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Andrea J. Nightingale (2006) *Can Social Theory Adequately Address Nature-Society Issues? Do political ecology and science studies in Geography incorporate ecological change?*, online papers archived by the Institute of Geography, School of Geosciences, University of Edinburgh.

There has been an expansion of interest in nature-society issues within human geography spurred by the rich, sophisticated analyses of environment-development issues within the Third World. This latter work emerged out of the fusion of cultural ecology and the political economy of resource use, but scholars are increasingly turning towards post-structuralism to engage with the complex, mutual constitution of symbolic and material struggles over land and resources. Yet to some extent, these theoretical trends are moving nature-society geography away from engagement with physical 'natural' processes despite rhetoric to the contrary. In this paper I raise the question of whether current work in critical Geography on nature-society issues adequately tackles the 'so-what' issues of socio-natural change. Do political ecology and science studies—the two, broadly defined approaches currently favoured by most critical geographers—accomplish what is required theoretically and methodologically to engage with fundamental issues of social and environmental change? I suggest that when used in isolation both approaches are inadequate to point us in politically useful directions. Instead I argue for more engagement with ecological theory and ecological processes as they articulate with social processes in contingent, dynamic ways.

That there is reason to be worried about nature I am taking for granted in this paper. The concern then is how do we understand environmental problems and how do we go about trying to change them? I am beginning from the assumption that critical geographers have two broad issues they address in relationship to environmental change.¹ First, critical geographers examine the cultural politics of environmental issues at all scales. This includes the production of knowledge about nature and environmental change; discourses of environment; policy implementation and policy making processes; social movements related to the environment; and the relationship of environmental issues to political-economic and cultural processes. Second, critical geographers examine the consequences of environmental change for socio-natural systems. While much attention has been paid to the social consequences of environmental change, I argue that the physical, environmental consequences have been largely neglected. This omission has serious implications for our understanding of the social consequences if one takes seriously the insistence by many critical geographers that social systems cannot be separated from environmental systems. I will try to illustrate this point more fully throughout this paper.

I believe the neglect of environmental consequences derives largely from our theoretical and methodological commitments. I will first trace the contributions of political ecology and science studies to these issues and point out some of the lacuna in these approaches. In particular the recent turn towards science studies needs to be critically examined as I argue that these approaches, while providing some very valuable insights, are seriously limited in their ability to engage with the *consequences* of socio-natural change. I conclude with some questions and suggestions about the direction critical geography needs to move to engage more directly with the social-natural issues at stake.

¹ I do not intend to police the boundaries of nature-society geography, rather I have arrived at these issues from a reading of the literature. They basically encompass the three outlined by Castree (2001 p. 10-16), I have collapsed them here as this is how I initially saw them. It could be useful to discuss if these (two-three) agendas are sufficient.

I want to begin by briefly defining some terms to avoid confusion and debates over the epistemological origins of these terms. I use 'ecology' to refer to the set of scientific disciplines that are engaged in examining physical environmental change. While the academic discipline of Ecology may not necessarily encompass all the sciences I have in mind, I use ecology as a short-hand for research into biophysical change (including physical geography) and corresponding representations of nature within those disciplines. Nature I use as a word to refer to the physical, material non-human world—that world that would continue to exist if all humans were wiped off the planet tomorrow. Environment I use to represent human understandings of nature. While I agree with many social constructionists (cf. Demeritt 1998) that 'nature' cannot be understood outside the parameters of 'environment' as I've defined these terms, I also prefer to retain a category that can capture the sum of 'environments' as different people understand them, and the physical world we inhabit as analytically separate from environments(s). Socio-nature I thus use to describe the material and symbolic manifestation of the social-nature nexus, what has also been called socationature by Erik Swyngedouw (as cited in Castree 2001 p. 13).² Finally, environmentalism I use to refer to mainstream environmental movements such as Greenpeace, the Nature Conservancy, etc.

Political ecology and science studies approaches, including actor network theory (ANT) have engaged with the first of my two issues remarkably well. The politics of environmental issues have been examined at a variety of scales and with different theoretical tools. Comprehensive reviews of this work have been done elsewhere (see for example Braun and Castree 1998; Bryant 1998; Bryant 2001; Castree 2001; Castree and MacMillan 2001; Demeritt 1998; Peet and Watts 1996; Sneddon 2000; Whatmore 1999). Here I want to briefly trace some of the major contributions in order point out how they fail to engage with the second issue I raised above: the consequences of environmental change for socio-natural systems. Of course in constructing this kind of review there are many scholars whose work spans the two approaches and therefore some of the critiques do not apply easily to them. Nevertheless, this seemed to be the clearest way to present them. I have arrived at these conclusions and critique based on a systematic, although not necessarily comprehensive review of the literatures in these two areas, thus I apologise in advance if I have missed work that is moving closer to the direction I have in mind.

Political Ecology

Political ecology, as it has risen out of cultural ecology, the political economy of development, marxism and post-structuralism encompasses a diversity of theoretical and methodological approaches (Bryant 1998; Peet and Watts 1996). While political ecology is not uniform, as a whole this body of work has placed four key issues within the centre of nature-society geography.

