Geography and paratactical interdisciplinarity: Views from the ESRC–NERC PhD studentship programme

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Abstract

Interdisciplinarity is a notoriously difficult concept to define, and even harder to achieve in practice. All too often social approaches reduce science to an object of study, or conversely physical science approaches are invoked as a source of ‘higher’ truth. Drawing upon our experiences as ESRC–NERC PhD students within geography, we outline a paratactical approach that links disciplines by adjacency rather than hierarchy. Toppling the disciplinary hierarchy creates the potential for non-reductionistic dialogue between science and social science, but it also raises a series of practical difficulties. These are considered around the themes of polyvocality, breadth over depth and (im)permanence. We suggest that while this kind of approach is increasingly encouraged by research funding bodies, it is less easily sustained within the everyday mechanics of the academic world.

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1. Introduction: geography and interdisciplinary

Better, perhaps, different coats to clothe the children well than a single splendid tent in which they all shiver (Goffman, 1961, p. xviv).

... human and physical geography are splitting apart (Thrift, 2002, p. 295).

In 1998 the ESRC and NERC introduced jointly funded interdisciplinary PhD studentships, as an experimental initiative to develop interdisciplinary capacity in the social and environmental sciences. It was experimental for a number of reasons. Firstly, interdisciplinary research generally tends to be multidisciplinary, involving teams made up of people from different disciplines. By contrast, PhD research is largely a solitary endeavour, focussing different demands upon the individual over a longer period. Secondly, the

joint institutional backing from the social and natural environment research councils demands some engagement across the ‘Great Divide’ between humanities and sciences (Snow, 1964). In recognition of these challenges the ESRC and NERC ran workshops each year for their interdisciplinary students to discuss the problems and opportunities of doing these PhDs, and the programme was initially only funded on an annually reviewed basis.

Perhaps because of the capacity to provide interdisciplinary supervision from both the physical sciences and humanities ‘in-house’, geography departments have been relatively successful in attracting these studentships. The interdisciplinary origins of geography have been variously attributed: from the polymathic tradition of early Enlightenment thought (Kates, 2002) to the rather less appealing influence of environmental determinism upon the birth of geography as a formal discipline in the late Nineteenth Century (Frenkel, 1992; Livingstone, 1992), and even to the view of geography as the perennial receptacle for everything that wouldn’t fit into neat disciplinary typologies since time immemorial (Capel, 1981; Turner, 2002a).

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However, opinions upon the desirability of this interdisciplinary heritage that emerge from reading a range of editorials and reflections in the leading journals are split, often between outright optimism and abject pessimism (for example, see recently Clifford, 2002; Johnston, 2002; Thrift, 2002). These extremes are symptomatic of the confusing current situation in which geography as an institution seems to be permanently under threat within the increasingly specialised disciplinary framework of schools and universities, while at the same time appearing to be an ever more relevant area of research and pedagogy to urgent high profile interdisciplinary issues such as environmental change and globalisation (Cook et al., *in press*; Harrison et al., 2004; Nissani, 1996; Stoddart, 1987; Turner, 2002b). This tension is reflected in the arena of research funding. While the rhetoric of interdisciplinarity abounds amongst funding bodies and those seeking to part them from their cash, research grants are often judged by atomistic peer review processes that can fail to assess the whole. For Geography, this problem is manifested most obviously in the split between the NERC and ESRC.

Given this context, the ESRC–NERC studentship scheme represents one of those rare instances when the interdisciplinary capacity of Geography has been explicitly beneficial within the modern academy. These studentships have highlighted a major competitive advantage in terms of interdisciplinarity, in that its own “interdisciplinarity” is actually “intradisciplinary” – science and humanities contained within one discipline. We have both held ESRC–NERC studentships in a Department of Geography and the rationale for this paper began as a feeling that being positioned as we were, with an interdisciplinary mandate for three years research within British geography, it should be possible to speak to current debates within the discipline from a fresh(ish) vantage point.

While our research focused on the quite different areas of urban wildlife and weather derivatives respectively, we were confronted with the same question over and over again – ‘how can we integrate science and social science without reducing either to the object of the other’s study?’ On a more personal level, we also often pondered the question of whether we were nobly re-populating the common ground of geography (Cooke, 1992), or, as Goffman muses in the quote that preceded this introduction, whether we would be left shivering out in the cold. In addressing these debates, we found ourselves grappling not so much with what interdisciplinarity was, but rather with how we as individual researchers had attempted it. So, if you like, we started from our experiences and tried to distil the common elements in our attempts *post facto*. This shift towards a more practical exploration of doing research is critical (see Collins and Evans, 2002¹), as theoretically ‘our fissiparous tendencies and the need to reverse them have been well-

rehearsed’ (Thrift, 2002, p. 295). The paper that follows reverses the order in which we came across its various components, adhering to the conventional ‘theory then practice’ model of journal papers in order to present our argument more coherently.

