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Landscape research in a world of domesticated landscapes: the role of values, theory, and concepts

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ABSTRACT

This review takes as its starting point the relationship between landscape history and environmental policy. Landscape historians now face the same problem that social scientists have long faced, i.e., how to relate to values and to the political use of scientific results, which demands greater conceptual and theoretical rigour from integrative landscape studies. The concept of social–ecological systems is criticised for its reduction of the complexity and human agency involved in land use; in contrast, Clark Erickson’s concept of *domesticated landscapes* offers an approach that can incorporate humanist as well as scientific considerations. The roles of surplus production and labour allocation in early societies are seen as crucial for understanding early domesticated landscapes. Different social formations result in different landscapes, but landscapes also have an earthly inertia. Investments in land govern, steer, impede, or inspire land use in subsequent social formations. This specific understanding of *time* and place is shared by quaternary geology, landscape archaeology, and historical geography, and distinguishes them from both history and physics.

Keywords: relevance, domesticated landscapes, landesque capital, engineered landscapes, labour, time

1. Introduction

Landscape history today plays a central role in relation to several different discourses at the interface between environmental policy and research. At the European level, the broad process initiated by the Council of Europe in the European Landscape Convention could move the landscape issue from the hands of experts and researchers to a broader arena, in which landscape is increasingly part of European policy and politics. At the global level, the environmental change debate, focusing on the climate, has highlighted the role of humans in the long-term change of the earth’s system to such an extent that it has been proposed that we now live in the anthropocene era. The question of whether the anthropocene started in the industrial revolution or at the dawn of agriculture has not been answered. The fact that the

question has even been asked, however, paves the way for the more intense involvement in broader global change studies of landscape archaeologists and other researchers focussed on long-term studies of human influence on the landscape.

This is a new development for many landscape researchers. Many of us started our studies at a time when landscape was a much smaller and narrower academic concern, and our main contact with the public was with local history enthusiasts. Any applied aspects of our research were mainly confined to cultural heritage planning. The new situation poses new challenges to landscape researchers. The positive aspect, of course, is that there is greater interest from society and more research money – at least if we can frame our projects so that they address wider issues of current political, social, and environmental concern. In these new fields of applied research in landscape and environmental history, politicized as they are, we can also see political agendas trying to make historical arguments serve certain purposes. This has been clearly illustrated by the archaeologist Daryl Stump (2010) in quite a different context. He demonstrates how arguments about the landscape history of terraced agriculture in East Africa have been used and misused in current debates on rural development. Landscape researchers, whether active in quaternary geology, archaeology, geography, or history, are now increasingly facing the challenge social scientists have faced since the birth of their disciplines: how to relate to values, relevance, and the political use of their results.

In this paper, I address this new situation in our field, and the demands this places on us. I then raise some conceptual and theoretical issues that need to be developed in response to these new challenges.

2. Landscape history and relevance

The two policy fields relevant to landscape research that I have mentioned above, namely, the European Landscape Convention and global environmental change, are not that closely connected. Different landscape researchers are exposed in different degrees to these two policy areas and may be involved in one or the other of the associated networks. Moreover, the Landscape Convention has so far been mainly a European concern, although its perspective and the agenda following its implementation could easily be seen in a global context. Both these fields are closely related to policy concerns. They have facilitated the direct involvement of landscape researchers in activities clearly positioned in relation to policies, politics, and action. Likewise, many discussions of future research agendas take

their starting point in the relevance aspect and the utility of the proposed research for national, European, or global policies (see, e.g., the recently launched science policy briefing, ESF–COST, 2011). In this section, I address this increased role of landscape researchers in relation to normative agendas, i.e., how things ought to be. I argue that the increased demand to produce “relevant” research, i.e., research that can be applied, makes the choice of conceptual framework and theoretical approach more crucial in our research.