First, political ecology has demonstrated the importance of examining the linkages between local environmental issues and global political-economic processes. The

² In some ways I have arbitrarily chosen these words—environment for example carries a lot of baggage (Castree 2001). I certainly do not see them as unproblematic, my main intention is clarity and simplicity in diction. 'Nature' in particular has presented me with many difficulties because the adjective is 'natural'. I have no intention of invoking all the meanings of natural. Unless I state otherwise I simply mean 'processes of/in nature' (i.e. socio-natural processes).

articulation of different localities with capitalist production is inextricably linked to the exploitation of 'natural resources' both for subsistence by increasingly marginalised primary producers, and for profit. This insight has placed an examination of production processes and labour relations in the centre of environmental issues, something which often continues to be overlooked by mainstream environmentalism. It has also served to undermine neo-Malthusian arguments about population pressure and ecological limits by demonstrating how population pressure is not directly correlated with ecological decline but rather is closely linked to political-economic pressures on resource consumption (Blaikie 1985). This is equally true for marginalised producers as it is for capitalist exploitation of resources (Blaikie and Brookfield 1987; Hecht 1985; Peet and Watts 1996). Thus, political ecology has drawn attention to the inter-relationships between ecological impacts and socio-economic power relations.

The second issue that has been explored by political ecology is the ways in which knowledge about nature is mobilised and contested politically. These knowledges include ecological representations of nature and 'local' or 'indigenous' knowledges that often combine understandings of physical processes with social histories of the land. Knowledges in this sense are used both to promote particular political positions and to contest ownership claims (Jarosz 1996; Moore 1996; Schroeder and Suryanata 1996; Zimmerer 1996). In other words, as Escobar argues, particular kinds of ecological knowledge are used to justify sustainable development programs that may be at core rooted in other priorities (Escobar 1995). Or as Jarosz has shown, the ecological effects of different land use strategies are mobilised to accomplish objectives in relation to settlement and distribution of population. Thus, swidden agriculture (shifting cultivation) has been constructed as damaging fragile tropical forests in order to undermine ownership claims by forest dwelling groups (Jarosz 1996). The construction of discourses about environment(s) then, is mutually constitutive of social-political struggles over land use.

The third and closely related insight that arises out of political ecology is that contestations over environment(s) inscribe the kinds of land use and management practices dominant in particular places. Thus, environmental justice activists have struggled to define 'environment' as encompassing everyday spaces in an attempt to gain the recognition that issues facing their communities are ecological issues and not 'just' public health issues (Chiro 1996; Pulido 1997). In other contexts, geographers have shown how the understanding of trees as ecologically (and commercially) beneficial has led to the planting of fast-growing, green, but otherwise invasive and ecologically problematic tree species (Robbins 2001; see also Shiva 1988). Conceptions of nature and what is natural then, strongly shape the kinds of land use and management interventions that are considered acceptable within different environments.

Finally, political ecology has examined the potential of environmental issues for mobilising social movements that link people across gender, class, race and nationality. Empirical investigations of nature-based social movements have demonstrated the key roles that women and other marginalised people play within them (Peet and Watts 1996; Rangan 1997; Rocheleau, Thomas-Slayter et al. 1996). Globally, some third world groups have been successful at soliciting the support of first world environmentalists for their causes, possible because both groups share a

common concern for (a common) environment (Watts 1998). Concern over changes in nature resonates for diverse groups of people if they can sufficiently agree on what environment they are protecting. The political potential of nature-society issues for causing systemic social change therefore seems to be more promising than movements based on class, although complex power negotiations cannot be neglected.

These four issues have been explored in the Third World by political ecologists and continue to be explored in different contexts (urban, first world, urban third world) by critical geographers (see for example Adams 1990; Batterbury, Forsyth et al. 1997; Bebbington and Batterbury 2001; Carney 1996; Freidberg 2001a; Freidberg 2001b; Nesbitt and Weiner 2001). To me one of the very important insights coming from this body of work is the way that economic development issues are always environmental issues, although this insight is not always explicitly acknowledged.

Early, political ecology was accused of being not political enough or ecological enough (Peet and Watts 1996). Most political ecologists have responded to the need for more politics by more carefully examining relations of power symbolically and materially at multiple scales, drawing on post-structuralism and feminist studies to do so. The charge of not enough ecology, however, has proven to be far more difficult. With very few, but important, exceptions (cf. Robbins 2001; Scoones 1997; Sneddon 2000; Zimmerer 1994), political ecologists have not attempted to engage with ecological data. Instead, nature continues to be treated as a background or context for the social-political disputes over land and resources—despite acknowledgement of their mutual constitution. Studies that do engage with ecological change (but not ecological data) often treat it as an impact or result of the social-political struggles and rarely examine the way that a *dynamic* nature constitutes the struggles that result. And perhaps most importantly, ‘ecology’ remains a separate, but interactive domain with ‘society’. The mutual constitution of them is rarely treated in a theoretically and methodologically meaningful way.