The first half of the paper discusses hierarchical power relations between disciplines, arguing that they are unhelpful to interdisciplinary work. We then outline a more experimental approach, which we term *paratactical interdisciplinarity*. This approach, literally ‘placing disciplines next to each other’, is elaborated through a consideration of ‘promiscuous realism’, Theodor Adorno’s work on aesthetics and Mikhail Bakhtin’s dialogic notions of polyphony and novelty. These ideas are developed through our own research experiences, considered around the themes of dialogue, breadth over depth and impermanence. The final component of the paper reflects on our experiences, good and bad, and while it ties in to wider debates and hopefully will resonate with the experiences of others, it remains an unavoidably personal account. We suggest that while this kind of approach is increasingly encouraged by research funding bodies, it is less easily sustained within the everyday mechanics and demands of the academic world.

2. The disciplinary hierarchy and why Geography must escape it

The discipline of geography has always grappled with the perceived divergence between the methods that inform each side of the discipline. This has been represented by Massey (1999) as a hierarchy of physics envy whereby concepts from physics are invoked as references to a higher (and by assumption *truer*) authority. Neurath (Cartwright et al., 1996) associates the rise of physics envy with an idle metaphysics that was driven by pseudo-rationalist science promising rational justification on a level of knowledge that is never available. The scientific hierarchy, for him, is part of the canon of positivism, which stems from a false premise that there is a single picture that concepts can help build up. Fig. 1 shows this hierarchy using

Fig. 1. The traditional hierarchy of disciplines (Hacking, cited in Cartwright, 1999, p. 7).

¹ Thanks to an anonymous referee for bringing our attention to this paper.

the common trope of the pyramid. While Neurath suggests that there are many scientific methods and concepts, with the sciences relating differently to each other depending on the point of application, the term physics envy has been used symbolically in other fields (see for example, Rorty's (2004) claims of philosophy envy). Regardless of envy though, the idea of a disciplinary hierarchy is fairly well established.

The disciplinary hierarchy of pseudo-rationalism has been partially toppled as our blind faith in science has been repeatedly shaken. But it would be ironic if, as a result, we were to unwittingly install a new hierarchy that subjects the sciences to the authority of the humanities. As Nicholas Clifford says about human geography,

The ridiculing of positivism and the deconstructionist excesses have taken their toll. What has, in effect, happened is to leave even those physical geographers who have explored the nature of explanation stranded – the grounds for mutual respect are gone, because the nature of the enterprise is fundamentally different (2002, p. 434).

Such an inversion of the hierarchy allows the social sciences to cast science as an *object* of study. For example, the discipline of science studies has the potential to provide a common ground for articulating science within its socio-historical context in a non-reductionistic way, but its encounters with scientists have more often polarised than unified debate as science is reduced to an object of study (Callon and Latour, 1992; Collins and Yearley, 1992; Gross and Levitt, 1994; Latour, 1999). This problem has been at least partially transmitted into human geography where claims about the 'other' become exclusionary. The conceptual languages human geographers have developed to articulate hybrid natures may show the categories of nature and society to be abstractions, but it is hard to make the case that such endeavours are interdisciplinary unless they are grounded in a more pragmatic engagement with what scientists actually do. While these efforts respond admirably to the imperative for human geographers to engage with questions of nature and the environment as *content* (Fitzsimmons, 1989), they are far from integrative in terms of *approach* (Petrie, 1986) (for example, note the absence of engagement with human geography's 'post-human turn' amongst ecologists and biogeographers). 'The Third Wave of Science Studies' (Collins and Evans, 2002), suggests that the sociological critique has gone too far and that science studies must re-establish the grounds for expertise that scientists have in their subjects. Though the ethnographic studies of science undoubtedly have afforded a richness to our understandings of scientific practice (Latour, 1987; Law, 1994, 2002; Powell, 2002), they should also then connect these practices with an appreciation of why scientists' expertise should be valued. As Chang (1999) puts it, science studies should act as a continuation of science by different means.

Fig. 2. Hierarchical interdisciplinarity.

Fig. 2 shows this inverted relation between science and social science. Note that the type of relation is different to that depicted in Fig. 1; while the pseudo-rationalist hierarchy is driven by the linear domination of reasoning, the reduction of science to an object places it *within* the explanatory framework of the social sciences. A brief anecdote of an exchange between a social scientist and an ecologist at the RGS-IBG conference in 2004 illustrates this point well. The ecologist presented a paper arguing that the major constraint upon putting urban ecology on the political agenda in cities was the lack of coherent baseline data concerning what species and habitats existed and where they were. This lack of data meant that their models of urban ecological interactions were severely deficient. The social scientist proposed that such investigations could be framed by the vernacular and lay knowledges of people living in cities, in terms of the values placed upon different ecological resources, and an appreciation of the culturally situated production of urban ecology. The ecologist replied that while there may be a place for such considerations, they would be entirely irrelevant until both a baseline data position had been established and an understanding of the processes governing urban ecology elaborated. Although only an impromptu and brief exchange, it shows how the positionality of researchers and what they see as the basis for inquiry is emblematic of the wider tensions between science and the social sciences. Both points of view can be construed as hierarchical, but whereas science reduces reality to an assumed causal hegemony of the non-human, the epistemological framing of social science places the scientific method further back in the queue.