2.1 The European Landscape Convention

Through its definition of a landscape as “an area, as perceived by people, whose character is the result of action and interaction of natural and/or human factors”, the European Landscape Convention (ELC) clearly brings citizens and their valuations of landscapes (as opposed to expert opinion) into the implementation of the Convention. As a Convention rather than a directive, and formulated by the Council of Europe rather than by the European Union, the Landscape Convention builds its power on democratic arguments rather than on state- or EU-led policies. The question of public participation has been central to the underlying reasoning of the Convention and to discussion of its implementation (Jones, 2007). The European Landscape Convention poses difficult and potentially controversial political questions at two levels. Intuitively, the question of public participation has often been thought of as the participation of “local” people – of farmers and rural citizens with a clear sense of belonging to the landscape and of identification with the land. However, the fact that the ELC is a convention permits a much broader, and politically much more challenging, view of who has the right to participate. The ELC regards the landscape as a common good and sets no boundaries between locals and outsiders. As Jones (2007) has explained, this expanded view of participation implies that locals, visitors, and immigrants should all be entitled to have their say about the landscape. Jones also raises the issue of illegal immigrants:

But what about other categories of immigrants, such as guest workers, temporary migrants, asylum seekers and refugees, who have restricted rights and entitlements? What about illegal immigrants (working often as labourers in construction or agriculture, as hotel workers or as domestic help) who are without formal rights? A radical, but undoubtedly controversial, interpretation of the Convention would mean that such groups should not be dealt with summarily but should be heard in matters concerning the physical environment in which they find themselves. (Jones, 2007, p. 622)

Formulated in this way, the ELC places the landscape at the centre of some difficult and highly politicized debates in European politics. For the landscape researcher, there is no longer any escape from politics and power.

At a European level, many arguments for preserving agricultural landscapes are heavily laden with explicit or implicit assumptions on the historical relationship between a group of people and their landscape. When Marc Antrop (2005) claimed that the landscapes of the past are important for the future, he characterized landscapes of the past in the following words:

For many centuries the changes were local and gradual and seldom were existing landscape structures wiped away completely. In the past, landscapes were experienced as rather stable and having a distinct character or identity. They formed a basis for the homeland of those who created it during centuries of work. (Antrop, 2005)

Not only is the reference to “homeland” dubious, considering Europe’s turbulent history, but Antrop’s argument implicitly neglects one of the most important paradigm shifts in the historiography of European landscape studies. When German landscape history re-emerged after the second world war, one of the great achievements of researchers such as Anneliese Krenzlin and Wilhelm Müller-Wille was to demonstrate that the European landscape, even before the agrarian revolution, was anything but stable or traditional. Instead, it was the result of a series of radical transformations rooted in the development of the social organization, changing power relationships, and new farming technology. The so-called “traditional landscapes” of Europe are often the result of capitalistic development in the late 19th and early 20th centuries and increased regional specialization. As Hans Renes has put it:

In old history writing and in modern planning, there is much nostalgic thinking about local people being heavily connected with their surrounding landscapes and therefore being the best suited for shaping the future of their landscapes. This idea of a long-standing local population as partner is, at least partly, romantic wishful thinking. ... People living in a landscape they shaped themselves, are the exception, not the rule. (Renes, 2011, pp. 127–128)

In a Europe where mobility is increasing, both within Europe and externally, the landscape historian plays a key role in refuting ahistorical assumptions regarding locality and belonging as well as myths of a stable and harmonious rural past.

2.2. *Global environmental change*

The second field I want to address is the broad and rapidly growing body of research into global environmental change. Since the climate scientist Walter Ruddiman launched the thesis that the greenhouse gas era began thousands of years ago with the beginnings of agriculture, the human aspects of global change from a long-term perspective have become a field of great relevance to climate modelling (Ruddiman, 2003). This field of interaction between climate science, archaeology, and palaeoecology is developing rapidly, demanding more knowledge of the global distribution and intensity of agriculture and other human influences over the last ten thousand years or more. Faced by the lack of palaeoecological, archaeological, and historical data at a global level, various datasets based on simplistic backcasting have been produced by climate modellers. Ramankutty and Foley (1999) have estimated global land cover changes over the 1700–1992 period. A reconstruction of the last millennium along the same lines has been published by Julia Pongratz et al. (2008). Goldewijk et al.'s work using the HYDE database has recently been extended back to 12,000 years BP (Goldewijk et al., 2010 and references therein). These reconstructions differ slightly in their methods but, when it comes to reconstructions of croplands, they can be broadly categorized as back projections from the 20th century using historical population estimates. This method tends to overemphasize European and colonial agriculture and largely reflects Eurocentric assumptions rather than historical knowledge. For Africa and the Americas, they grossly underestimate human influence on the landscape. This does not stop climate modellers from using the data.