In an attempt to address these problems, one new focus within political ecology is on environmental imaginaries and the ways in which these imaginaries are linked to the social and cultural processes of land use and ownership (Nesbitt and Weiner 2001; Watts and Peet 1996). While engagement with these environments is important, what this work fails to do is critically engage with what these imaginaries might tell us about changes in nature. Ecological analyses are not complete (cf. Haraway 1991) as political ecologists have so elegantly demonstrated—the social-politics of ecology (both as scientific practice and as land or resources) shapes the way in which environments(s) are constructed and mobilised historically and geographically. As a result, it is crucial that we develop theoretical and methodological practices that can critically interrogate what insights we can gain into changes in *nature* from an analysis of ecological and ‘other’ environmental imaginaries.

Most importantly, as many ‘new’ ecologists and some political ecologists have highlighted, nature is not stagnant nor in equilibrium (Botkin 1990; O'Neill 2001; Zimmerer 1994). Not only do environmental imaginaries and the social-political contestations over the environment change constantly, so do the natural (physical) processes. Thus we need a way to theorise the relationships between nature and society that conceptualises both the human and non-human dimensions as non-linear, unstable and therefore highly contingent and context dependent. In other words the

mutual constitution of social and natural (physical) processes shift continuously in addition to the processes themselves shifting constantly somewhat independently of each other.

The Turn to Science Studies

As a result of calls for more theoretical integration between nature and society, many critical geographers have drawn on the work science studies, particularly Bruno Latour's actor network theory (Latour 1987; Latour 1993) to achieve that integration. ANT conceptualises nature-society processes as networks that are animated by both human and non-human actors (or actants). The dualism of nature-society is rejected and replaced by networks which link together humans, organic (i.e. living) non-humans, technological objects and 'quasi-objects' or 'hybrid objects' that are combinations of technology and living organisms (Castree and MacMillan 2001; Latour 1993; Murdoch 1997; Whatmore 1999). Donna Haraway's notion of the cyborg (1991) is emblematic of such objects. The methodological task is to trace these networks in relationship to a phenomenon of interest, uncovering how different actants are recruited and play an active role in the extension of these networks. In this way, the dualism of nature-society is undermined and the ways in which nature and society are mutually constituted becomes more evident. Also, ANT does not take the relationships between various actants for granted, but rather examines how they are recruited and what influence they have on an ever changing network.

Also at the core of critical geography inspired by science studies is the project of deconstructing the process of scientific knowledge production. Central to this project is the insistence that environmental and ecological constructs are artefacts—representations of objects and not the objects themselves (Demeritt 1998; Demeritt 2001a; Haraway 1997; Latour 1987; Latour 1993). Thus, 'climate' is an artefact of atmospheric and hydrological processes that are represented by ecologists as a 'thing' about which we currently have much concern.

Most scholars working with science studies approaches do not reject the existence of physical processes, but rather insist that our understandings of those processes are representations and not 'reality'. This insight is crucial for validating other kinds of knowledges and environments that do not fit nicely into ecological understandings of nature. Nature can be represented in many different ways, some which are more meaningful or useful than others for particular people in particular moments in history. This is not to suggest that it's all relative, rather it is to highlight the importance of making moral and political judgements about which environments we seek to understand and protect (Haraway 1997). I believe it is in this sense that Castree and Braun conclude their chapter *The Construction of Nature and the Nature of Construction* by saying, "... we need to fashion new—or refashion old—analytical and political tools, tools for making the future natures that we wish to inhabit," (1998 p. 35). 'Science studies' scholars thus seek to understand how the artefacts of science are produced politically, historically and in relation to other human and non-human actants.

This strand of critical geography often does engage with ecological data such as remote sensed images, cartography, climate change models and geological data (Demeritt 2001a; Demeritt 2001b; Willems-Braun 1997). This engagement with

ecological data, however, is done to deconstruct it and reveal the politics behind its production. For example, despite Demeritt's insistence that he does not contest the validity of global climate change models, he nevertheless does not engage with the *results* of the models except to discuss the politics of scientific uncertainty (2001a). The deconstruction of ecological data has lent crucial insights into the workings of science and the need to treat ecological data as partial and situated rather than universal and disembodied (Haraway 1991; Haraway 1997; Latour 1987; Longino 1990). But it doesn't provide many insights into how else we can engage with socio-natural change. We need to use ecological data both because it is a valid way to understand nature (at least as valid as any other), but also because we don't have a lot of alternatives. The question is how do we incorporate ecological theory and data *into* our analyses?

Despite contributing important insights, I would argue that the turn towards science studies has not brought critical geography much closer to an engagement with the 'natural' (physical) consequences of socio-nature issues. Instead, if a (partial) reading of the literature and recent conference papers at the AAG in New York and Los Angeles and the IBG in Belfast are any indication, science studies has served to pull critical geography *away* from an engagement with these crucial questions about nature. I will try to support this claim based on what I see as five main theoretical and related methodological problems with ANT and related science studies approaches.