As Magilligan states in the recent "Conversations" discussion piece in *Area* (Harrison et al., 2004, p. 438), 'determining what constitutes 'natural' is not only scientifically problematic, but...demands inter-disciplinary perspectives'. As the interchange above demonstrates though, actually achieving this is far from easy. Like icebergs, superficial differences in approach conceal more bulky epistemological issues beneath the surface. As Kitcher (1998) has argued (promoting a middle ground) scientists too often wish to return to a golden era when '...outsiders sing only happy songs around the scientist's campsites' (p. 49), whilst

Fig. 3. Different disciplines in a non-hierarchical relationship (Hacking, cited in Cartwright, 1999, p. 8).

sociologists too often conceive of scientists as ‘...brain-dead from the moment they enter the laboratory to the moment at which they leave’ (p. 37–38). Either of these scenarios precludes meaningful interdisciplinarity in geography. Massey suggests a two-way model of exchange whereby ‘ideas in philosophy can feed through to physics as well as *vice versa*, insights from the social sciences can be helpful in biology’ (1999, p. 273). In order to ask what a non-hierarchical disciplinary framework might look like we need a way to juxtapose disciplines in such a way as to allow for such exchanges of meaning, a kind of arrangement such as that shown in Fig. 3, that accepts a certain loss of philosophical rigour in order to avoid implicating any (decisive or divisive) power hierarchy that would see the insights of one discipline reduced to those of another.

3. Parataxis – losing your grip

The ontology of critical realism presents one potential approach to non-reductionist interdisciplinarity. Critical realism argues that the world is stratified; the social world is emergent from, but not reducible to the biological world, which is emergent from, but not reducible to the physical world (Bhaskar, 1997). As the notion of hierarchy is seen as an ontological condition rather than an epistemological categorisation of the world, interdisciplinarity is not only desirable but unavoidable. Thus rather than epistemological reductionism, critical realists adopt a more pragmatic realism that responds to an emergent ontological hierarchy, but which resists the epistemological reductionism and potential misattributions of causality of parochial disciplinary enquiry (Sayer, 1999, 2000). Whilst critical realism became an acknowledged philosophy within social science there have also been wide-ranging debates about the applicability of critical (and other types of) realism within physical geography (for example, see Bassett, 1994; Harrison and Dunham, 1998, on the possible significance of the quantum

theoretic framework for critical realist approaches to geomorphology but also Raper and Livingstone, 2001; K. Richards et al., 1998; Richards, 1990), highlighting the arguably problematic philosophical assumption of the hierarchical ontology underlying critical realism.

Dupré (1993) develops the realist position away from these implicit hierarchical underpinnings, invoking a pragmatic view that we must approach the world as fragmented and differentiated, claiming that academic inquiry requires a form of ‘promiscuous realism’. The Stanford School of Philosophy of Science (Cartwright, Dupré, Cat amongst others) adopts a position between those scientists who wish for everything to be neatly ordered and those who claim the world is completely chaotic. This position is reflected in the work of Otto Neurath, who uses the example of ‘the economy’ to demonstrate the inability of science to completely order and explain a complex system in a single model. As O’Neill (2003) notes in relation to Neurath’s work on associational socialism, Neurath rejects ‘scientific pyramidism’, espousing the appreciation of pluralism and a science that relies on association rather than hierarchy.

There is another word for non-hierarchical relationships between entities – parataxis. This word literally means to place side by side, but in its specialised linguistic sense means ‘the placing of propositions or clauses one after another, without indicating by connecting words the relation between them’ (OED, 2005). Its opposite linguistic construction, hypotaxis, involves subordination of one proposition or clause to the other, or the arrangement of unequal constituents. Take the statement,

Although Seurat’s intention was to render the canvas more luminous, he failed because the optical mixture was too evenly distributed.

This sentence is properly hypotactic because the failure is in spite of the stated intention. By contrast, the statement ‘Seurat tried to render the canvas more luminous *and* he

failed' is paratactic because the causal relationship is obscured by 'and', which indicates coordination rather than subordination. Parataxical clauses are therefore compounds, being formed of two or more independent clauses.

The notion of parataxis has been extended beyond the linguistic realm by Adorno (1997) in his classic exploration of artistic meaning and beauty, *Aesthetic Theory* (first published in 1970). He did not wish to write a book in the standard mode from beginning to end, but rather (in some ways pre-figuring Derrida) to engage in less syntactic forms of writing that highlighted a multiplicity of arguments through which all parts could be understood, but not reduced to each other. He achieved this through a paratactical² form of writing, in which a dense weave of prose is built upon subtle references, abrupt expressions and intensive argument. The spirit of parataxis we wish to propose draws its inspiration from this model of writing, but does not necessarily imply this paratactical writing style – it is concerned more with doing than writing.

Within geography several authors (Bracken and Oughton, 2006; Demeritt and Dyer, 2002; Powell, 2002) have used the notion of dialogue to think about interdisciplinarity. Dialogue allows different voices to speak and acts as a critical process that adopts methodological rigour (Oinas, 1999) or field-sites as sources of conversations between disciplines. Yet dialogue is only part of the challenge. It is too easy to converse and then retreat back into the disciplinary silo; the critical issue is in *doing* interdisciplinarity.