Historical geographers and archaeologists have been slow in responding to this challenge. It is mainly in the field of palaeoecology that work towards a broader synthesis based on empirical data, rather than modelling only, has started (Gaillard et al., 2010). Palaeoecologists and geographers have also followed up the Ruddiman thesis with detailed investigations of the connections between the Little Ice Age and the demographic collapse in South America following the Columbian encounter (Dull et al., 2010). A small international project has also been initiated to answer some of these questions from the viewpoint of historical geography and economic history through global reconstructions of agricultural systems for AD 1000, AD 1500, and AD 1800 (Widgren, 2010). To sum up so far, it can be said that the demand from climate researchers for empirical data at the regional and global scales is not matched by the work of landscape historians. We live in a time when there is

great need for global syntheses and comparisons, but not many landscape researchers seem willing to take up this challenge. This is partly because of a lack of data for large parts of the world, but also partly because landscape researchers focus more on local levels and other thematic issues.

2.3. *Relevance, theory, and values*

The researchers to whom I referred in the previous section clearly address issues that are highly relevant to understanding current global change and the impact of human decisions on land use. They choose their research topics in relation to current policy concerns, but do not take a normative approach. Another approach, taken by a group of historically oriented global change researchers gathered in the Integrated History and Future of the People on Earth (IHOPE) initiative, is to take an open activist position. In an editorial in the journal *Global Environmental Change*, Sara Cornell et al. (2010) argue for the role of historical approaches in global change studies, advocating a systematic “science of the past”. They also argue for a new role for scholars in relation to the questions of relevance: “We are deliberately pursuing knowledge for action. This is a major extension of the normal role of academia, a shift in the usually accepted social contract of scholars” (Cornell et al., 2010). A similar approach to the relationship between relevance and research was taken earlier in the plea for *sustainability science* as a normative science. Kates et al. (2001) argued that “science must be connected to the political agenda for sustainable development” (p. 642).

In light of the enormous environmental challenges facing the world today, it might seem uncontroversial to call for environmental consciousness and relevance-oriented research among a group dominated by natural scientists. It should be seen as an attack on a widespread culture in natural science, a culture that claims that all science should and can be value free. What is striking in this context is the almost total lack of debate on values and on the ethical, epistemological, and ontological issues that such a normative and activist standpoint entails. For social science, in contrast, debate on values, objectivity, and action is almost as old as social science itself.

Despite much talk of interdisciplinarity, the boundaries between the humanities and social sciences, on the one hand, and the natural sciences, on the other, seems less porous than ever when it comes to such issues. Many of the concerned sustainability scientists and earth systems scientists seem, in their friendly interdisciplinary invitations and their

appreciation of the social sciences, ignorant of or immune to the critical and reflexive aspects of the social sciences and humanities.

The debate on values in social science is ongoing. There is a plethora of standpoints on objectivism and values and how they relate to choice of problem areas, theories, and methods. It is probably fair to say that most social scientists acknowledge that theories and methods cannot be seen in isolation from values, and that our research design choices can serve various interests. Few social science researchers would, for example, claim that advancing a social science vision for saving the world would be simple and uncontroversial.

Immanuel Wallerstein, who can be classified as an activist, addressed these questions in a paper presented to an interdisciplinary forum of global change scholars. He clarified the issue by highlighting “three different ‘mental operations’ in which scholars/scientists necessarily engage when dealing with any topic” (Wallerstein, 2007, p. 379). The *intellectual task* of understanding a phenomenon and its development is central to all research. The concerned scholar, according to Wallerstein, also has the *necessary task of moral evaluation*. Should this moral evaluation be seen as outside the defined role of the scholar? Wallerstein argues that the segregation of moral and intellectual analysis is impossible. Finally, there is the *political question*: How can we move forward to achieve the moral objectives?

While it is impossible to segregate the intellectual, moral, and political tasks, they are not identical. At any given moment, we need to know which of these three mental operations we are engaged in, otherwise we will make mistakes. According to Wallerstein (2007), “the rich literature about global environmental change moves uneasily and a bit fuzzily among these three mental operations without always formulating clearly the distinctions” (p. 380).