The failings of science studies for critical geography

1. The first critique I have of science studies approaches concerns the deconstruction of scientific practices. In *Science in Action* Bruno Latour (1987) purports to open the black box of science by following the way that scientific knowledge comes to be accepted as 'true' by a community of scientists and what actants play a part in that construction. This has helped to demonstrate how scientific knowledge can be socially constructed and yet actively shaped by non-human actants—ecological knowledge does not arise in an environmental vacuum.

Yet, I argue that Latour places 'Scientists' into a black box. He claims to follow Scientists at work in the laboratory, but he ascribes way too much agency and deliberate purpose to individual scientists (see also Schneider 2001 in reference to Demeritt 2001a). I base my comments on my experience of working as a technician in a biochemical laboratory. Most scientists 'at the bench' are painfully unaware of the larger context of their work, even within a strictly biological realm. Thus, molecular biologists do not necessarily have a very complex understanding of animal physiology even though they study, for example, membrane proteins of animals cells—much less the politics of membrane activity.³ In addition, most scientists are not in positions of power to influence the research agendas they pursue, the kind of work they do on a daily basis, or the dissemination of their results. The scientists who run large laboratories in some cases do fit well with the Scientist caricature of Latour's but the vast majority of bench scientists do not. Thus, science studies as it has been used by Latour and interpreted by critical geographers, does not help us to

³ Membrane proteins are responsible for transporting molecules across cell membranes. Thus research into them is significant for cancer research (can they target cancer cells to 'turn them off' or kill them without harming healthy cells?), drug therapies (which drugs will be incorporated into which kinds of cells?) and many other medical applications with tremendous socio-economic implications.

understand the complex negotiations and power relations within scientific daily practices and how that is shaped by and reflected within the ecological constructs that scientists study.

The compartmentalisation of scientific inquiry is as much (re)produced by the power relations within laboratories that keep bench scientists engaged in relatively narrow experimental realms and the funding opportunities that different scientists can access, as it is by the historical development of science and the physical apparatuses of laboratories. I therefore suggest it is more useful to return to Escobar's (1995) rendition of discourse and examine the way that discourses of science come to be dominant and to shape the practices of those operating within it, combined with a 'political ecology' perspective that seeks to understand the cultural politics of knowledge production and ownership of scientific knowledge. Perhaps simply extending the network inquiry a bit more deeply into the laboratory (or other scientific space) would suffice, but for me, this raises significant questions about how to prioritise the objects of inquiry in ANT. Is the goal thick description of society and nature (as it may be for Latour as an Anthropologist) or is the goal understanding socio-environmental processes (see also the critiques of ANT in Castree and MacMillan 2001; Murdoch 1997)?

2. My second critique is more directly related to the linking of society and nature in ANT. Scholars that are working within an ANT framework seem to focus on tracing the relevant networks instead of the consequences of those networks (Castree and MacMillan 2001 p. 222 make a similar point). Conference papers in particular reflect this theoretical and methodological commitment in that they present 'thick' descriptions of socio-natural networks that identify various organic, non-organic and quasi-objects that have all played a role in the development of a particular problem. I remember sitting half in shock at the conclusion of a paper in NYC that traced the development of copper mining in the southwest USA. The mining company—after using many other techniques and changes in management had occurred—used hydrochloric acid (HCL) to extract residual metal from the mining tailings, resulting in contamination of the ground water with HCL. That was the conclusion, how the network circled around between the human, economic, technical, organic, and chemical events. Echoing my thoughts, the discussant (Noel Castree) said he was left wondering 'so-what'. But that did not lead to a discussion about the ecological so-what. The *ground water* was polluted with one of the world's most corrosive acids rendering it toxic to all but specific bacterial species. Water moves and acids diffuse in water, thus the spatial extent of the HCL 'contamination' was large, albeit its concentration would be reduced with spreading (this was partially traced in the paper). While I don't have any data to support it, based on what I know of HCL I can postulate that the environmental consequences for human and non-human species are dire. Humans would have to find alternative sources of water (and could as they live in the USA) but non-human species may not recognise the danger until too late. I asked myself, how did tracing the network of this problem get us any closer to dealing with the socio-natural consequences of this kind of mining? How did it help us to build a politics that could prevent such practices in the future?

While I am certain the author of that paper built in more of the ‘so-what’ into publications based on that work,⁴ nevertheless, the first analysis was on the network and breaking down nature-society dualisms. In short then, tracing the networks and identifying quasi-objects and non-human actants in much of ANT work, although certainly not all (cf. Robbins 2001), becomes a substitute for engagement with the *consequences* of those networks and actants in/on nature (and sometimes even the political consequences). The theoretical and methodological focus turns to the networks and not the issues that drive an analysis of those networks in the first place.