Adorno provides the skeleton for our concept of paratactical interdisciplinarity as a multiplicity of perspectives, each of which can make contributions that the others often miss, while *making no effort* to reduce one to the other. Similar arguments have been made recently in the field of political ecology, a field whose very name demands some engagement with Manichean disciplinary power relations (Walker, 2005). For example, Zimmerer and Bassett claim that political ecologists need to be 'inveterate weavers of analysis' in order to bridge the social and physical sciences (2003, p. 276). However, this way of working begs two questions. Firstly, is this not more akin to multi-disciplinarity, suggesting parallel investigations that fail to interconnect? Secondly, if working paratactically requires the researcher to relax their grasp upon the ordering process, what is the pay-off for this loss of analytical grip?

We argue that the loosening of analytical grip is actually essential to creating *novelness* between disciplines. Here the work of Mikhail Bakhtin, and the so-called Bakhtin Circle of Russian thinkers, upon dialogue holds a number of

pointers for the paratactical interdisciplinarian. Bakhtin argued that all meaning is created through dialogue between the fundamental categories of the 'I' and the 'other', generating 'the human being's absolute need for the other, for the other's seeing, remembering, gathering and unifying self-activity' (1990, p. 36). Building upon his fellow countryman Valentin Voloshinov's conception of the word as a 'shared territory' (1986, p. 86) he argued that meaning is established by common usage of a word by others in the past, but that 'they do not have any underlying invariant fixed meanings...(they) are meaning potentials open to different interpretations' (Lahteenmaki, 1999, p. 90). Because meaning is created through open dialogue, it is always partially borrowed, or shared, *and* partially creative.

Bakhtin developed this idea of novelness through his literary critique of polyphony. Bakhtin believed that the goal of the novel was to articulate the inherent surprisingness of human behaviour – its unfinalizability. If authors write from the perspective of a single character (whether in the first or third person voice) then it is impossible to write novels demonstrating human freedom, as the reader and author can comprehend the entire range of motives and desires of a character. In his analysis of Dostoevsky, Bakhtin argues that he was the first author in the modern European tradition to surrender the author's 'essential surplus' of vision, by unfolding the story through the voices of multiple characters (1989). In doing so, he placed himself on the same level as his characters, which is to say, to know about them at any given moment no more than would be possible for the characters to know. Polyphony thus refers to a form of writing that establishes a new relation between the author and their characters.

To write about people who are really represented as free, polyphonic authors design their entire writing process so that the characters may surprise them. Such surprise is not accidental but essential to the unfinalizability that lends this process its creativity or novelness. This is a critical epistemological shift; as Michael Holquist puts it, 'novelness is the name Bakhtin gives to a form of knowledge that can most powerfully put different orders of experience – each of which claims authority on the basis of its ability to exclude others – into dialogue with each other' (1990, p. 87). The spirit of Bakhtinian dialogics can be found lurking in a number of contemporary lines of geographic enquiry that reject what Haraway terms the 'God-trick', or the 'view from nowhere' (Haraway, 1997). For example, Cook and Crang (2007) and Crang (1992) (amongst others) advocate the power of multi-locale ethnographies which allow a multiplicity of voices to speak and be heard. Scholars of science studies advocate the need to capture the ability of non-human things to 'object' to stories that are told about them (Hinchliffe, 2001). Indeed, science itself proceeds not by getting things right, but by getting things wrong (a trait formalised in Popper's, 1963 notion of falsification).

Polyvocality requires a multi-perspective approach; each voice must be allowed to speak its own language without being forced into a single explanatory (or adjudicative)

² It is worth clarifying that the adjectival form of parataxis is not parataxical, but paratactical. Parataxis is an archaic psychoanalytic term used to describe the mode in which subconscious attitudes or emotions affect overt interpersonal relationships, whereas paratactic is something pertaining to or involving parataxis i.e. coordinative. Parataxis as we use it shares a similar etymology to that of syntax/syntactical, and we feel that the suggestive connotations of para – *tactics* is apt for the form of experimental rather than strategic interdisciplinarity that we are working towards.

system. However, the associated relaxing of analytic grip raises a series of important questions when we apply it to the research process in practice. The next section works through how dialogue is actually achieved between disciplines using examples taken from our own experiences as ESRC–NERC PhD students in order to shed light upon the practice of doing interdisciplinary research. Before doing so, it is worth briefly clarifying our own authorship strategy within this paper.

The paper is the result of ongoing dialogue between us, and as such we have used the first person plural thus far, representing ourselves as one ‘fused’ author. While the paper represents our ‘shared territory’, we come from slightly different academic backgrounds and have differed on various points within it. In the next section we switch to the first-person singular to illustrate our own autobiographical positionalities (Cook, 2001), that highlight the multiple ways in which interdisciplinarity can unfold. Each individual problem presents new challenges that cannot necessarily be approached in the same way as the last one. Hopefully the paper is written in a way that reflects the kinds of creative polyvocal dialogue that we have discussed.³

4. The paratactics of geography

4.1. Towards polyvocal research

(i) *Wildlife corridors.* Wildlife corridors, habitat patches and edge habitat constitute the three major components of landscape conservation planning (Spellerberg and Gaywood, 1993). Wildlife corridors have become especially popular amongst planners seeking to retain habitats and species within highly fragmented landscapes, as they are seen to play a vital role in connecting areas of habitat that are cut off by other types of land-use (Forman, 1991). As pressures upon land resources grow, conservation models such as wildlife corridors are increasingly appealing as tools with which to plan people and nature across space, and this logic holds at a number of scales, from the continental (for example, schemes such as the USA Wildlands Scheme) to the city scale in the UK. However, while a wealth of scientific work has accumulated, their scientific basis remains inconclusive, and the ecological efficacy of wildlife corridors has become a point of contention amongst ecologists (Beier and Noss, 1998), conservationists (Dawson, 1994) and planners (Barker, 1997). In light of increasing developmental and conservation pressures upon space, practitioners and scientists are divided over whether the continuing genuflection to the orthodoxy of corridors is either justifiable or useful (Whitfield, 2001).