It is argued in some contexts that the urgency of environmental problems, or any other pressing political issue, creates a need to put theoretical and methodological disagreements to the side (the basis of Wallerstein’s intellectual operation) and focus on the knowledge needed for the solution (the political task). The counter-argument, however, is stronger. Applied research needs to be more rigorous and more precise about theories and methods than does research written solely for internal disciplinary use, since the former will later be used to guide actions and decisions in real life.

In the case of the topic addressed here, this should mean that, as landscape research is facing a situation in which its results are in greater demand, it needs to move away from the localist and descriptive tradition that has hitherto been largely dominant. The alternative is not a normative research agenda in which intellectual, moral, and political issues are blurred, but sharp, theoretically informed research into relevant issues. Theoretically incisive,

curiosity-driven research is not in opposition to relevant landscape research. Relevant research, on the other hand, does not have to lead to diluted scholarly and theoretical ambitions; on the contrary, it will demand more of theories and methods. Landscape history, with its combination of humanistic and scientific perspectives, is in an ideal position to take on such research.

3. Humans and the rest of the natural world

Central to any understanding of the relationships between humans and their environment is how we conceptualize the role of humans in relation to the rest of the natural world. This conceptualization is at the heart of all efforts to understand this relationship from a long-term perspective, especially if we demand theoretical and methodological clarity. In research into global environmental change, it is possible to discern three distinct approaches to that problem. A simplistic understanding of past environments focuses on the *scars* that humans and civilisations have inflicted on nature. A seemingly more integrative perspective focuses on *social–ecological systems*, but fails to break from its mechanistic understanding of humans and societies. I argue that the concept of *domesticated landscapes* offers a much more creative and incisive starting point for theoretically understanding the role of humans and their environments from a long-term perspective.

3.1. First perspective: “scars in Nature”

Simplistic understandings of the negative role of humans, civilisations, and world systems on their environments still play an important role in popular science and survive in part of the academic literature on environmental history. The sociologist Sing Chew has written widely on the historical relationship between world systems and the environment, starting from the assumption that almost all human influence on the natural system is exploitative and ultimately leads to environmental degradation (Chew, 2001, 2007). Chew’s schematic understanding of such relationships is underlined by his use of the concepts of “Nature” and “Culture” as fixed entities, which represents a gross simplification of the complex and dialectic relationships between human use and changes in ecosystems. According to Chew (2007), human influence on the landscape should be considered “scars in Nature” (for a critique, see Widgren, 2007a). This approach is oblivious to the rich evidence

from the past that humans have built sustainable production systems and even increased biodiversity through their management. I would also argue that the study of the relationships between large imperial world systems and their environments needs to be approached from an empirical, not a normative, standpoint. Large empires exploited both human labour and natural resources. In some areas, this led to environmental degradation, while in others it led to investment in soil and water conservation. Where and when the one or the other of these two tendencies gained the upper hand are empirical questions that remain to be answered. More importantly, though, the conceptual division between Nature and Culture is, as much recent research has demonstrated, an impossible avenue by which to approach empirical research into humans and their environment, and is hence a blunt tool for relevant landscape history.

3.2. *Second perspective: social–ecological systems*

The fact that humans have, for a longer or shorter period of history, played a central role not only in ecosystems, but also in the whole geobiosphere, has increasingly led researchers in systems ecology and earth systems science to consider aspects of human and social systems. As a result, the concept of *social–ecological systems* has been proposed as an integrative approach; the concept is central to work based on the *resilience approach* (Chapin et al., 2009). This approach suffers partly from the same type of categorical distinction between the natural and social worlds (i.e., Nature versus Culture) as described above. The proponents of such an approach usually do not address the issue of where the line should be drawn between the two subsystems, the ecological and the social. Are anthropogenic soils and irrigation systems “soil resources” that belong to “ecological properties”, or are they parts of “infrastructure” that belong to “social properties” (referring to Fig. 1.3 in Chapin et al., 2009)?