3. My third critique is the one I feel is most important of all five and one I believe others have not addressed. The backbone of the networks in ANT is the actants which can be human, organic non-human, technological, or a combination, quasi-objects. Quasi-objects in particular seem to be emblematic of the breakdown between nature and society as they can be human produced, technological and organic (living) non-human. Latour argues quasi-objects have been kept ‘hidden’ and unacknowledged because they defied the nature-culture divide of modernist thought (Latour 1993). Making these objects visible is thus politically and theoretically critical to undermining that dualism. Genetically engineered species such as OncoMouse™ (Haraway 1997) are classic examples of these kinds of quasi-objects, but roasted coffee beans are another (Whatmore and Thorne 1997). It is no longer possible to classify them as either natural or technological, they are both. I find this insight to be extremely useful particularly in drawing attention to the way in which what counts as ‘natural’ and what does not is socially constructed and deeply problematic. ‘Nature’ as an unexamined and pre-given category, allows people to appeal to what is ‘natural’ thereby exempting their actions from political and moral scrutiny. As Haraway (1997) points out, such conceptions have underpinned ideas of racism for centuries and served to justify immoral treatment of those constituted as other. To use a different example, scientists create new species through genetic manipulations, but they are seen as ‘laboratory animals’ instead of part of the gene pool of those species in nature. This kind of separation obscures the difficult task at hand of making moral judgements about quasi-objects and recognising their political and environmental significance (Haraway 1997). Thus ANT has helped to disrupt the division between society and nature and has provided a different way to talk about the living and non-living objects of socio-natural issues without resorting to dualisms.

But the focus on objects within ANT is exactly what I find problematic about it. The actants of ANT are representations and temporary manifestations of socio-natural *processes* (Haraway 1997 p. 142-144). While actively acknowledging that actants change as they are recruited within and play a part within networks, they nevertheless remain *objects* within ANT (cf. Latour 1993 p. 95-109). To me, this serves only to further fetishize (following Haraway 1997 p. 142-144) quasi-objects and non-human actants. Some actants, such as soil degradation in a coffee-based network (Whatmore and Thorne 1997), are processes, not objects. Soil becomes an object within scientific inquiry when one talks of halting soil erosion like stopping traffic (when does soil movement really stop and what temporal scale is used to judge?). Yet it is a complex physical, geo-chemical process that is represented as a thing for analytical and linguistic simplicity. The representation of complex processes as objects is typical of

⁴ It was a fascinating case study and the material wonderfully rich which was part of the reason I was so disappointed by the conclusion. He’d done such a great job of addressing both the social and ecological aspects I was waiting for him to tell us why that mattered.

many linguistic traditions. And perhaps because of their taken-for-granted qualities, scientists and lay people alike often conflate the abstract conceptualisation and the process—leading to a variety of socio-natural consequences as Haraway (1997) discusses in relationship to genes.⁵ This is the mistake that ecologists make when they substitute the representations of their research subjects for things-in-themselves (Haraway 1997; Latour 1993).

Yet within ANT, geo-chemical weathering of soils remains an *object*, even if that object might change to ‘soil conservation’ (from soil erosion) as it shifts in relationship to other actants in the network. Thus, ANT examines the social-political processes of artefact construction instead of the ecological processes that produce those artefacts. This does not help me to engage with the mutual constitution of soil processes, agricultural practices, global capitalist production of coffee, and consumption of coffee from the perspective of constantly changing natural processes and consequences. ‘Soil conservation’ may signal an important shift in the discourse about the environment (and human land use practices), but not necessarily a shift in the underlying physical processes (soil weathering may not shift to what we consider ‘conservation’ despite changes in land use and rhetoric about it). Thus even with a focus on networks, an ANT analysis depends on objects and not on ecological processes, pulling us more into an object-based analysis rather than a process based analysis. ANT, therefore as it’s been used so far, does not adequately engage with the socio-natural processes that we are concerned about.

Perhaps the failing is methodological and critical geographers need to simply extend the networks of ANT further. We have neglected delving into the networks of soil ecology and rather chosen to stop the analysis at ‘soil’. This could perhaps be a useful analytical move, but again, would require engagement with ecological data (and other soil imaginaries as some political ecologists and cultural ecologists before them have done). Can ANT take on board ecological constructs and data? Will it still allow for a critique of the production of that data while actively incorporating it into the network? I can see some potential for that and a more detailed discussion about it would be useful. Robbins (2001) attempts such a synthesis in a recent article. His analysis, however, did not incorporate a ‘network’ within the environment, he used ecological methods to analyse the environment and then linked the results to his ANT-inspired analysis (see also below). This suggests that extending the networks further into nature may be difficult. My next two points raise other questions about the adequacy of extending networks. While such a move might be useful, is it *enough* or desirable theoretically and methodologically?

4. My forth point is the most difficult to articulate. To me, social and ecological processes are qualitatively different. While I definitely argue for the ‘agency’ of non-human actants (to use the language of ANT), and agree that humans and non-humans are mutually constituted, the agency of non-human actants is *different* from human agency.⁶ In part this is because of human consciousness and our apparently unique

⁵ Haraway talks about the way that the fetishization of genes has allowed for them to be thought of as things that can be privatised, stored in genetic banks and encoded. ‘Genes’ represent a dynamic biochemical process and are not in fact, things-in-themselves. It is only by constructing them as things-in-themselves that genes can be owned, stored, etc.