I became interested in the issue of wildlife corridors tangentially through the science component of my PhD. My initial research was upon brownfield biodiversity and development in Birmingham, and this involved visits to urban brownfield sites to observe habitat and species surveying for ecological assessments and research projects. This was accompanied by attendance at conferences upon urban ecology and basic ecological training. This component of the research was segued with an urban ecological research project within the department studying the efficacy of wildlife corridors and brownfield sites as habitat patches in, amongst other places, Birmingham. Funded as part of the NERC Urban Regeneration and the Environment (URGENT) Programme that ran from 1997 to 2004 (NERC, 2003), the project aimed to inform strategic planning policy with cutting-edge ecological research. The project concluded that urban landscapes are more porous to nature than had previously been assumed, and that connectivity was not a major determinant of urban biodiversity.

On the one hand, the research upon planning politics showed that wildlife corridors played a powerful role in mediating between ecological and developmental interests in the city (Evans, 2003), while on the other, the scientific data concerning the actual usage of corridors by animals and plants in the city suggested they were largely irrelevant for species dispersal (Centre for Ecology and Hydrology, 2001; Small et al., 2003). In spite of these findings the city conservation policy remained based around wildlife corridors, and presentation of the results to other ecologists often caused outrage (Evans, 2006). This disjuncture prompted me to explore why they remained so popular amongst planners, conservationists and ecologists. I argued that the scientific myth of limited species dispersal persists because it is complicit with a wider cultural assumption that nature and society are antithetical to one another. This assumption is manifested scientifically in the idea of a wildlife corridor which, nourished by the wider dissociation of nature from the city (e.g. Hinchliffe, 1999; Williams, 1973), is in turn highly complicit with capitalist spatial imaginaries of the city as the site of development (Evans, 2004; Swynedouw and Kaika, 1999). The wildlife corridor provided a shared territory (not dissimilar in this case to the concept of a ‘boundary object’ (see Jokinen, 2006)) that was discursively flexible enough to retain meaning across distinct social worlds, binding those worlds together.

To follow this thread from science to social science I had to try and abandon any philosophical imperative to unify the scientific and the critical approaches that were adopted or establish theoretical power relations between them. Equally though, it was important not to completely disengage the science from the social science. Existing academic commentaries on the corridor debate have focused upon disentangling the various functions (ecological, amenity, transport, habitat, etc) and associated interpretations of the wildlife corridor concept (Dover, 2000; Hess and Fischer, 2001). Rather than reduce the corridor to its constituent parts, the aim was to bring scientific and cultural analyses

³ Going beyond a polyvocal approach and writing interdisciplinarity up in a paratactical format is beyond the more reflective remit of this paper. The approaches of other authors could be used, from the autobiographical text/academic footnotes to a more conversational format (Angus et al., 2001).

into dialogue with each other. This is not to deny that the scientific results were the product of a specific techno-political assemblage, and that they can be seen as such. Rather it is to emphasise that this should not preclude the use of scientific method and results to drive a critical analysis, the *conjunction* of science as *method* rather than as *object*. In this case the science mattered in highlighting the contested character of wild life corridors. Creating space for science and social science to speak their own languages generated novel insights (it was impossible to predict where this research was going to take me) into the complex set of connections between science, ecology, culture and space.

Elaborating upon the exact form of dialogue further, the two approaches were juxtaposed in series rather than in parallel. Similarities exist with current developments in the realm of citizen science, with a range of deliberative strategies being refined to put social concerns and science into serial dialogue with one another (Demos, 2004; Petts and Brooks, 2006). These serial engagements are characterised by attempting to get those involved to loosen their grip over the process. The classic case in risk studies is the need to create space for lay knowledges within a traditionally specialist technical domain (Wynne, 1996), and it is this kind of loosening that we are advocating in a disciplinary context.

(ii) *Weather derivatives*. Weather derivatives are a form of socio-natural hybrid requiring the approaches of both sciences and humanities. I have found that social scientists and meteorologists often wish to remain within their disciplinary confines; meteorologists examining the relationship between meteorology and information supply to the market, social scientists informed with critiques of the commodification of nature. For a firm to engage in weather risk mitigation it is vital to establish scientifically how the weather affects the earnings of that firm. A weather sensitivity analysis is an important component of research on weather derivatives as it shows how firms could use these financial contracts. For meteorologists an ideal contract would be a direct swap (Thornes, 2003) between two companies with opposing weather risks (a direct swap being a financial contract whereby risk is directly swapped between two companies to even out volatility).