A second, and perhaps more difficult, problem with this approach concerns whether it is even possible to reduce the social subsystem to the same logic and same scale as the ecological subsystem. Most social scientists would argue that social systems differ fundamentally from ecological systems. Social systems include both human intentions and power relationships. The concept of social–ecological systems still struggles to escape from its background in natural science – a reductionist systemic view – which tends to subordinate the humanities and social sciences to an epistemology and ontology based on the natural sciences. Such approaches display, in the words of Hornborg (2009), a severe lack of

understanding of the “strong research traditions in the social sciences that have persuasively shown that socio–ecological systems are historically and currently characterised by structural problems of power, conflicts of interest, and unequal distribution” (p. 252).

Thomas Kirchhoff et al. (2010) have conducted an epistemological enquiry into the historical roots of this version of social–ecological systems. They argue that what is proposed as a scientific conceptualization of ecosystems, and then projected onto social, economic, and coupled ecological systems, is already from its ecological beginnings characterized by a cultural bias, an idea of society projected onto nature. According to Kirchhoff et al. (2010), the resilience approach to social–ecological systems “does not apply a value-free natural scientific concept to society ... but reapplies a particular cultural idea, which has been transformed into an only seemingly natural principle” (p. 31).

The resilience approach and much of the research into social–ecological systems is also characterized by a strongly normative research agenda. It moves, in Wallerstein’s words, “uneasily and a bit fuzzily” between the analytical, moral, and political. The resilience literature does not usually discuss value issues or conflicting interests, but seems to assume that “sustaining desirable systems properties” for social–ecological systems can be handled in a valueless vacuum.

The theoretical shortcomings of integrating social and ecological systems in such a model are clearly illustrated in the IHOPE initiative’s efforts to take an integrated approach to global environmental history. The agenda is commendable: “Understanding the history of how humans have interacted with the rest of nature can help clarify the options for managing our increasingly interconnected global system” (Costanza et al., 2007). In this programmatic paper, however, the integrative approaches are not convincing. While data on global temperatures and greenhouse gas emissions can be plotted with detailed accuracy in a diagram covering the last 100,000 years, the human aspects of the same development are reduced to either simplified backcasting of population figures or marginal notes. Costanza et al. (2007) emphasize that their main diagram must not be understood as visualizing causation between the indicators of environmental and human history, but the choice of variables in the diagram mean that it can hardly be seen in any other way. Efforts to establish such an integrated human/environment history of the earth are seriously hampered by the data imbalances between the sciences and the humanities from such a long-term perspective. This difference in the availability of quantitative data at a global level will always privilege the natural sciences in such a synthesis.

3.3. *Third perspective: a world of domesticated landscapes*

A recent and thought-provoking contribution to the mapping of global environments is the launching of the concept of anthropogenic *biomes* by Ellis and Ramankutty (2008). These authors argue against the established view of the world's environments as consisting of a series of naturally and climatically determined *biomes*. They argue that “human-dominated ecosystems now cover more of the Earth's land surface than do ‘wild’ ecosystems”. They conclude that “nature” is now embedded within human systems, and that it is no longer possible to conserve nature by simply avoiding human interactions. The authors accompany their argument with an interesting map based on a categorization of areas of the world into various *anthromes* or *human biomes*.

If we add an historical dimension to the work of Ellis and Ramankutty (2008), we must ask *when* nature became embedded in human systems. Seen from that perspective, the concept of anthropogenic biomes, and the global mapping of them by these authors, seemed to offer an interesting option for the long-term understanding of human influence on global vegetation. The question of the age of the anthropogenic biomes is left open in Ellis and Ramankutty's 2008 publication; with a team of co-authors, they have more recently answered that question in a way that unfortunately displays a complete neglect of the historical evidence of global agriculture and human influence on landscapes (Ellis et al., 2010).

If we instead turn to recent research into historical ecology and plant domestication, we find results that challenge us to fundamentally re-conceptualize the age of this embeddedness of “nature” in human systems. Based on archaeobotanical research, Purugganan and Fuller (2009) have recently argued that domesticated plant species did not originate in single events but in extended multistage processes in which traits arose sequentially over several thousand years (p. 845).