⁶ After writing this section I was surprised to find a somewhat similar critique in Castree and MacMillan (2001 p. 221). Here they draw from Lauier and Philo (1999) to ask whether the

ability to plan and modify our surroundings at scales and rates not done by other living species. The premeditated and analytical aspects of this kind of agency are critical to my argument. Non-human actants, while actively engaged in shaping their surroundings, do not engage in such large scale and rapid rate modifications and don't analyse *how and what* changes they will implement.⁷

Beavers are a classic example. They create dramatic phase changes to their surroundings—beaver dams create wetlands or small lakes on rivers that then drown out their food species of aspen trees and woody shrubs. They tend to eat themselves out of their homes and have to move onto another place. The place they leave behind is often changed 'permanently' (over a long time scale) into a habitat for different plant and animal species than the one the beaver encountered originally. But this kind of modification is not analysed by the beaver. Experiments have shown that beavers build dams when they hear water running. Beavers living in lakes (that don't build dams) will build a dam in response to a tape played of running water. While humans sometimes engage in similarly thoughtless ecological modifications, we nevertheless have the abilities to analyse and decide whether or not to repeat those modifications.

If the agency of humans and non-humans are different, then the processes by which they mutually constitute each other are also somewhat different (although with some shared processes). Is it adequate then to place humans and non-humans into networks 'side-by-side' giving them the same analytical treatment as ANT attempts to do (see for example Watts and Goodman 1997; Whatmore and Thorne 1997; Whatmore and Thorne 1998)? This is related to the issue of how to incorporate ecological data into an ANT analysis. If we draw on theories of power, subjectivity and political economy to understand the social processes of a network, is it a simple matter to incorporate ecological processes to understand the physical aspects? How do we link those processes back together? What are the consequences for analytical understanding of socio-nature issues and building a useful politics?

This is where ANT fails me. ANT doesn't help me to link the agential differences between human and non-human actants that are so crucial to building moral and political arguments about the need for humans to fundamentally change the way they act in the world.⁸ If non-human actants build networks in the same way as humans,

indifference or flattening of difference between actants within ANT is problematic. What I am suggesting here is both that indifference is a problem, and I'm extending that to argue that some 'noun chunks' *and verb* chunks are different and that this difference seriously challenges the utility of ANT as an explanatory paradigm for socio-nature.

⁷ I personally suspect that many marine mammals likely have the same cognitive abilities as humans, but they don't seem to engage in modifications of their surroundings at anything like the scale and rate that humans do.

⁸ Here my critique is aimed more directly at geographers' interpretations of ANT than Latour's formulation mainly because within Latour's work I think his purpose for defining actants is different (Latour 1993). In *We Have Never Been Modern*, one of his central arguments is making a case for the importance of anthropological inquiry 'at home'. In addition, I think he is trying to suggest new ways of thinking about science and science studies. Theoretically, he is interested in showing how different networks have greater or lesser dominance than others (i.e. which discourse is dominant), rather than engaging with the *implications* of socio-natural change. These objectives are different than the applications to which many critical geographers have used ANT. Geographical applications have largely focused around explaining particular human-environment interactions and current environmental controversies.

why do humans need to modify their behaviour or take responsibility for ecological change?

5. To extend this argument in a slightly different direction, my fifth critique is not so much aimed at the turn towards science studies as it is aimed at ‘social nature’ geography more generally. There is often a lack of adequate recognition in nature-society geography that nature is not totally under our control nor completely understood by us. Here I want to pick on the quote I used earlier from Braun and Castree (1998). While I do think they intended that comment in the spirit in which I quoted it earlier, I found it a disturbing sentence. To suggest that we need to “[make] the future natures that we wish to inhabit” (1998 p. 35) is to suggest that we can determine that. Ecology, natural resource management, and other physical sciences have long since demonstrated that humans are nowhere near understanding the complexity of physical processes adequately to create environments (much less nature). We may decide what we want, but it is far more likely that we will end up with something different materially and symbolically *because* of the nature-society nexus. Nature-society geographers to date have provided us with innumerable examples of such outcomes. But Braun and Castree’s appeal signalled to me that they have not taken seriously the conclusions of their own work and that of ecologists. I found it especially ironic that Castree again basically repeats that phrase in their new book immediately following a section about the importance of recognising that humans cannot completely manipulate the physical environment (Castree 2001 p. 19). We cannot make the kind of nature(s) we wish to inhabit, which is for me the most fundamental message that comes from both ecology and critical geography. And that is why we have serious cause for concern over the increasing ways in which the environment is socialised (cf. Katz 1998; Smith 1998).