In practice this is highly unlikely due to the necessary time searching for a company with opposing risk, the levels of knowledge needed to price the contract ‘correctly’, issues of trust over such confidential information and the regulatory hurdles that must be overcome. The practical side of this equation required that I used a qualitative approach that describes the actual nature of the weather derivatives market rather than the theoretical market. Nevertheless this qualitative critique is held in tension with the quantitative work, because without the sciences of meteorology and economics quantifying the risk, there could be no operative market in the first place. There is not space here to sketch out the other interdisciplinary debates that weather derivatives can contribute to, but examples can be found in other publications (see Pollard, et al. this issue and Randalls,

2006). Studying weather derivatives thus requires that the processes of science and humanities remain in tandem informing each other rather than slipping back into a hierarchical approach.

Both of these research experiences required some degree of loosening of academic control over the research framework. However, it is important to distinguish this from epistemological relativism – we are not advocating ‘letting a thousand flowers bloom’, as Paul Feyerabend would have had it (1988). Rather, we are trying to construct a meta-epistemology based upon promiscuity that enables independently validated and established disciplinary approaches to communicate in a non-reductive way. The notion of losing your grip is reminiscent of Gregory Bateson’s claims that ‘advances in scientific thought come from a *combination of loose and strict thinking*’ (1978, p. 49), in which ‘wild hunches’ (loose/interdisciplinary thinking) are then backed up by rigorous analysis (strict/disciplinary thinking). In the case of wildlife corridors, the ecology had to be allowed to speak in its own language as “science” in order to create a loose situation in which it could prompt the critical analysis of how wildlife corridors reproduce an ideologically charged spatial ordering of humans and non-humans.

This moves beyond an integration of content or subject matter, what Ron Johnston termed the vernacular of geography (1986), towards a dialogue between the different academic languages or approaches of science and social science. While we agree with Johnston that the *integration* of academic approaches is impossible, we disagree that this precludes interdisciplinarity as a polyvocal dialogue between disciplinary approaches. Bakhtin’s notion of meaning (and indeed existence) as socially embedded dialogue avoids having to abstract paratactics to either a rational communicative naivety, or abandon it to a hopeless relativism. As Demeritt and Dyer (2002) argue, a pragmatic approach calls us to abandon both naturalism and relativism in favour of an approach that places methodological rigour, trust and credibility at the heart of knowledge claims. The shared territory articulates this emphasis upon mutual trust while allowing the emergence of novel forms of dialogue.

Fore-shadowing some of these arguments concerning polyvocality, Millstone (1978) has argued that rather than using deconstruction and science studies to dismember science, perhaps we should be using these critical powers to *make space* for science (as method) within social science. Rather than collapsing the disciplinary hierarchy, we need to topple it onto its side. Paratactical interdisciplinarity is not about operating in the space between disciplines – bridging, commensurating, reducing or *cooling* in some way. It is about rubbing one against the other, creating dialogue, friction, heat and fire.

4.2. *Depth versus breadth*

We have argued that geographers must be paratactical. But isn’t this in essence what geography has been doing all along, placing sub-glacial physicists in offices next to

economic geographers? Yes and no. Spatial proximity does not necessarily result in innovation or conversation (though it may do). Developing social links with others in a department is of course an important aspect of engaging in research that crosses boundaries, but as often as not there are clear institutional barriers to this. The factions between human and physical geography are often well mapped within a department, both spatially with academic segregation between floors or even buildings, and temporally, with seminars and the accompanying sessions in the bar occurring at different times. Given the increasing specialisation and proliferation of sub-disciplines within geography departments, even crosscutting events and seminars become seen as specialised events. As with promiscuous behaviour in every aspect of life, there are limits to promiscuity in academia, and these are problems many geographers will have encountered.

Conflating two or more strands of literature is a standard practice in PhD theses, but we found that we had to traverse literatures that were more methodologically and philosophically distant. For example, understanding weather derivatives requires drawing upon literatures as diverse as financial economics, meteorology and sociology of science, not to mention economic geography and nature-society debates. This creates a series of purely practical problems, not least of how to find space to accommodate unwieldy literature reviews. Splitting into two chapters is one solution, but that in itself makes it harder to construct a dialogue between the two literatures. To some extent, we have both realised a very basic form of Adorno's 'dense weave', juxtaposing literatures in series and connecting them by subtle (if at times, circular) references. But this way of working can be confusing.

At a practical level this requires more words (this is particularly problematic when it comes to publishing in journals – an issue dealt with below). Vastly divergent literatures will also have specialist languages that have to be understood and bridged. The languages of meteorology and social semiotics are not obviously compatible. Reading at the cutting edge of each area is a challenge and thus one's knowledge of literature can become shallow and broad, rather than narrow and deep (lest we forget, the supposed point of PhD training!). This logistical stretching of the PhD format also conjures up a host of treacherous pedagogical problems concerning what is generally taken to constitute a PhD. While a paratactical approach can provide multiple understandings of the focus of study rather than reducing everything to one standpoint, the author runs the risk of being labelled a dilettante. Finding an external examiner for the thesis who will accept a relatively weak analytical approach to the material and will overlook gaps and inconsistencies in their specialist area for the purpose of accommodating another (inevitably viewed as inferior) disciplinary viewpoint is not always easy. This type of research, suggests Chang (1999), actually calls for a return to 19th century natural philosophy with its sense of holism and joining of science and philosophy.