Purugganan and Fuller state that their results, which are based mainly on archaeology, stand in contrast to molecular evolutionary findings regarding domestication that assume “rapid, single origins”. They argue that domestication must be seen as only one aspect of plant–animal co-evolution, occurring in much the same way as natural selection. It is merely in type and intensity that the human–plant relationship, resulting in domestication, differs from natural selection and from other plant–animal co-evolution processes, for example, ants' and beetles' “domestication” of certain fungal species. In mapping the geographical areas of this human–plant co-evolution, they also identify more than twenty areas of domestication on all continents except Australia. All these have contributed crops of global significance today,

such as the sunflower of native North Americans, the potato of South America, sorghum of Africa, and the banana of New Guinea – to mention just a few. It has long been known that the old image of a unicentric origin of agriculture in the Middle East is no longer valid. Knowledge of the many different centres of domestication has gradually increased, and now we can almost no longer even talk about a polycentric pattern, but rather a dispersed pattern of domestication. Domestication occurred independently in different places and regions and at different times, extending from 13,000 years before the present for wheat and barley in the Middle East to perhaps 3000 years BP (or even later) for African rice.

Early plant domestication is thus seen as a gradual process based on a series of different management practices. For the Americas, this has been described by Clark Erickson as involving “planting, transplanting, tending (‘husbandry’ or ‘mothering’), cultivation, weeding, transport outside natural habitats, and the use of fire as a management to enhance survival of economic species” (Erickson, 2006a, p. 241). Erickson also comments on how Amazonian peoples, as they move in the landscape, are “constantly gardening the forest, weeding and pruning here and there” (op. cit.).

All these activities play a key role in the domestication of plants; moreover, as Erickson argues, it is also possible to broaden the concept and think of those activities as the human domestication not only of single plants, but of the whole landscape. The domestication of landscapes thus encompasses “all nongenetic, intentional and unintentional practices and activities of humans that transform local and regional environments into productive, physically patterned, cultural landscapes for humans and other species” (Erickson, 2006a, p. 241). Accordingly, irrigation systems, anthropogenic soil, managed forests, etc., are all seen as parts of the domesticated landscape, and Erickson hypothesizes that Amazonian peoples invested more energy in “domesticating landscapes as a whole than in domesticating individual species of plants and animals” (Erickson, 2006a, p. 236).

Through the lens of these recent works, one can now see the nature–culture dichotomy in a new light: We clearly must take account of several thousand years of human–environment interaction that have led to the development, not only of a few domesticated plants and animals, but also of anthropogenic biomes or domesticated landscapes. More and more evidence is accumulating that indicates that what was previously seen as wilderness may in fact represent the results of various types of human–environmental interaction over millennia. The simple cropland–wilderness dichotomy used in some modelling poorly represents the world’s past landscapes. The domestication of the landscape and of plants and animals clearly represent two alternative ways of producing food beyond the subsistence

level. This understanding also has implications not only for how we see the nature/culture dichotomy, but also – and perhaps even more challengingly – for how we see the forager/farmer dichotomy that has played such an important role in our understanding of the development of prehistoric natural resource management.

The understanding that we live in a world that has for several millennia been one of domesticated landscapes is fundamentally opposed to many previous assumptions regarding humanity as “conquering Nature”. Moreover, Erickson’s perspective on domesticated landscapes also implies that landscapes and vegetation are incorporated into social systems. Erickson also criticizes approaches based on the co-evolution of social and ecological systems, claiming that they “often depict humans as being swept up in a long-term process that unconsciously modifies the environment” (Erickson, 2006a, p. 244). Instead, he emphasizes the intentionality of humans in transforming and domesticating their environment. In my view, the perspective implied in the domesticated landscape concept can also bridge the gap between perceived and physical landscapes – a dichotomy that has sometimes created unproductive divisions in landscape studies. I believe that this dualism in the landscape concept should instead be seen as the basis of a creative, dialectic analysis.

4. Labour and landscape

If we agree that the world we live in is – and has been for millennia – a world of domesticated landscapes, we have to go beyond the obvious and start looking at the processes by which the landscape is domesticated, worked, and reworked. This understanding puts the emphasis on human labour.

4.1. Scenery, domestication, and invisible labour

It is only through labour, for example, pruning trees, tending wild plants, and weeding, that plants and landscapes can be domesticated, and it is only through organizing labour that irrigation systems, terracing, and other “heavier” investments in the land can be made. This fact is so obvious that it is often overlooked. The question of labour is a central focus when it comes to reaching a more thorough understanding of how humans have changed the landscape over time.

