival in Oxford and over the course of recent decades

Aeolian geomorphology and evolving techniques

In 1969 you published a paper exploring the application of Horton's Stream Law to the study of sand dunes. This referenced both Grove's paper on 'Landforms and Climatic change in the Kalahari and Ngamiland' (1969), based on fieldwork you undertook together, and Horton and Strahler's mid-century work on quantitative techniques and dynamic geomorphology.

Grove has expressed the reticence he and Bruce Sparks felt towards the ideas Chorley propounded at Cambridge following his time at Colombia, remarking:

> I think we felt it was – was possibly too theoretical for us and very much concerned with statistical approaches … mathematical approaches anyway. And I think we'd both been brought up with the importance of – of field observation and measurement rather than – rather than theory' (Grove, 2010: 265).

- Were you aware of such a reticence at the time of your graduate studies?
- How did Grove respond to your 1969 paper on 'Statistical Laws and Dune Ridges'?
- Both your 1969 paper and your first academic post at Oxford had an emphasis on statistical techniques. To what extent have statistical techniques remained important for your work?
- Did the 1941 publication of Bagnold's *The Physics of Blown Sand and Desert Dunes* mean dynamic processes were already emphasised in Aeolian geomorphology at the time of the mid-century publication of Strahler's work on *Dynamic Basis of Geomorphology*, and *Statistical Analysis in Geomorphic Research*?
- Reflecting on the mid-century emphasis on quantitative/dynamic geomorphology at Colombia, Strahler commented in 1992 that:

  > Perhaps with a note of sadness, we must demote statistical analysis to a realistically lower level … Perhaps we can reassure ourselves that there is 'safety in numbers' – not in the significance numbers of our statistical tests, but in numbers of experienced scientists focused on the same problem (Strahler 1992: 74).

  More than half a century after Chorley arrived at Cambridge espousing an agenda strongly influenced by his graduate studies with Strahler how do you see the role of statistical techniques in geomorphology today?
- How have specific techniques shifted the balance between what Summerfield referred to at your retirement symposium as 'macrowaffle' vs 'micropiffle'?
- How has the relationship between form and process varied in geomorphology (i) over recent decades, (ii) across different specialisms in geomorphology

Recollections of the BGRG/BSG as an (i) early career (ii) mid-career and (iii) senior geomorphologist

- What are you earliest memories of the BGRG?
- Do you have any recollections of the 1979 meeting on Equipment and Techniques at Bedford College?
- Has your involvement in the BGRG/BSG shaped your career?
- What do you see as the role of the BSG in the future?

Human impact and the anthropocene

- What provided the impetus for your work on the human impact in geomorphology?
- How does the debate about the 'anthropocene' relate to this work on human impact?

Disciplines · Technologies · Communities of Practice

Space and Practice in Geomorphology

Field · Lab · Model · Text

Dear

Many thanks for your willingness to participate in this research.

Below are a few themes which I hope might provide a helpful starting point for discussion when we meet tomorrow. There is no need to look at the questions in advance, however, if you wish to do so then it might be helpful to note down any specific examples that come to mind.

With all best wishes and look forward to talking tomorrow.

Rachael.

Biography and disciplinary history:

- Can you talk about your early experiences as a geomorphologist, including memories of the department at and the BSG? What were the main frontiers for research being explored when you were doing your doctorate? What new techniques were becoming available?
- What changes did you perceive in British Geomorphology before and after your time in ?
- How important have efforts to examine interactions between ecological and geomorphological processes been (i) to your research and (i) to the discipline since the early 1990s? What are your earliest recollection of researchers in fluvial geomorphology incorporating this emphasis? How was this early work received and how has this changed?

Techniques:

- Which new techniques have had the biggest effect in opening out research possibilities during your career to date?
- What changes to fieldwork and modelling have occurred as a result of changing data sources including, for example, satellite data, LiDAR, and drones?
- How important have changes in available dating techniques been for your research?

Fieldwork and modelling:

- What is the role of modelling and fieldwork in moving from a channel cross section, to reach, to catchment-scale analysis?
- How valuable is familiarity with a particular field site, both for individual researchers and for the discipline? Are there particular field sites that you have returned to for a number of research projects? Can you think of sites you have researched where existing datasets or knowledge held by other geomorphologists have been decisive factors?
- How much does a geomorphologist need to know about the internal workings of a model in order to use it? Can a model be used 'off the shelf' or is work required to adapt it to a specific application?
- Are there divisions of labour within fluvial geomorphology around the development and application of models?
- Can you talk through/ show me any examples of desk-based processes involved in (i) early stages of exploring a possible research question / site selection and (ii) data analysis and modelling.

Disciplines · Technologies · Communities of Practice

Space and Practice in Geomorphology

Field · Lab · Model · Text

Below are a handful of questions that it would be great to discuss when we meet. As ever there are a number of themes I'm keen to cover; this selection is intended just as a quide / starting point for discussion.
All best wishes,
Rachael.

Embodied knowledge

In conversations with individual researchers, it has been clear that over time the application of specific techniques may become more automatic; for example, a researcher may find they become able to rapidly identify relevant features or data, or may find the collection of data becomes less stressful as techniques become 'second nature'.

Can you think of examples where this also happens collectively?
E.g. how does experience with particular techniques accrue at the level of the (i) laboratory or (ii) discipline, allowing researchers to benefit from the lessons learned during previous applications of a technique?
How is such know-how held and communicated? For example, is it stored and conveyed via:

informal discussion with colleagues?
advice from permanent laboratory staff?
articles? supervisors? social media?
software? other?
shared resources such as spreadsheets?
fixed processes or arrangements?

Are there cases where such knowledge is not retained or communicated at a collective level, meaning mistakes or dead ends are repeated? How can this be avoided?

Natural history

How do you understand the legacy of natural history in biogeomorphology? As you worked to demarginalise the role of organisms within geomorphology were you aware of latent resources, within the discipline, that could be harnessed to this task? Can you discuss this in terms of practices, techniques and equipment?

Mapping geomorphology

Can you sketch a map or Venn diagram showing relationships between disciplines and fields, for example:

biogeomorphology • geomorphology
earth sciences • geology • ecology
heritage conservation • ecosystem engineering
geography • engineering
[others?]

Are there specific texts, practices, equipment, or individual researchers that have defined the relationships between these disciplines?
How have these relationships changed over time?

Research

In relation to your current research what are the biggest sources of frustration? What factors limit the work you would like, given the chance, to undertake? (Funding? timeframes? Academic or disciplinary conventions?)

Fashion?

anthropocene • ecosystem engineering • climate
earth systems science • complexity

You once referenced the role of fashion in the terminology employed by geomorphologists. To what extent is the use of terminology (such as above) driven by (i) access to funding? (ii) the need to communicate with audiences beyond geomorphology and/or to maintain the credibility of the discipline? (iii) by the need or desire to carve out fresh ground in the literature and the discipline, e.g. to open out terrain where previous generations of scholars have less purchase, opening up space for new careers to be forged? or (iv) to enable a concrete progression in the understandings available to geomorphology?