This difficulty extends to supervision. ESRC–NERC students become harder to supervise, because they do not easily fit into an existing area of expertise or mode of supervision. In our experience, physical geographers and human geographers are often supervised in different ways with varying expectations at important stages of the research process (when to work in the field, how much to read, when to write papers, what constitutes acceptable upgrade papers in departmental assessments and so on). Nevertheless many supervisors find this an interesting and thought-provoking challenge. In fact, many of our supervisions created a space in which two or three colleagues who might not otherwise engage with each other at an academic level have the luxury of being able to explore each other's work and ideas. They often had a large 'blue skies' component in terms of talking about bigger more abstract ideas and problems, and opportunities and parallels. Once the idea that one side is to dominate the other has been lost (and that usually takes all of the first year), supervision became an eminently enjoyable dialogue between all parties. This encourages others to adopt less singular approaches to their own work. So for supervisors this means appreciating different literatures and methods. For students this means that rarely can one simply become a clone of one's supervisor, having to take control of the work relatively early on in the PhD process. This prevents theoretical domination by either side and allows for a potentially more innovative piece of work.

The trade-off between the pros and cons of disciplinary polyphony in doctoral research maps broadly onto a tension between depth and breadth of research. Interestingly, this tension is also apparent within contradictory intellectual readings of the notion of parataxis. For example, the grammatical arrangement of words in dependent or subordinate relationships of causality, logic, space and time, is usually taken as a characteristic of mature, formal or disciplined speech, whilst the linguistic notion of parataxis is seen as a primitive (specifically medieval) semantic construction. However, the related notion of polyphony is generally advocated as indicative of a highly advanced form of literary narration, or ethnographic methodology. While we do not want to go into the implicit parallels between pre-modern and post-modern communicative paradigms and their contrapuntal relationship to the rational paradigms of Modernity, this line of thought may hold some relevance to current tensions in academia between breadth and depth of research.

4.3. *(Im) Permanence and closure*

One of the problems with the paratactical form in Adorno, as the translator of the 1997 edition notes, is that it is only supported by its density and,

With few exceptions paratactical works are therefore short, fragmentary, and compacted by the crisis of their own abbreviation (xvi).

This may seem a little strange given the problems of needing more, not less, space mentioned in the preceding section. But rather than a practical limitation, this statement alludes to the relative *impermanence* of the paratactical mode. Rather than following a clearly prescribed disciplinary path based upon the accretive specialisation of content (subject) and form (approach), parataxis

...[e]merge(s) when scholars forget about disciplines and whether ideas can be identified with any particular one; they identify with learning rather than with disciplines. They follow ideas and connections wherever they lead instead of following them only as far as the border of their discipline. It doesn't mean diletantism or eclecticism, ending up doing a lot of things badly. It differs from those things precisely because it requires us to follow connections (Sayer, 1999, p.n.p.).

Given this relative lack of definition, the question of how to lend such endeavours some degree of permanence (particularly in the context of academic careers and research trajectories) and closure (particularly in terms of research projects) assumes some relevance. These two issues are intimately related, as both spring from a certain lack of fit with the existing academic system. Mechanisms may be needed that value breadth (or 'promiscuity') as well as depth, connections being made across disciplines rather than just insights that deepen a discipline. This does not mean as Sayer notes 'doing a lot of things badly', but rather it promotes a concerted political and ethical engagement through tracing connections and things across disciplines (see Cook, 2004; Massey, 2005 for recent discussions of this mode of working), and across the science – social science divide so important to addressing contemporary environmental challenges (Richards, 2003). The funding system would need to be sensitive to the experimental aspect of paratactical research; projects that are ideally loose, polyvocal and unfinalizable are going to lack pre-ordained points of closure. We are painfully aware that while the ample element of caprice that informed our own research paths was acceptable within the framework of a PhD, it may be less so in the context of big-money research applications. Proposals in this instance would be not so much planned from birth to death as heavily proscribed by the mix of different disciplines and the modes of interaction. Interestingly, this mode of interdisciplinary research is far from 'dull and applied' (a slur sometimes levelled by strict disciplinarians), but rather assumes more of a 'blue-skies' element.

Potential support and a source of permanence for such work may be found in the current trend amongst funding bodies towards so-called 'remit-drift' (the funding of projects that often lie at least partially outside of their own disciplinary portfolio), and large interdisciplinary research programmes (for example, Sustainable Urban Environments (EPSRC), Rural Economy and Land Use (ESRC), Towards a Sustainable Energy Economy (EPSRC/NERC/ESRC) to name but a few). However, the (literally) disciplined nature of journals, RAE groups and academic net-

works makes it less obvious how to proceed as an 'interdisciplinary' individual (although this is definitely not to say *impossible* either within or beyond geography, merely less *obvious*). Many people are interested in interdisciplinary work because it is different, but it is precisely this trait that lends it an element of risk, suggesting that the PhD was actually the easy part and that getting published and getting a job may be harder. It is far from clear how such an individual will fit in to well-established academic disciplines, what the publication channels are for such work, how a research profile will develop, and how such work would fit into what are often highly segmented departments, clearly demarcated journals and rigid disciplinary assessment procedures. We have both failed to get papers published that contain scientific and social elements, getting criticised by referees for not elaborating enough upon the complexities of concepts and debates within their own discipline, while failing to provide enough basics on those from others. The (far from progressive) answer is always to separate the scientific and social scientific into separate papers. When we have refereed interdisciplinary research proposals, we have received covering notes explaining how this type of research should be judged differently to 'normal' disciplinary proposals. To the best of our knowledge, no such practice occurs in the arena of refereeing journal papers.