Instead of making the nature(s) we want to inhabit, critical geographers need to promote the message that we *cannot* make ‘nature’ and for that very reason the kind of novel, rapid rate and simultaneously larger and smaller scale ecological changes that are occurring as a result of human activities are cause for great alarm. We have very little idea what the long-term consequences might be and at what rate they will occur but we have some idea that we could be unpleasantly surprised (see for example hypotheses on global climate change and the probability of a sudden, rapid rate change in climate rather than a gradual warming (Demeritt 2001a)). Critical geography has shown how these kinds of rapid changes can have significant implications for society and particularly for social justice—also in ways that are unpredictable, but tend to affect marginalised people the most. Global climate change scientists, despite their own evidence to the contrary, continue to make the political mistake of suggesting we can predict climate change.⁹ We can predict a range of scenarios and that has been useful, but we do not and cannot *know* what will happen. This makes the need for critical engagement with ecological theory and data and better conceptualisations of the relationships between social and ecological change that much more important. I certainly do not want to leave the task of evaluating changes in nature entirely in the hands of ecologists for all the reasons that science studies scholars and critical geographers have so saliently pointed out.

⁹ This tendency is related, of course, to the roots of many physical sciences in positivist principles.

Where do we go from here?

In the sections above I have tried to trace some of the key contributions but also some of the inadequacies of current work in critical nature-society geography. I want to make it clear that critical nature-society geography has contributed a number of extremely important insights. My purpose in this paper is not to reject those contributions but to push current work to incorporate more understanding of physical processes and to ask some important questions about how to accomplish such integration. While there have been calls for more engagement with the material consequences of human-environment issues (Castree 2001; Castree and Braun 1998; Peet and Watts 1996; Zimmerer 1994), the analyses and theoretical positions taken by critical nature-society geographers have in my opinion taken us further *away* from such engagement. In response, I have tried to highlight the importance of engaging with data and theories from the ecological sciences to grapple with the consequences of environmental change. I argued that such engagement is critical for two main reasons. First because it is clear that any kind of social-political action involves action by/in nature as well (the message from science studies), and second, because ecology is a very important source of knowledge about nature, even if it is a partial and situated source. The main question remaining then is how do we integrate ecological analyses with the very sophisticated social theoretical approaches we use?

This question has both theoretical and methodological consequences. Theoretically we need to recognise the dynamic, contingent relationships between society, environment and hybrids (i.e. quasi-objects or quasi-processes). ANT and ‘post-structural dialectics’¹⁰—or multi-lectics as I prefer to think about the mutual constitution of various aspects of socio-nature—are moving in promising directions. Nevertheless, these conceptual frameworks cannot import ecological theories wholesale. In part the problem lies with ecological theory itself because of its roots in systems theory which leaves little room for understanding the articulations between society and nature outside of feedback mechanisms (cf. Botkin 1990; O’Neill 2001). More significantly, it is extremely difficult to find an *analytical* entry into processes that are contingent and continuous. In other words, Latour (and Haraway) has conceptualised quasi-objects as a way to theorise the continuum between society and nature, but as I pointed out earlier, this brings us back to an object-based analysis. How do we think about ‘quasi-processes’ that at various temporal-spatial points might be more ‘natural’ or more ‘social’?

For example, how do we think about the socio-nature of water irrigation when the ‘water’ in question begins as ‘pure’ but of course has various ‘natural’ elements in solution and suspension (i.e. minerals, ‘dirt’), yet, by the time it reaches the end of the irrigation pipe it has dissolved human-produced chemicals and organic wastes such that the concentration of H₂O is so minimal that it is questionable whether it can be called ‘water’?¹¹ At what point did it stop being water? How do various concentrations of H₂O articulate with social and power relations over water? Is there some threshold point when the concentration of H₂O becomes so low that there are

¹⁰ I am trying, perhaps unsuccessfully, to think of a succinct way to talk about the hybrid post-structural-historical materialism approaches that are represented in *Liberation Ecologies* and related work.

¹¹ This example comes from a talk given by Erik Swyngedouw at the University of Minnesota. He did not engage with the chemical properties of water in his talk, but indicated that water quality declined as it moved through the system. Some of the questions I raise here I asked him at the time.

‘suddenly’ different socio-political struggles, new people involved and productive relationships surrounding that liquid, or do these relationships also change on a continuum with decreasing concentrations of H₂O? What about from the perspective of non-humans, at what point does the ‘quasi-water’ become more harmful to them than nourishing? These kinds of questions raise serious issues about the categories (actants) we chose within our analyses, how we work with the need to talk about relatively stable manifestations of processes as things, and yet retain an analytical stance that recognises them as processes.¹²

I thus find ANT to be more useful as a methodological technique than as a conceptual framework. ANT’s object-based analysis doesn’t help to conceptualise actants that are in a state of constant change. Methodologically, however, it points to the need to examine a wide range of relationships that are connected to the problem at hand. It avoids defining which relationships are important ahead of time—although I would also argue that we continue to be influenced by our ‘little theories’ of what we think is important (Burawoy 1991). Nevertheless, when used in isolation, ANT doesn’t provide much insight into how to bound an investigation. How do we determine which relationships are more important than others?