Such a materialist view of landscapes may seem in sharp contrast to the cultural geography approach of the 1980s, which focussed on the visual aspect and on landscapes as ways of seeing, best represented by the works of Denis Cosgrove. His work, and that of other cultural geographers, also exerted a strong influence on one strand of landscape archaeology dominant in post-processual archaeology discussions in Britain. However, it must be recalled that Cosgrove's approach to landscape as the visible aspect was also based on an analysis of the processes that made work in the landscape invisible. He wrote about the "enormous expenditure of surplus labour" (Cosgrove, 1998, p. 99) to create gardens and landscapes and hence the visual aspects of the landscape to be consumed by wealthy people. The focus on the often invisible labour underlying the scenic was further developed by Don Mitchell in a series of works. Mitchell emphasizes that understanding landscapes "requires an examination of human practices – of forms of labor" (Mitchell, 2003, p. 235).

4.2. *Surplus versus subsistence*

When Cosgrove and Mitchell raise the issue of invisible labour, it is of course in social and chronological contexts that differ substantially from those with which many landscape archaeologists are working, and from those of pre-Columbian Amazonia, in which Clark Erickson developed his ideas of domesticated landscapes. The comparisons do not, however, have to be so extreme. Clark Erickson argued that the transformation of land into domesticated landscapes was driven by "social demands far beyond the subsistence level" (Erickson, 2006a, p. 236). A basic tenet in trying to understand the allocation of labour in almost all known social formations is that they all produce a surplus (see a short summary of the argument in Håkansson and Widgren, 2007, p. 235). The issue of the relationship between labour and social formations cannot be confined to hierarchical societies. As long as there is the need for a surplus, the question of labour allocation to produce that surplus will be crucial in understanding how different social formations influence or create distinctive landscapes.

It is only by better understanding the differences between social formations that we can understand the circumstances under which human labour is allocated to the land in such a way that it degrades future productive capacity or enhances biodiversity. Today, most historical ecologists would argue that neither of the two simplistic approaches to past human–nature relationships are valid: neither the myth of the noble savage or the ecological peasant who, based on the insider's ecological knowledge, was always a good resource manager, nor the idea of human influence as always causing degradation are now accepted. History is full

of examples of both sustainable and unsustainable land use. While local knowledge is certainly part of the picture, we need to know much more about the role of the social formations underlying the allocation of labour in particular landscapes and their role in the degradation versus the enhancement of future production potential.

4.3. *Concepts of the worked landscape*

Many concepts are used in the literature to capture how human labour has changed the landscape, often to improve the future production capacity of the land. These concepts overlap to a certain extent while focussing on different aspects. The concept of *domesticated landscapes* broadens the picture, urging us to look beyond the most obvious signs of human influence, and is inclusive of the role of foragers in changing the land. The concept of *engineered landscapes*, on the other hand, as used by Lansing (1991) and Earle and Doyel (2008), focuses on a specific type of domesticated landscape, with facilities such as terraces, canals, dams, and reservoirs for agriculture, as well as walls, roads, buildings, monuments, villages, and cities (cf. Earle and Doyel, 2008, p. 21). It is almost synonymous with *the built environment*, which David Harvey (1982) sees as a “humanly created resource system, comprising use values embedded in the physical landscape” (p. 233). An important distinction here is that Harvey is not focussing solely on investments directly related to production, but on the whole infrastructure related to transport, storage, trade, and the reproduction of the labour force. If we narrow the discussion to the productivity of the land, the concept of *landesque capital* is more specific. This term came into wide use in studies of land degradation versus soil and water conservation in the 1980s, and refers to “any investment in land with an anticipated life well beyond that of the present crop, or crop cycle” (Blaikie and Brookfield, 1987, p. 9; for discussions of the concept from a long-term perspective, see Fisher and Feinman, 2005; Widgren, 2007a).

These concepts all share the understanding that investing human labour in the land can improve its productive capacity for a period beyond the immediate future. They also stand in marked contrast to the ideas about natural carrying capacity that have flourished in much research (cf. Widgren, 2007a, p. 63). They are therefore crucial to any historical approach to sustainability and for the more nuanced analysis of the role of humans in the rest of the natural world.