These concerns are manifested in the problem of establishing post-disciplinary identity in a highly disciplinarised modern academy. To take a slightly flippant but nevertheless indicative example, the ESRC classifies us rather nebulously under 'interdisciplinary studies', but at the University of Birmingham the 'Interdisciplinary Research Centre' deals with 'Materials for High Performance Applications'! From our personal experiences we label ourselves very differently within various academic environments. To a human geographer we might call ourselves interdisciplinary, to a physical geographer we might label ourselves as renegade scientists, to physical scientists we might simply become geographers. These labels are important, but they become particularly stretched with interdisciplinarians, because it is easy to hide behind the labels to excuse our lack of depth in a particular subject and it is also easier for others to characterize us as not really part of them. Our sense of belonging is promiscuous.

This potential identity crisis has ramifications for the practical points of writing interdisciplinary work up. We have become acutely aware that there are simply differences in style, presentation and format between scientific, environmental and social scientific journals. Work that tries to please everyone runs the risk of pleasing no one (try presenting a conference paper to ecologists that talks about 'lay' knowledges, or to social scientists based upon scientific results that haven't been suitably situated in terms of their production). Related to this problem, the journal publication format has evolved as a way for specialist material to be communicated in a relatively brief format, on the assumption that the audience and scope of the readership is

highly specialised. Put quite simply, more room is needed to explain oneself. This might partially explain why the majority of journal articles dealing with interdisciplinarity are commentaries and discussion papers, rather than empirical research papers.

5. Conclusions

Building upon our experiences as ESRC–NERC students, this paper has argued that the reductionism inherent in disciplinary hierarchies is antithetical to interdisciplinary working. Instead, we have developed the notion of parataxis to describe commonalities between our attempts to work in an inter- or multi- disciplinary way around our PhD topics, and denote the promiscuous process of bringing different work into dialogue without reducing one to the other. Toppling the disciplinary hierarchy yields certain benefits, in that it can create novel (non-reductionistic) dialogue between science and social science. However, it also requires the researcher at least partially to ‘lose their grip’ on the analytic coherency of a project, as various approaches are allowed to speak in their own voice. This represents a different type of academic process, which we have characterised as breadth over depth, and while this approach seems well supported in the world of research funding, it is less so in the academic world.

To return briefly to Bateson, if ‘this combination (of loose and strict thinking) is the most precious tool of science’ (1978, p. 49) then we urgently need to extend the institutional spaces in which such impermanent thought can occur. As Isabel Stengers argues (2000), the hallmark of good research can be the ability to put itself ‘at risk’, in order to broach new boundaries. The ESRC–NERC studentships are avenues of opportunity for researchers to experiment with novel juxtapositions of approach and method. Returning to the tension between individual paratactics and the suitability of this model to large-scale group work, it can be argued that individual interdisciplinarity is stronger than in a group situation as the temptation to slide back into one’s own discipline is lost. Too often group interdisciplinarity soon dissolves after a particular research project and the divisions are re-instated. Of course there is a paradox here – if interdisciplinary work becomes too successful it grows into a sub-discipline and eventually a discipline, becoming locked-in and fossilised. Impermanence becomes permanence. In some cases this will be desirable, in others it will not, but it is important to note that within the shifting context of higher-education University departments may become less important as conduits of research in the future.

Agreeing with Massey (1999), we would argue that paratactics as experimental dialogue supports the idea that the different theoretical traditions of human and physical geography could inform each other to the mutual development of each – but through a process of association rather than domination. Marxism can inform ecology and ecology can inform Marxism. The idea of the shared territory is again a potentially fruitful one here, suggesting that certain con-

cepts can act as facilitators of dialogue between disciplines. A number of potentially fertile conceptual grounds for dialogue have been raised in the geographical literature in recent years, from space-time and materiality (Lane, 2001; Massey, 2001) to scale and emergence (Brenner, 2001; Marston, 2000; Phillips, 1999, and perhaps most importantly Purcell, 2003 and Sayre, 2005). Perhaps it is only as large-scale interdisciplinary projects yield their fruits over the next 5 years that we will see a truly experimental engagement with these shared territories rooted in *research*.

The paratactical approach that we have developed thus presents an opportunity for engaging in non-reductionist interdisciplinarity that overcomes some of the common assumptions often hidden within attitudes and approaches to interdisciplinary work. It is the rejection of ‘either/or’ in favour of ‘and ... and ... and’. To end on a personal note, it is worth stating that the experience of taking an interdisciplinary PhD has been a positive one for us both. Perhaps the biggest advantage is the levels of interest they generate, both for the student undertaking the work, the supervisors, and in terms of their reception in academia. This is of course related to the fact that they represent something of a *terra incognita*. While the inertia of disciplinary fragmentation is affecting employment prospects through the RAE panels and assessment criteria, we are finding more opportunities than constraints, and view the interdisciplinary studentship model as a successful one. However, with respect to geography in particular, whether we are re-populating Cooke’s disciplinary common ground or just shivering in the cold remains to be seen.

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