I thus find ‘post-structural dialectics’ to be more useful than ANT theoretically. By conceptualising mutually constitutive, dynamic relationships between *analytical* domains, one can conceive of relatively stable objects and relationships and yet investigate how they are embedded within each other, contingent and changing (see also Harvey 1996 although he does not engage with physical nature in the ways I’m suggesting). At the same time, those analytical domains can be challenged and recognised as artificial constructs themselves. ANT in some ways also offers such analytical possibilities, but dialectics begins with a set of relationships that are assumed to be important and seeks to analyse *how* they are important and what other relationships might also need to be included. In other words, the subjects of analysis are theoretically defined rather than ‘discovered’ through the research process. I certainly do not find this to be a ‘solution’ to the theoretical issues I raised above, however. I would be interested in having a more detailed discussion about what challenges ecological theory raises for social theory and how to arrive at a better conceptualisation of these processes.

Having said that, I am also wary of assuming that social theory is enough. I think we need to work more closely with ecologists and physical geographers to develop better theoretical and methodological tools to examine socio-natural issues. Not all scientific inquiry is working in a positivist, reductionist paradigm. We need better ecological theories that represent ecological change in such a way that it takes seriously the social aspects of that change and moves away from systems theory. How can we link more effectively with ecologists and physical geographers to push both social and ecological theory? As part of this we need to recognise that

¹² Perhaps it is less a need a more of an historical convention in linguistics. But I think there is also a material influence on this convention. While the moor outside my window is constantly changing, it also remains a moor over decades and looks more or less the same. Similarly, the paint on the outside of my house is constantly dissolving, but it nevertheless continues to be paint for many years (and then it becomes ‘peeling paint’). In other words, change is temporally and spatially-scale dependent, and humans often perceive things as stable that are not on other temporal-spatial scales (or to other organisms).

ecological change is equally as complex as social change but in some ways qualitatively different. What are dynamics of ‘quasi-objects’ and processes if we accept this difference? For me this is still an open question that I do not think Latour or Haraway have addressed adequately.

It also requires that we take seriously the concerns expressed by Stephen Schneider (2001) in his response to David Demeritt (2001a). Schneider’s critique of Demeritt’s article on the construction of global climate change was in part based on what he sees as an unnecessarily philosophical account, and the impenetrable language. He further challenged Demeritt on his interpretation of debates within the climate change community about the kinds of models that are most appropriate and the uncertainties inherent in them. While I found Demeritt’s article to be far more helpful than problematic, I also remember how I felt my first few years in graduate school as I encountered these debates (and thus words) for the first time. I hated all the words I had to look up in the dictionary again and again because they held no meaning for me. It is possible to talk about nature-society issues in a straightforward way and in the interests of interdisciplinary collaboration we need to work on retaining analytical complexity and promoting linguistic simplicity.¹³ We also need to take seriously Schneider’s critique of Demeritt’s claims about the way that global climate change scientists themselves represent their work. While I agree with Demeritt that in popular publications ‘scientists’ tend to over-simplify the conclusions of their work (with serious political implications that we should challenge them on), we also need to engage with them as colleagues, taking seriously the implications of the debates that occur within their own journals.

Methodologically there are also a number of challenges. Most fundamentally, if ecological data is incorporated into a socio-environmental analysis, what data is relevant and how do the epistemological origins of that data matter? Studies that have tried to incorporate ecological data have largely used it ‘side-by-side’ with social data (cf. Robbins 2001). In Robbins excellent paper in the *Annals*, the section talking about the ecological dynamics of ‘quasi-forests’ in India is inserted into the social-political narrative. He outlines the ecological significance in a separate section then links these results back into the narrative. But how can we analyse that data in *relationship* to the social data—as mutually constitutive methodologically as well as theoretically?

I cannot claim to have over come these issues, but one technique I have experimented with is using ecological and social data more explicitly together. Whenever possible, I collect both ecological and social data on the same questions. In my case, I was examining forest change in Nepal. I worked with social and ecological data sets separately, and used the appropriate tests of ‘validity’ or ‘rigor’ that are conventional within the relevant disciplines for each data set. In the analysis, however, I treated them as equivalent in terms of what they might tell me about forest change. The most successful attempt I had with this was using oral histories and aerial photo interpretation of forest change. I did not assume the aerial photos were ‘right’, rather I looked for the ways that the aerial photos and the oral histories gave me different narratives about forest change. They did correspond to some extent, but they also were quite different. If I had only looked at the aerial photos I would have concluded

¹³ Something which I fear I have not modelled in this paper!

that forest cover has changed very slightly (less than 5% total change in cover in twenty years). The oral histories gave me a different scenario, however. The villagers insisted that forest cover had improved significantly. Rather than thinking the villagers were deluded, I examined the discrepancies between the data sets. This helped me to understand what aspects of the forest the villagers valued the most and to give more importance to small areas on the aerial photos that had improved. The result was that my interpretations of both data sets were far richer. It would be interesting to discuss whether others have tried similar techniques and in what contexts this might or might not work. It seems to me, however, that to examine socio-natural issues our methodologies need to also reflect the complexity and diversity of the issues at stake.

I hope that this paper has raised a number of challenging questions for critical geographers and that we can have a lively discussion about them in Hungary.

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