The question of labour in relation to domesticated landscapes has so far been treated more thoroughly mainly in relation to terraced and irrigated landscapes. The forms of

organization and the control of labour are questions that are central, for example, to the whole debate on irrigation and Oriental despotism raised by Wittfogel's works (cf. refs in Earle, 1997). A crucial understanding in this context is how different social formations – not only hierarchical as Wittfogel assumed – have been able to mobilize the labour needed for large-scale irrigation and terracing (Widgren, 2007a, p. 68; Earle and Doyel, 2008; Watson, 2009). Analyses of the relationships between social formations and such landscapes have come far, through careful and empirical analyses of farming practices and worked agricultural landscapes, on one hand, and of political economy and social stratification, on the other. Such an approach needs to be expanded to cover all sorts of domesticated landscapes as well.

5. Time

If it is possible to argue that there is indeed a close relationship between social formations and the labour that transforms the landscape, is it possible to understand landscapes as determined by their contemporaneous social formations? Can we expect transformations of landscapes to have resulted from a series of transformations of social relationships? Following the above argument, it would be easy to say that an understanding of social, political, and economic factors is essential to understanding how landscapes change and how changing labour relationships are expressed on the ground.

The main problem with such an approach is that it usually underestimates the chronology embodied in landscapes. At one level of generalization, a certain type of agrarian landscape can be understood as reflecting, or being part of, a specific period of political and economic development. However, closer scrutiny of a particular ancient landscape usually leads to a much more complicated chronology. It is no surprise that a field-oriented landscape archaeologist such as Clark Erickson is sceptical of simplistic connections between political economy and landscape. Erickson's argument for a bottom-up, people-centric approach to landscape is at the same time an argument for a certain set of methods and approaches to landscape archaeology. He demonstrates how a desk-bound archaeology focussing on settlement patterns fails to understand the landscape. His text is a clear, eloquent, and fervent appeal from a landscape archaeologist working in the field to uncover the problematic chronologies of the intentional and unintentional signatures of people working the land (Erickson, 2006b). I argue that anyone who has worked in the field to solve the chronological and functional puzzles posed by past farming landscapes should realise that practising

empirical landscape history indeed leads to a different understanding of time from that of a desk-bound historian or settlement pattern archaeologist.

All past investments in the land are incorporated into subsequent landscapes, worked, reworked, abandoned, or destroyed. Although the chronology may at first seem uncomplicated, most field-oriented landscape researchers will finally become aware of the long time span involved. Not only do landscape investments continue to play a role, and be partially reworked, in subsequent historical phases under different social formations; almost all investigations would also identify older, antecedent phases before the main period of use. This holds true for clearance cairn fields in Scandinavia as well as for pre-colonial irrigation systems in Africa. Rather than being attributable to only one specific type of political economy, such investments tend to survive in different social and political contexts. I have previously argued that landscape capital, being incorporated into the land, is much more obviously spatial than chronological in character, being fixed in space but “fluid” in time (Widgren, 2007a, p. 72).

In studies of the built environment, Dodgshon has discussed this in terms of inertia, explaining how such investments, once formed, will be carried forward in subsequent economic phases. The emphasis on chronology in history and archaeology has often misled landscape historians to reconstruct cross-sections of landscapes typical of a certain phase in political and economic history, as if these landscapes were not heavily framed by their past, but only by their present context. This relates to what Dodgshon calls the *synchronic illusion*, that we tend to “experience the past as if it were synchronic when in fact, it is richly diachronic” (Dodgshon, 1998, p. 166). The specific relationship between place and time is a phenomenon that human geography and geomorphology share, distinguishing these two sub-disciplines of geography from both history and physics, as once explored by Doreen Massey (1999). I think that her arguments can easily be broadened to include both the more scientifically based landscape archaeology, leaning towards geomorphology, and the more humanistic strand of landscape archaeology.

6. Concluding comments

As I have argued here, long-term landscape history is in a favourable position today. The challenges posed by global change studies and by increased policy-related interest in landscapes, especially in Europe, must be taken seriously by researchers in archaeology, quaternary geology, geography, and other disciplines focussing on long-term landscape

history. The strengths of these fields lie in their rich empirical tradition and rigorous methods. Meeting the intellectual demands emerging from global change and landscape policies calls for conceptual and theoretical development, and more regional and global syntheses.

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